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the 1990s, the incidence of *S. flexneri* has increased in the United Kingdom [10]. In the United States, *S. flexneri* has been reported to be the most common serotype of *Shigella* isolated from children with shigellosis [11]. In the United Kingdom, *S. flexneri* has been reported to be the most common serotype of *Shigella* isolated from children with shigellosis [12].

The purpose of this study was to determine the prevalence of *S. flexneri* in the faecal flora of children with shigellosis in the United Kingdom. The study was conducted in a tertiary care hospital, which is a referral centre for children with shigellosis. The study was conducted in the United Kingdom, where the incidence of shigellosis is high.

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8

HARVARD PSYCHOLOGICAL STUDIES

EDITED BY

HUGO MÜNSTERBERG

VOLUME IV



CAMBRIDGE
HARVARD UNIVERSITY PRESS
1915

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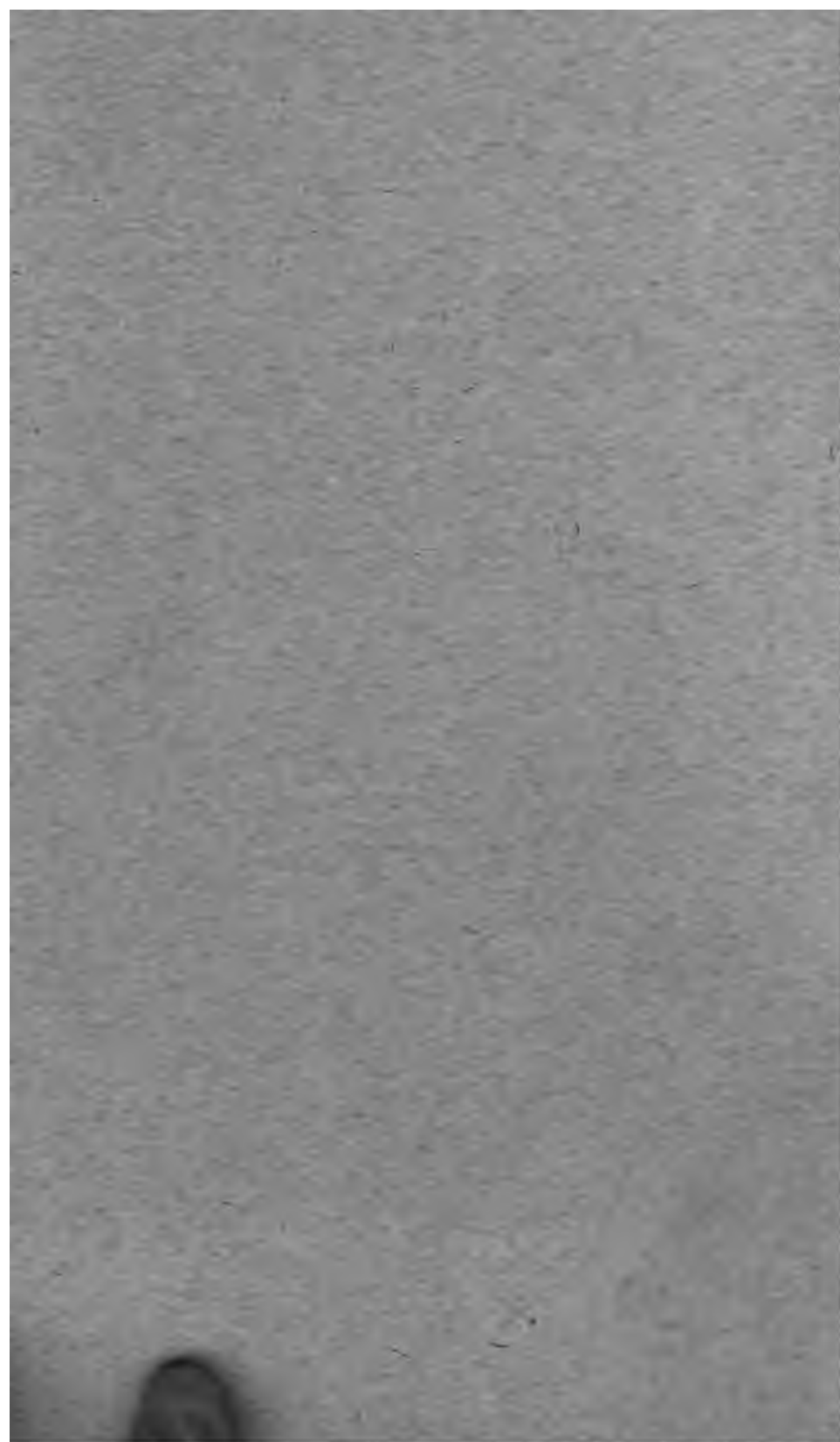


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VOLUNTARY MOVEMENT UNDER POSITIVE AND NEGATIVE INSTRUCTION

BY HERBERT SIDNEY LANGFELD

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In two previous articles¹ upon the negative instruction the main interest was directed toward the effect of such instruction upon the suppression of imagery and the muscles of the vocal organs. The work described in the present article was undertaken in order to obtain an analysis of the process of executing a skillful movement, that is, a movement requiring perfect motor control, under the conditions of positive and negative instruction.

The experiments were conducted in the Harvard laboratory in the spring of 1912 upon five subjects referred to as *A*, *B*, *C*, *D*, and *E*. They were all graduate students, with a training in experimental methods.

The instrument used was a tracing board similar to the one recommended by Whipple.² The subject passed the wire attached to the stylus through his coat sleeve, so that it would move freely with his arm and not interfere with its movements. He sat before a table of convenient height with the board directly in front of him.

The positive instruction given him was: "Go down the middle of the groove." That is, he was to pass the stylus down the middle of a groove between two brass sides 25 cm. long, and at the nearest end $\frac{1}{2}$ cm. and at the farthest end $\frac{1}{10}$ cm. apart. When the stylus touched the sides a telegraph sounder indicated the contact. The precautions advised by Whipple were observed.³ Preliminary trials were made until the subject could traverse the distance in nine

¹ 'Suppression with Negative Instruction,' *Psychological Bulletin*, June, 1910, Vol. VII., pp. 200-208; and 'Suppression with Negative Instruction,' *PSYCHOLOGICAL REVIEW*, November, 1911, Vol. XVIII., No. 6, pp. 411-424.

² 'Manual of Mental and Physical Tests,' p. 120.

³ *Idem*, p. 121.

seconds. Ten trials were then made with the right hand, with five seconds pause between each trial. The instruction was repeated after every second trial. The subject then gave his introspection, which was recorded by the experimenter. It was deemed advisable to take the introspection at the end of the series instead of after each trial in order that the subject should not be disturbed in his attitude by too long a pause. Also any effect of practice or warming up would not be lost and altogether the voluntary movement would be a more natural one. In regard to this latter point suppose the introspection were taken after every trial. And let us also suppose there is a content in the margin of attention, which the subject attempts to describe. He finds difficulty in presenting it clearly. In the next trial his full attention is given to it and the movement is no longer normal. Although this is a danger to be avoided in all introspection, it is more difficult to do so when the introspection is frequently given. The obvious disadvantage of such more or less sporadic report, *i. e.*, that some of the data of the first trial will be lost, seems to be of relatively minor importance. And, it must be remembered, it is only the report, not the introspection which is sporadic, for the latter is spread more or less equally over the entire series.¹ At first in order to avoid suggestion the subject was merely asked to describe fully his conscious state. It was only after it was evident that certain contents were present that questions were introduced to obtain a full description of these contents. After the report of the introspection was finished and a pause of five minutes allowed for rest, ten trials were made in similar manner with the left hand, and again the introspection was noted. There was in general an intermission of one week between each series. In several instances two weeks inter-

¹ G. E. Müller in his 'Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes,' *Zsch. f. Psychol.*, Ergbd. 5, Leipzig, 1911, p. 135, says: "Stellt man eine Reihe von 240 Versuchen an, so wird man bei Untersuchung mancher Fragen den gleichen Einblick in das innere Verhalten der Versuchsperson gewinnen, wenn man dieselbe nur in 30 bis 40 über die Versuchsreihe verteilten Fällen zu einem Vorgangsbericht veranlässt, wie dann, wenn man das Verfahren mit jedesmal verlangtem Vorgangsberichte anwendet, ohne dabei eine gleiche Ablenkung der Vorgänge von der Natürlichkeit zu riskieren wie bei dem letzteren Verfahren."

vened. After five series had been made under the positive instruction, in the case of two subjects after four series, the negative instruction "Do as before but this time direct your attention to inhibiting the stylus from going toward the sides in going down the groove," shortened to "Do not touch the sides," was introduced and the trials conducted in all other respects as before. The two forms of instruction, positive and negative, do not necessarily imply the same action. While under the positive a straight line must be made, under the negative the stylus may zigzag so long as it goes down the groove without touching the sides. Practically, however, in consideration of the narrowness of the groove, this difference may be neglected. In both cases the attention was directed toward steadiness of movement. Only in one instance was advantage taken of the extra privilege of the negative instruction. A subject said after one negative series that by saying to himself that he could move his hand laterally so long as he did not touch the sides, he felt less constrained.

QUANTITATIVE ANALYSIS

One of the striking features of the results is their variability for the same individual from week to week as well as

SUBJECT A
Positive Instruction

Series	Right Hand			Left Hand		
	M.	Md.	M.V. ¹	M.	Md.	M.V.
1	6.8	7.0	2.7	7.8	7.6	2.4
2	9.8	10.4	2.4	4.9	4.3	2.6
3	5.7	5.9	1.4	3.8	3.6	1.4
4	6.1	6.5	2.0	5.3	5.0	1.8
av.	7.1	7.4	2.1	5.4	5.1	2.1

Negative Instruction

5	4.7	3.8	2.7	7.1	8.8	3.0
6	7.4	8.1	3.9	9.5	8.4	4.3
7	6.9	6.9	2.0	5.6	5.5	2.4
8	7.0	6.1	1.9	6.8	7.2	3.0
9	10.0	10.3	2.6	13.4	10.5	4.4
av.	7.2	7.0	2.6	8.5	8.1	3.4

¹ The m.v. is taken from the mean, the median is quoted throughout the paper and not the mean.

SUBJECT B

Positive Instruction

Series	Right Hand			Left Hand		
	M.	Md.	M.V.	M.	Md.	M.V.
1	8.2	7.7	4.1	8.7	8.5	1.9
2	5.4	4.5	2.2	7.4	6.5	3.1
3	8.5	8.2	3.8	6.1	6.6	2.4
4	4.9	5.8	3.9	7.6	5.9	4.4
5	7.9	7.4	3.3	8.6	7.5	3.2
av.	7.0	6.7	3.5	7.7	7.0	3.0

Negative Instruction

6	5.6	4.1	3.4	3.8	2.4	2.2
7	8.6	9.2	1.4	10.6	9.1	3.5
8	9.3	9.3	1.7	9.2	9.1	1.8
9	10.6	9.2	3.9	8.3	7.7	4.2
10	8.9	9.2	.9	12.8	12.5	4.1
11	10.2	9.5	1.6	9.4	8.6	3.2
12	8.2	9.0	1.4	7.5	8.0	3.3
av.	8.8	8.5	2.0	8.8	8.2	3.2

SUBJECT C

Positive Instruction

Series	Right Hand			Left Hand		
	M.	Md.	M.V.	M.	Md.	M.V.
1	5.0	5.6	2.2	7.8	7.7	2.3
2	5.7	5.7	1.2	13.4	13.9	4.2
3	7.7	7.1	2.1	11.4	11.9	4.2
4	8.4	8.8	2.7	11.2	10.0	3.6
5	9.5	8.2	4.9	11.0	10.3	2.8
av.	7.3	7.4	2.6	11.0	10.8	3.4

Negative Instruction

6	5.9	4.6	2.9	9.9	10.2	2.7
7	9.8	10.1	2.9	9.8	8.7	4.5
8	11.5	10.9	2.3	9.9	9.7	3.0
9	8.6	9.0	3.4	14.2	15.5	4.0
10	8.2	9.0	2.7	10.0	9.3	2.0
11	8.6	9.5	1.5	10.4	10.5	1.6
12	8.6	9.5	2.5	9.8	9.5	3.1
av.	8.7	8.9	2.6	10.6	10.5	3.0

from trial to trial. For example *B* does 4.5 cm. one followed by 8.2 cm. the next and 5.8 cm. the next. *D* 14.9 cm. one day, 8 cm. the next and 10.6 cm. the *A* does 3.8 cm. followed by 8.1 cm. and 6.9 cm. and The m.v.'s are in many instances large and the de

SUBJECT *D*
Positive Instruction

Series	Right Hand			Left Hand		
	M.	Md.	M.V.	M.	Md.	M.V.
1	13.5	14.9	6.1	14.0	15.4	4.7
2	10.1	8.0	3.9	15.7	16.4	2.8
3	11.7	10.6	4.0	11.0	10.3	4.5
4	9.6	9.0	3.3	10.8	10.3	3.5
av.	11.2	10.6	4.3	12.9	13.1	3.9

Negative Instruction

5	12.7	11.0	3.8	16.5	15.5	2.4
6	10.2	10.0	2.0	10.3	10.3	2.6
7	11.5	10.6	3.2	15.0	14.9	2.1
8	9.7	9.2	3.1	11.2	9.1	2.9
9	7.2	6.6	2.2	13.0	12.5	2.6
av.	10.3	9.5	2.9	13.2	12.5	2.5

SUBJECT *E*
Positive Instruction

Series	Right Hand			Left Hand		
	M.	Md.	M.V.	M.	Md.	M.V.
1	5.4	6.5	2.3	13.9	12.7	3.2
2	10.2	10.8	1.7	10.4	10.0	4.4
3	8.4	7.3	2.9	9.5	8.1	4.1
4	5.0	5.0	2.1	10.9	11.2	3.5
5	11.6	10.6	3.0	9.6	10.7	3.2
av.	8.1	8.0	2.4	10.9	10.5	3.7

Negative Instruction

6	12.9	12.7	3.4	13.6	13.9	3.0
7	9.4	9.2	2.3	9.7	9.7	3.1
8	8.1	8.5	2.5	14.3	14.9	3.8
9	8.9	9.5	2.4	8.5	9.0	2.1
10	10.8	9.5	4.1	11.1	9.2	4.1
11	10.0	10.3	2.6	13.4	10.6	4.4
12	10.5	9.5	3.9	10.2	9.1	3.6
av.	10.1	9.9	3.0	11.8	10.9	3.4

correlation between low averages and small m.v.'s is slight. This large variability is not confined to either hand, but generally a large m.v. for the right hand will be followed by a large m.v. for the left on the same day and the same with the small m.v.'s. It may be significant that in the case of *D*, who is the only one who does better with both hands under the negative instruction, the m.v. is also on an average

decidedly smaller for that instruction. This holds in the case of *A*'s left hand under positive and *E*'s right hand under positive instruction.

There are considerable individual differences in the effect of the different instructions. As mentioned, *D* is the only subject to show improvement both hands under the negative instruction. *C* shows a difference for the left hand and a worse average for the right. *A* is about the same for the right and almost poor for the left. *B* and *E* have a worse average for both hands under this instruction. Nowhere can one find the results of practice. Under the presupposition that movement is more difficult under the negative instruction it might be said that, inasmuch as the negative instruction is positive, those who did worse in the negative instruction showed the effects of practice if the instruction was changed and those who showed some improvement have shown a still greater one.

The first average in several instances under the other instruction was the best or at least very ample in the case of *C* for both hands under the positive instruction, the right under negative, *E* for the right hand under negative, *B* for both hands under the negative and *A* for the left hand under negative instruction. When only one hand showed this peculiarity it was the right, that is, the first hand which suggests that the low average was due to lack of interest in the movement.

QUALITATIVE ANALYSIS

The introspection on the fore-period of movement will be given and analyzed and then that of the movement. Only introspection bearing on the subject of this movement, quoted and repetitions will, as a rule, be omitted.

FORE-PERIOD

Subject A

Positive Instruction.—(1) R. h.¹ I tried to imagine there was a groove. I got a visual image of this groove. (2) L. h. There is a visual image of this groove.

¹ R. h. = right hand. L. h. = left hand. The introspection is put in order to facilitate reference in the following analysis. Remarks are put in parenthesis.

the center that I am to follow. (3) L. h. The idea of going down the middle is represented by the path a ball takes that I imagine I am to bat and because I am able to associate this imagery with the left hand I am able to keep the idea of the sides out of consciousness easier and longer than with the right hand. (He always batted left handed, although he was right-handed in other things. The record with the left hand is better than that with the right.)

Negative Instruction.—(4) R. h. The idea of keeping away from the sides was no represented by visual imagery but rather by my attention wandering from side to side of the groove. I could not think of both sides at once. (5) L. h. Auditory image of instruction was present. (6) R. h. There was a visual image of the point wavering from side to side. The sides were not prominent and faded out when I began to move down the groove. (7) L. h. The instructions were entirely auditory.

Subject B

Positive Instruction.—(8) R. h. I had an auditory image 'Down the middle. (9) L. h. I kept the instructions pretty well in mind and seemed to succeed in keeping other things out. (10) R. h. I shut everything out of consciousness but the auditory image of the instruction, which remained in the margin of consciousness. (11) R. h. I banished everything from my mind, not even retaining the instructions. I felt I could do the test. (Not as good a record.) (12) L. h. I did not start until I thought everything was out of my mind.

Negative Instruction.—(13) R. h. Auditory kinesthetic imagery of instruction I banished all else out of consciousness. (14) L. h. It is more difficult to keep other things out with the left hand than with the right. I had to keep repeating the instruction. (15) R. h. I tried to banish everything. I did not bring up the instruction. (16) L. h. Every time I started I recalled the instruction as auditory image. There was an effort to keep the instruction in mind. (17) R. h. I tried to get my mind centered both on going down the groove and not touching the sides. (No further description was obtained.) (18) L. h. The muscular tension is not as great as in the right hand. (19) R. h. I tried to obtain an auditory image of the instruction and to banish all else

Subject C

Positive Instruction.—(20) R. h. I had a visual image of a long line, also of the Grande Cañon. (21) L. h. There was a visual image of a continuous line.

Negative Instruction. (22) R. h. There was an image of some long passageway. The situation was exceedingly exciting and consequently pleasant. There was some great danger connected with this imagery. There was also auditory imagery of "Keep away from the sides. Go as near as you want so long as you do not touch the sides." This seemed a better instruction, as it seemed to give me more privilege. (23) L. h. There was visual imagery as before, also a vacant state of mind when I could not keep the concept. Finally I got the idea of keeping away from two rocks, from good and evil, etc. I kept switching from idea to idea. (24) R. h. I had a visual image of a tube like the Cambridge subway. The curve in the tube represented the negative not to touch the sides. At times it was represented by the idea of not touching the lamps. (25) L. h. There was a visual image of being between two rows of soldiers who held their bayonets toward me. "Do not look to the right or left or the bayonet will stick you" was represented in auditory imagery. (26) R. h. There was sharp pain in my ear and tension in my head. The instruction was represented by the idea that my head must be kept away from something. (27) L. h. I had a visual image

of a narrow bridge. The railings were freshly painted. There was also the auditory image "Keep away from the sides" and kinesthetic sensations in the knees. (The fore-period continued to the end of the series to be occupied by a concept of some sort of danger to be avoided, although the imagery representing this idea changed from day to day.)

Subject D

Positive Instruction. (28) R. h. There was a kinesthetic sensation of a line down the center. (29) L. h. I had a visual image of a line down the middle. (30) R. h. I had the kinesthetic sensations of going down the middle. The situation was that of the difficult task of walking down the middle of a narrow plank. There was also a visual image of a central path. (31) R. h. There was a kinesthetic sensation of trying to go down a straight line. (32) R. h. I had a visual image of a straight line.

Negative Instruction.—(33) R. h. I had a visual image of walking along a plank and not going off the sides. (34) R. h. There was auditory imagery of the instruction and visual imagery of the sides and the point wobbling between them. (35) R. h. There was a visual image of a ditch and my attempt to walk down without touching the sides. There was tension accompanied by sensations of wobbling. (36) L. h. There were auditory repetitions of the instructions and vague visual imagery of the point wobbling, also tension. (37) R. h. I had kinesthetic sensations of balancing. (38) R. h. The negative part of the instruction is represented by tension of the muscles and a dizzy feeling.

Subject E

Positive Instruction.—(39) L. h. I had an auditory image of the instruction. (40) R. h. I did not say the instructions to myself until I started. (41) L. h. I said the instructions to myself. There was also a visual image of a line down the center. I did worse the times I did not say the instructions before starting. I also said "I am going to keep down the middle this time," accompanied by an effort of bracing. (8.1 cm. His best record.) (42) L. h. The instruction was not so vividly in mind. (11.2 cm.)

Negative Instruction.—(43) R. and l. h. Instructions were present as auditory imagery. (44) R. h. There was a visual image of a line, but not so far down as with the positive instruction. (45) There was the auditory imagery of the instruction and the visual imagery was not so prominent. The attention was divided between the two sides. (46) When I paid most attention to the instruction I was less conscious of the tension in the arms and vice versa.

As might be expected there was a change of imagery with a change in the instruction. A visual image of a straight line was very common under the positive instruction. In the case of one of the subjects, if this imagery did appear under the negative instruction it was in some way changed. As for example in one instance it did not extend so far, that is, it was partially inhibited (45). We also find instances under the negative instruction of the sides of the groove becoming more prominent in the visual imagery or in the perception and of the attention wandering from one side to the other instead of

remaining in the middle (35, 45, 4). Subject *C* introduced an emotional element, that of fear, under negative instruction. His visual imagery took the form of some danger to be avoided, *i. e.*, a bend in the subway, bayonets, etc.; finally the instruction centered in a rigidity of the head, which was not to be moved to the right or left. Consequently we shall see in the main period, that at times when the point touched, this emotional complex was aroused (24, 25, 26; see also 90, 96).

Subject *B*, who was the only one who had no visual imagery, occupied the fore-period in repeating the instruction verbally as much as was necessary in order to keep everything else out of consciousness (9, 10), or else tried to inhibit all thought without the aid of the instruction (16). This subject made the second best record. When, however, he worked under the negative instruction, he found it difficult to keep the instruction. Evidently the attitude suggested by the instruction conflicted with his accustomed attitude. At one point we notice he makes a compromise, in that he attempts to divide his attention between going down a straight line and keeping the idea of not touching in mind (17). His records, both with the right and the left hand, are worse under the negative instruction, although the first records made under these conditions were the best he ever made. This seems to suggest that the negative instruction increased his interest in the work at first although on the whole it hindered him.

The breaking up of an advantageous attitude used under the positive instruction by the negative instruction is clearly seen in the case of *A*. This subject does much better with the left hand under the positive instruction than with the right, although he does not usually prefer his left hand. He does, however, when using a base-ball bat, and he is therefore able when using the left hand to get very strong visual imagery and we may say a correspondingly strong muscular set by visualizing the situation on the ball field and especially the straight line taken by the pitched ball (3). The strength of this imagery also aids him in keeping the idea of the sides

out of consciousness. He is unable to place himself in a similar attitude with the right hand and hence the above-mentioned poor result. And further this imagery cannot bear for him the meaning of the negative instruction. He, therefore, avoids all visual imagery for the left hand under this instruction, merely repeating the negative instruction verbally. His records show that he is able to go only half the distance he went under the positive instruction. If we examine the introspection of the main period about to be quoted, we shall find a possible explanation for the results with the right hand. We shall see that even under the positive instruction with the right hand a more or less negative attitude was assumed. There was not so much an idea of going down the middle as of avoiding the sides. The attention was on the control of the hand to prevent it wavering. It is also difficult for him to keep the idea of the sides out of consciousness. In other words the inhibitory process was uppermost. Under the negative instruction there can, therefore, be little change of attitude. The numerical results seem to bear out this introspection, for the very slight indication of improvement in the right hand under the negative instruction can very well be due merely to practice.

MAIN PERIOD

Subject A

Positive.—(47) R. h. When I ran off to the side I had a visual image of a branch of the line which seemed to pull the point over. (48) L. h. I was not as concentrated upon the groove, because more of my attention was given to the control of my arm in order to keep my hand from going back and forth. I lose the visual image of the groove when my hand goes off to the side. (49) R. h. When the point goes to the edge I seem more conscious of the brass edge than of the central line. I try to avoid the edge. (This is a negative attitude. No further analysis was obtainable.) (50) L. h. As soon as I go off the line there is an effort to bring the hand back. The idea of drawing back the hand is somehow present. (Negative attitude again.) (51) R. h. When I get half way down I do not as a rule seem able to keep the idea of the sides out of mind. This I succeeded in doing when I made the best record. (52) L. h. I can keep in mind the path I am to follow so much easier with the left hand. (53) R. h. As I deviate from the line the idea of going down the middle leaves my mind and the sides take a prominent place in consciousness. (54) R. h. As my hand begins to waver the instructions are lost. As long as I run steadily the instructions seem well in mind. The wavering at the upper end does not seem to cause the instruction to vanish as often as the wavering at the more critical points. (55) L. h. I was not conscious of the sides. The instruction was well in mind all the way (record 0 cm.).

Negative.—(56) R. h. As the point approached one of the sides the instruction came up as an auditory image and I tried to go toward the other side. (57) L. h. The wavering of attention from one side to the other confused me to-day. (57*b*) R. h. The instruction faded out only to come up again when I approached one side or the other. Then there came the desire to push away from the side and with it a visual image of a wavy motion. (58) L. h. The same as before. Something seemed to make me go the other way when I approached the side. I believe it was an auditory image of the instruction, although it might have been a kinesthetic one. (59) R. h. It seemed as if nothing were in mind until I reached the critical point, then the recall of the instruction inhibited the movement (toward the side). (60) L. h. I had a visual image of a wavy line in the lower part of the path. When I neared the end there was a tendency to stop to get my bearings. I noticed visceral sensations when I approached the sides. They seemed to bring up the instructions. (61) R. h. The checking of the movement and the coming up of the instruction seemed to occur at the same time. There were kinesthetic sensations in the tongue. (62) R. h. I had a tickling sensation in the arm as I neared one side or the other. (63) L. h. I was less conscious of the instruction. It was more that I neglected it than that it was difficult to keep it in mind. When I approached the sides it came up again.

Subject B

Positive Instruction.—(64) R. h. There was only present the auditory image of the instruction. (65) L. h. The same as above except that when my hand began to waver my attention was directed to it. (66) R. h. I tried to banish everything else out of consciousness. For example in one instance I said to myself "No mice!" (67) L. h. My mind this morning was more on the movement. The instruction seemed to blend with the movement. (His best record with the left hand.) (68) R. h. When something came to my mind I did not make as great an effort to banish it. (Record 7.4 cm. Not as good as last time.) (69) L. h. I started my hand and let it go. The instruction was not in mind. (Slightly poorer record than the previous one.)

Negative Instruction.—(70) R. h. I watched the needle carefully and if it wavered I brought it back. (71) L. h. Even though I did not repeat the instruction it must have been in some way represented, for if the point went toward the side I immediately tried to bring it back. (72) R. h. When I made a bad record I repeated the instruction. My hand was always in a tense condition. When I went to the right or left, there was a decided effort to bring the hand back. (73) L. h. I experimented in keeping the instruction in mind and seeing if the hand would follow it of its own accord (record 9.1 cm.). It seemed easier than with the right hand to keep the instruction in mind. (Better record than with the right hand.) (74) R. h. When I struck at 20 cm. I was not successful in keeping other things out of my mind. There was a tension in the muscles of the hand. (75) L. h. It is not as difficult to keep the instruction in mind (better record). (76) R. h. The instruction was in the margin of attention all the time. When my hand moved to the side I brought it back. (That is, his attention was on that feature of the task.) (77) L. h. The instruction left more often and the movement of the hand persisted in coming into consciousness more frequently and the more it did the worse I did. (Record 12.5 cm. The worst record he ever made. The movement evidently became too prominent in consciousness, inhibiting the effect of the instruction.) (78) R. h. I noticed continually whether the point went to one side or the other. This kept other things out of consciousness. (Here he had a visual impression of the movement. In the previous series, he prob-

ably had kinesthetic imagery, which entirely inhibited the instruction as well as everything else.) (79) L. h. There was more or less tension in the hand. The other hand was clenched. When I started the instruction was only faintly in mind while the attention was centered on the point. (80) R. h. There was no difficulty in keeping other things out of consciousness this morning. I did notice tension in the muscles, however. (Record 9 cm. Only excelled by the first record of the series.)

Subject C

Positive Instruction.—(81) R. h. The idea of going out of a straight line seemed to occur before the movement. I had to renew a picture of the straight line. (82) L. h. It was more difficult to keep the image of a straight line. It kept disappearing. (83) R. h. It seemed as if the line were falling down the side of a steep embankment toward the side of the instrument. It was impossible to project the line far beyond the point. (Due to its going toward the side?) (84) When the hand went to the side the visual image of the line changed its path and came back to the straight path before the hand did. (85) L. h. When I went to the 4.5 cm. mark I had an excellent control of the visual image. (86) R. h. Only once could I keep the visual image of the straight line in consciousness. (Then he made the best record of the day, 4 cm. The average for the day was the highest of the series.) (87) L. h. It was difficult for me to carry the instruction for any length of time. My thought centered on the left hand. (Notwithstanding this difficulty he made the best record of the series with the left hand.) (88) R. h. When the point touched I experienced a mental blank. This seemed simultaneous with the touching, as far as I can say

Negative Instruction.—(89) R. h. There was present the idea of avoidance. (No further analysis was given.) (90) L. h. There was the fear of touching the sides. (91) L. h. Before I touched the side I was conscious of eye movements, combined with the auditory image "You looked." (92) R. h. There was the visual image of being in a long shaft, accompanied by great eye strain. The instruction took the form of a straight line. I could not keep the negative in mind. (93) R. h. There was a strain in the back of the neck, which became very intense as I neared the end of the groove. (94) L. h. It was quite difficult to keep the instruction in mind, quite decidedly more so than with the right hand; I noticed no tension except in one trial. (Record 15.5 cm., the worst of the series.) (95) R. h. I repeated auditorily "Do not touch those things." (96) L. h. I was frightened when I touched. (97) L. h. The image kept fading a great deal sooner toward the end with this hand than with the right.

Subject D

Positive Instruction.—(98) R. h. There was auditory kinesthetic imagery, "I must keep in the middle." My attention seemed on the point when I did best. (99) L. h. When I did well I was not thinking about it. (That trial he went to the 2 cm. mark.) (100) The first few trials I forgot the middle path and tried to keep my balance. In the last part the motor set of going down a straight line was in mind. (101) R. h. There was the kinesthetic sensations of a sweeping motion and no particular thought of the sides. Sometimes I had a visual image of the path ahead of the point. (102) L. h. When I did best there was the consciousness of a steady, even movement. The instruction was in mind as a straight even sweep. (Form of imagery not given.) (103) R. h. The instruction was not prominent. It seemed as if it disappeared just before the point touched. (104) L. h. I tried harder to keep the instruction in mind this time. The imagery was more visual. (Record 10.3.)

Negative Instruction.—(105) R. h. I found it very hard to keep the instruction in mind. In fact I tried to forget it. When I did think of the instruction I had a sensation of dizziness. When I hit I was thinking of the instruction. If I forgot the instruction I felt myself going along smoothly. (106) L. h. I thought I must keep the instruction in mind and struck sooner than when my mind wandered. (107) R. h. There was much trouble in retaining the instruction. I wanted to go down the middle instead. There was the muscular sensation of holding the stylus steady. (108) L. h. I did not repeat the instruction often to myself. There was present the auditory kinesthetic imagery "When it goes out of the middle pull it back." I had an idea that the instruction was working this time. (10.3 cm. Second best record.) (109) R. h. I was conscious of the sides and of watching the needle and pulling it back. There was tension in mouth, shoulders and arms. (110) L. h. As I saw the point approach I said "Come away from there." My mind wandered much more with this hand. (14.9 cm.) (111) R. h. I was conscious of intention to watch the point and to pull it back. There was visual and verbal imagery with some sensation of pulling the point back. The instruction tended to disappear and I kept repeating it. (112) L. h. This time I did not repeat the instruction so much. It remained in the margin of consciousness. (9.1 cm. The best record with the left hand.) (113) R. h. The instruction was dim. There was sensation of tension and a feeling of an orderly progressive movement in a straight line. Once or twice I realized I was not thinking of the instruction. When I knew I was doing well I did not try to hold the instruction. (6.6 cm. The best record with the right hand.) (114) L. h. I was more conscious of the instruction and the effort to hold it.

Subject E

Positive Instruction.—(115) R. h. When visual imagery was present I felt a much greater steadiness in my arm. If my mind wandered so did my hand. (116) R. h. There was a feeling of tension when I started down. The instruction was constantly in mind. (117) L. h. I was not able to get as definite an image of the line down the center and it was harder to keep it after I did get it. I was more conscious of kinesthetic sensations than with the right hand. (118) R. h. There was a great effort to hold the hand in the center and that broke up the visual imagery. With an effort I brought back the visual imagery. When my hand wandered it at once brought up the instruction. (119) L. h. When I touched the side I was attending to the movement rather than to the visual imagery. When I went furthest I was able to retain a distinct visual image. (120) R. h. Usually after I touched, the visual imagery disappeared. Once or twice this occurred just before I touched. I noticed a tendency to waver whenever I lost the instruction. (121) L. h. It seemed that as the auditory imagery decreased, the visual imagery increased in vividness. (122) L. h. It seems as if the instruction goes out of mind just before I touched. (He was asked to report upon this point.)

Negative Instruction.—(123) R. h. There was a visual image of the two sides. The possibility of two sides prevented a steady concentration. I went furthest when the old instruction came back to me. (124) L. h. There was a greater tendency for concentration to be inhibited. (125) R. h. I made the poorest record when I could not keep the instruction in mind. I watched the point and brought it back when it neared the side. (126) L. h. My attention was divided between the two sides. (127) L. h. I inhibited the visual image of the line as it interfered with the instruction. There was a tension in the arm. I think the time I went furthest I lost sight of the instruction. I seemed to have my eye on the side and when my hand went to the side I pulled it

back automatically. (128) L. h. Tension was very decided. The greater the tension the more successful I was. It seemed that whenever I touched the side the tension was somewhat relaxed. (129) L. h. I could note a definite correlation between tension and touching the side and vividness of instruction and touching. When the instruction faded from my mind I most often touched. (130) R. h. My arm and whole body were rigid. (131) L. h. I was not able to feel as tense. There was not a feeling of steadiness. It was slightly more difficult to keep the instruction in mind. (He made a slightly better record, however.)

Upon examining this introspection we find that there are two important characteristics of the conscious representation of the negative instruction, the inhibitory set as seen in a tension of the muscles of the hand, the back of the neck and sometimes of the whole body (72, 74, 79, 91, 93, 109, 128) and a focusing of the attention upon the movement of the stylus (70, 71, 109, 111, 127). In the latter case the verbal instruction is not infrequently either in the margin of consciousness or entirely absent as a conscious element. The repetition of the instruction in the fore-period may be sufficient to enable it to continue through the main period as a determining tendency in the form of a neural set. The perception of the movement to the right or the left, which by the force of the instruction immediately occupies the fovea of attention, induces directly an inhibitory action and the hand is drawn back.

A further difference is seen in the greater difficulty to keep in mind the verbal negative instruction (92, 105, 107, 110). It seems probable that this is due to the fact that the instruction in its verbal form, "Do not touch the sides," suggests the idea of touching or at least the idea of the sides, and as this is opposed to the purpose of the task, it is more or less suppressed by the determining tendency and instead is represented by some form of imagery which directly aids the movement and at the same time adequately represents the meaning of the instruction.

In the preliminary report of this work¹ mention was made of the fact that there were indications of a greater difficulty in keeping the instruction in mind for the left hand than for the right. That is, there seemed to be a coördination between

¹ 'The Clark Meeting of Experimental Psychologists,' *Psychological Bulletin*, June, 1912, p. 236.

the smoothness of the nervous discharge (assuming that the left hand is the unpracticed hand) and the ability to keep the corresponding imagery in consciousness. There are exceptions, as may be seen in the above introspection, when a greater ease was reported for the left hand and even as regards the movements of the right hand we have quoted an instance in which this difficulty of retaining the imagery was met with in a case where the record was a very good one. Yet on the whole the data of the completed tests bear out this preliminary statement (94, 97, 114, 124). That subject *A* was able to keep the instruction well in mind with the left hand is explained by the unique association described above, which this subject made with the left hand (52).

This brings us to a survey of the presence of imagery as an accompaniment of the voluntary act. In the case of all the subjects we find some instances of imagery, but the amount varies greatly, as does also the difficulty they have to retain the desired imagery and the importance they attach to its presence. Subjects *C* and *E* frequently call up visual imagery (85, 86, 115, 117, 119, 124, and notice it especially when the record is good as in 8). In subject *A*'s consciousness there is little representation of the instruction until the point wavers, when it appears in verbal form. His are the best results. In 54 and 55 we find exceptions. Here the instruction is in consciousness and the results good. Subject *B*, who has the second best record, has little imagery except the repetition of the instruction, which is done, partly at least, to keep distracting thoughts out of his mind. When the hand is moving smoothly and the records are good we find entire absence of the instruction (99, 112, 113).

An attempt was made to discover if the verbal instruction and other imagery disappeared just before or at the time the point touched the side. This was in many instances difficult to decide. In fact under almost any conditions the time order is one of the most difficult things upon which to introspect. That the act of touching should, through its disturbing influence drive the instruction out of mind is natural, but that the original imagery should persist until the pencil

the end was uppermost. We
 —*A*—assumed a negative
 action and that the results
 are that the best work
 action, but this may vary in
 voluntarily the negative
 Whether this was the best for
 whether, if he had forced him-
 results would have been better
 certainly in consideration of these
 instruction becomes important.
 Introspection to ascertain what
 essential. With untrained subjects,
 this is impossible. A uniform
 suggested by the experimenter
 by emphasis and explanation.
 of one subject, *D*, and once with *A*,
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 this was a movement requiring accuracy
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 nated by the performance of the movement itself."
 on the auditory imagery of the instruction, the
 imagery of the result best sets off the several neural

¹Motor Action,' PSYCHOLOGICAL REVIEW, March, 1913.

touched, even though the hand is no longer following the instruction but going to the side, shows that there is still present a will impulse, although may be a most incipient one, directed against the actual movement (88, 120). That the imagery under the positive instruction should disappear at the moment the point began to deviate would be expected (48, 57, 81, 103, 120, 122). On the other hand the negative instruction should appear as the point begins to waver (56, 57, 58, 163).

As in the fore-period, so also here, only in the case of subject *D* do we find any reference to imagery either visual or kinesthetic of the intended movement. This was in the form of a kinesthetic sensation of a sweeping motion down the board (100, 101, 102).

CONCLUSIONS

Although the foregoing facts are gathered from only five subjects during the short period of four months they suggest the following:

1. The variations in the average of ten tests on the accuracy of movement for one individual from week to week are so great, for example 3.8 cm. one week followed by 8.1 cm. on the succeeding test day, that the results of one day cannot be taken as an index of an individual's motor control. It might be argued that, as the first tests are often the best of a series, they might well be taken as an index. There are, however, too many exceptions to justify this conclusion.

2. Just as in the reaction experiments we have two different attitudes, a motor and a sensory, producing two different sets of results, so here we have what for want of a better name may be termed a positive and a negative attitude toward the task and these attitudes also influence the results. This does not mean that the subjects at times assumed only a negative attitude. It would be absurd to suppose that the negative, not to touch the sides, could produce the movement down the board. What is meant is that a negative attitude exists along with a positive attitude either as conscious content or as neural set and that in many instances

here described the negative attitude was uppermost. We saw that at least one subject—*A*—assumed a negative attitude under a positive instruction and that the results clearly showed this. The indications are that the best work is done under the positive instruction, but this may vary in individuals, Subject *A* adopted voluntarily the negative attitude for the right hand. Whether this was the best for the neural arc of that hand or whether, if he had forced himself into a positive attitude, his results would have been better cannot be answered here. Certainly in consideration of these results the wording of the instruction becomes important. In laboratory experiments the introspection to ascertain what is the actual attitude is essential. With untrained subjects, especially with children, this is impossible. A uniform attitude can, however, be suggested by the experimenter and at least partially obtained by emphasis and explanation.

4. Except in the case of one subject, *D*, and once with *A*, neither in the fore-main or main period, under the positive or negative instruction, was there any imagery of the intended movement. Although there were considerable individual differences, the usual conscious content aside from the verbal instruction was a representation of the end or result to be obtained, especially under the positive instruction. It must be remembered that this was a movement requiring accuracy and skill. We know that under similar conditions in daily life this is the attitude assumed. The pitcher sees the ball cutting the plate, the bowler sees the line which the ball makes. In the questionnaire sent out by Professor Thorndike¹ the majority of the psychologists preferred the statement "To make your spear fly straight and pierce the breast of your enemy, it is useful to imagine the spear striking him full in the breast" to the following: "To make your spear fly straight and pierce the breast of your enemy, it is useful to think hard of the visual, tactile and kinesthetic sensations originally produced by the performance of the movement itself." Aside from the auditory imagery of the instruction, the visual imagery of the result best sets off the several neural

¹ 'Ideo-Motor Action,' *PSYCHOLOGICAL REVIEW*, March, 1913.

arcs. An act of skill requires a smooth movement, which is produced by the proper innervation of a group of muscles. If the attention were on the movement, one particular part of the movement is liable to receive undue emphasis and the desired result will not be obtained. The end result—in our work a visual image of the line to be traversed—is related by experience to each group of muscles equally and therefore best serves the purpose.

If, as according to the ideo-motor theory, an idea tends to produce the movement it resembles, then it seems more than likely that, taught by experience, it would have been an image of the movement, either visual or kinesthetic, which our subjects would have most often selected. That with the above-mentioned exceptions not one of our subjects did so, speaks against this form of the theory. This is in support of the doctrine set forth by Thorndike.¹

5. The negative attitude was characterized in the case of all the subjects by an initial innervation of antagonistic muscles, which innervation had its conscious representation in a feeling of tension at times extending to muscles other than those to be inhibited.

6. There were subjects who required imagery, visual and kinesthetic, in order to carry out the movement. There were also those who needed only the instruction verbally and at times not even that. A repetition of the instruction in the fore-period would set off a sufficiently strong determining tendency, so that all that was necessary was to add another, a negative tendency, *i. e.*, that of keeping all else out of consciousness. Even those who required visual imagery reported that an especially good result was obtained without

¹ *Op. cit.* See also his 'Elements of Psychology,' pp. 281-284. Our data are also in accord with C. T. Burnett's as published in 'An Experimental Test of the Classical Theory of Volition. Studies in Philosophy and Psychology,' pp. 393-401. Further we find our results supporting Thorndike's statement in his 'Ideo-Motor Theory,' p. 100: "The rule is that a mere likeness does nothing; when . . . an increase in likeness goes with a decrease in the strength of the habit's bonds, likeness has the appearance of diminishing an idea's potency to arouse its act; when greater likeness of an idea to an act implies greater frequency of the idea as a situation leading to the act in past behavior, then greater likeness has the appearance of increasing the tendency of the idea to arouse that act." Compare the attitude assumed by *B* with his left hand.

the instruction being represented. Those who trusted most to a non-represented determining tendency did the best. This seems to suggest that the progress in muscular control is inversely proportional to the amount of imagery employed. In the beginning imagery is required. As the control becomes better the imagery may drop out. If the coördination is still imperfect a comparatively successful movement is only obtained when the imagery is present. As in the case of *E* there is a good correlation between relatively satisfactory movement and permanence of imagery. On the other hand there are probably those who inherit more or less perfect coördination and with such persons the imagery may disappear within a very short time. These are generally the skillful persons and they can as a rule give you the least information as to how they performed the task.

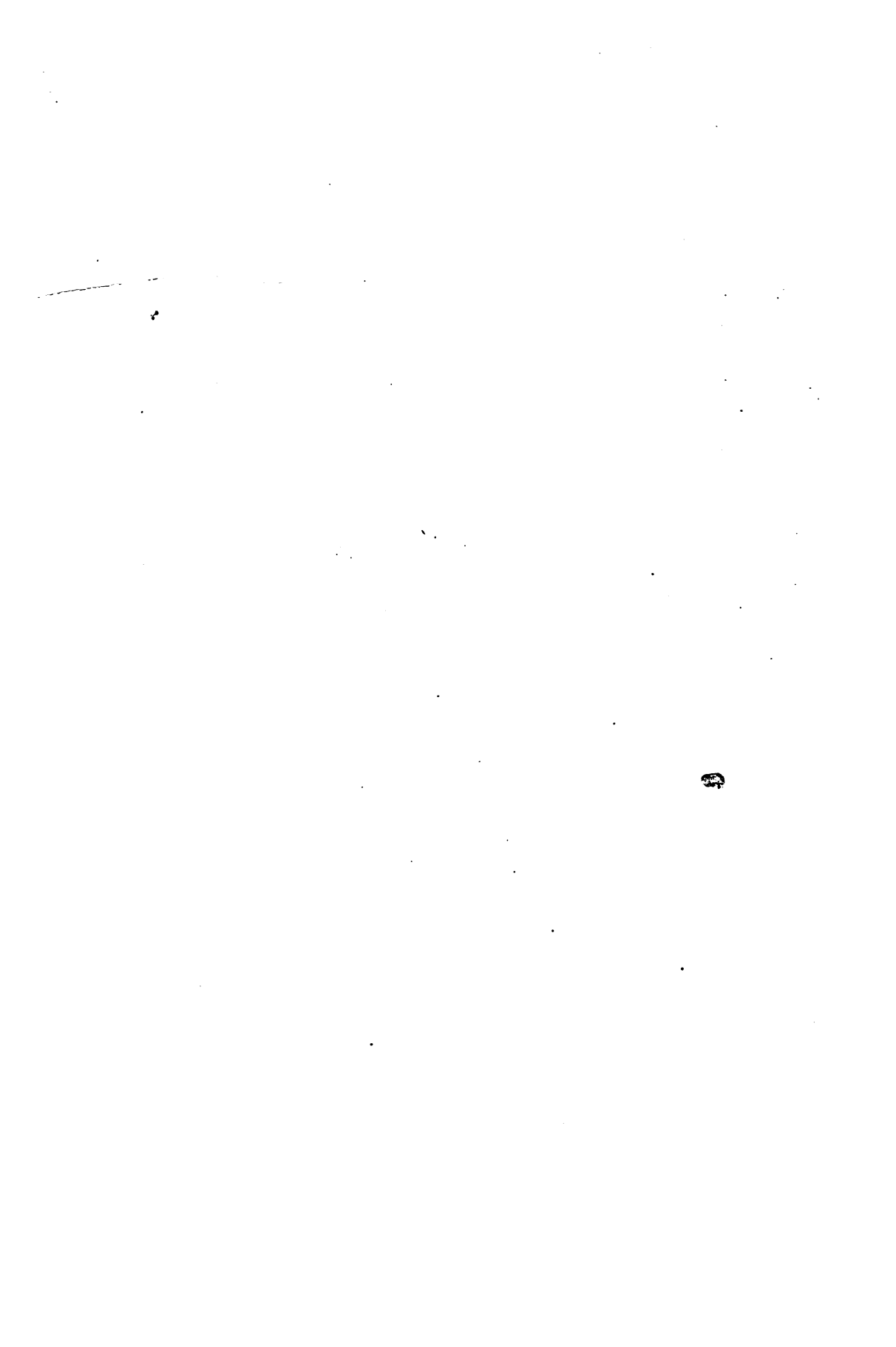
Finally the attitude of keeping all else out of consciousness is also significant. That is essential to skillful movement. A determining tendency is at once inhibited, at least partially, by the entrance of a foreign thought. This suggests a reason why a certain type of mind is unskilled in movement, that is, the imaginative type, which is continually thinking of other associated things. As there are more of the imaginative type among the intellectual, so it is not surprising that there is more motor skill among the non-intellectual.¹

It would seem that when E. C. Rowe says that "in matters of control ideational processes are only vicarious and supplementary, and even then their functional value may be

¹ Bolton, T. L., 'The Relation of Motor Power to Intelligence,' *A. J. of Psych.*, 14, 1903, pp. 615-631. The statement of Professor J. B. Watson in his article 'Psychology and Behavior,' *PSYCHOLOGICAL REVIEW*, March, 1913, pp. 174, that "It has been shown that improvement in habit comes unconsciously. The first we know of it is when it is achieved, when it becomes an object. I believe that consciousness has just as little to do with thought processes" is not in contradiction to my exposition, if I interpret Watson's view correctly. The subject is probably not conscious of the improvement. It would be a fallacy to suppose that he interpreted the amount of imagery in terms of degrees of practice. This relation is found by the experimenter after an examination of the results of introspection. In the last sentence if 'to do' is meant to imply an interaction, then I should say the statement is correct. If, however, a correlation between conscious states and degrees of improvement is found, then the amount of conscious content does stand as an index of this improvement and in that sense it has 'to do' with the improvement.

questioned. Control is necessarily perceptual and sensory" he overlooks the fact that the ideational processes represent the determining tendencies and that it is only when this representation is present that we should speak of a fully conscious voluntary movement, that is, a movement in its highest stage of development, before the short-circuiting in the lower centers has begun.¹

¹ See E. C. Rowe, 'Voluntary Movement,' *A. J. of Psych.*, 21, 1910, p. 562.



EXPERIMENTAL CONDITIONS

In the spring of 1912 Agostino Levanzin, a lawyer by profession came from Malta to undergo a prolonged fast at the Nutrition Laboratory of the Carnegie Institution of Washington in Boston. It was deemed advisable, in order to have as complete an investigation as possible of all the conditions during fasting to supplement the physiological by mental tests. It is the purpose of this paper to present the results of the latter.

Levanzin has for a number of years been interested in the subject of fasting, believing that most human ills can be cured by abstinence from food for a long period of time. He had already made one fast of forty days. It was his claim that during that period all his mental faculties so increased in efficiency, that he could hear, see, smell and think better and that on the 26th day he was able to plead a case in the law court. His ostensible purpose in coming to America was to substantiate, if possible by strict scientific methods his own casual observations. It is desired to emphasize this attitude in order to convey an idea of the keen interest which he showed during the tests and the willingness with which he attempted to fulfil all conditions, a co-operation the thoroughness of which might have been doubted in the case of a man fasting merely for pay.

Before his first fast he weighed about one hundred and eighty pounds and after the fast 140 pounds. When he arrived at the laboratory his weight was 134 lbs., at the end of the fast 106 lbs.¹ He was a man of 40 years of age, of medium height and slender. When not in conversation his manner was languid and it is perhaps due partly to this that he seemed to lack physical strength and vigor. In temperament he is of the decidedly emotional southern type, sensitive, quick to anger, loquacious, credulous and fertile in imagination. This last characteristic is probably responsible for the fact that the unusual appeals to him. Once having espoused a cause or entertained an idea he holds to it

¹ There was an almost steady decrease of about one pound a day.

tenaciously. He is a man of a few fixed ideas or complexes which form the basis of his mental life.

L. arrived at the laboratory on the afternoon of April 10th. He took his last meal on the evening of April 13th and his fast was considered to date from the morning of the 14th. Absolutely nothing but 750 cc of distilled water daily passed his lips during the thirty-one days. He lived on a balcony in the laboratory, slept at night in the calorimeter and was watched constantly. His luggage was examined on his arrival and all of his mail was opened at his request in order to preclude every possibility of his receiving a stimulant through those sources. His daily program consisted of tests by different specialists and varied but slightly. His leisure he spent in reading and writing and receiving guests. Unfortunately the weather for a good part of the time was unfavorable, but when possible he went on the roof for an hour or took a ride in an open carriage with an attendant.

The tests herein described lasted from April 11 to May 15th inclusive.² Food was taken on April 10th, 11th, 12th, and 13th and again on May 15th. The intervening 31 days were fast days. The psychological tests were made at five p. m. each day and lasted one hour. During the half hour before the tests he rested.³

From the above it is seen that the psychological tests were made under as nearly as possible ideal conditions. Alone from the fact of the complete control of diet and occupation the tests seemed worth the attempt, for it is seldom that psychological experiments can be conducted over such a length of time under such constant conditions. Perfect as they were, however, one factor important to mental measurement was found to vary, that is the mood of the subject. As far as L's willingness to co-operate is concerned there was nothing to indicate to the experimenter a change in this attitude or that his general interest in

² An idea of his intelligence and interests may be obtained from the association reactions. See appendix II.

³ The tests on April 11th were tentative and are not included in the curves.

the work relaxed at any period of the series. On the other hand there is no doubt that he was happier during the first days, rather depressed and silent in the middle and somewhat irritable and excitable toward the end, although this irritation was at no time directed toward the tests. The greatest depression occurred after a prolonged continuation of bad weather and very much decreased after he was able to go out in the air. He was also much happier after having received visitors. He himself remarked that the monotony of the program was the most difficult thing he had to endure. As to his physical condition he made few complaints. He felt well throughout and insisted that he had no sense of hunger, not even during the first days.⁴ The only discomfort of which he spoke was the coated condition of his tongue and the unpleasant taste in his mouth. It was his idea that the fast should continue until this disappeared and it was for this reason that he was loath to break his fast on the 31st day.⁵ Although he seemed more feeble toward the end of the fast and gave one the impression of a man convalescing from a weakening illness, yet he was always able to walk without assistance and at no time was it necessary to omit or alter a test through lack of strength on his part. On May 15th, the day he broke his fast, he suffered severe colic induced by the food he ate and although tests were made, the conditions were most unfavorable. It had been planned to continue the examination for several weeks longer, in as much as such tests would ob-

⁴This is contrary to the experience of most fasters. W. B. Cannon and A. L. Washburn (*An Explanation of Hunger*. *Am. Jour. of Physiol.*, 1911-12, p. 441) describe the feeling of hunger as follows: "Hunger . . . is a dull ache or gnawing sensation referred to the lower mid-chest region and the epigastrium. It is the organism's first strong demand for nutriment, and, not satisfied, is likely to grow into a highly uncomfortable pang, less definitely localized as it becomes more intense. He further states (p. 442): There is abundant evidence, however, . . . that during continued fasting hunger wholly disappears after the first few days." Professor Cannon has recently informed the author that from what certain fasters have told him he believes that sensations of hunger may be absent from the beginning; that in fact some people may never have the sensations of hunger as just described.

⁵Thirty days were considered sufficient for the physiological tests and he was allowed one day more to excell Succi's record.

viously be of inestimable value for comparison with the fasting tests. Unfortunately that was quite impossible under the circumstances and an entire year elapsed before further records could be obtained.

Several factors influenced the selection of the tests. In the first place the time was limited. There was only one hour daily available and it seemed advisable to arrange for as many tests as possible during this hour in order to obtain a good mental picture. It was therefore necessary to choose short tests and also those requiring the minimum of effort, as one test had to follow the other without pause for recuperation. For example prolonged tests for fatigue would have been of great value but they could not be considered. In the second place the fasting began a few days after L's arrival and little time could be devoted to preliminary trials in order to obtain the best combination and the program once arranged could not be fundamentally changed.⁶ After consultation with Professor Dodge a series of tests were selected. A few days' experience, however, showed the necessity of several alterations, and the revised program was as follows: 1. Rote memory for words, 2. Tapping test, 3. Strength test, 4. Tactual Space threshold, 5. Touch threshold, 6. Free association and reproduction reactions, 7. Association reactions, genus-species, 8. Association reactions; noun-verb, 9. Cancellation test, 10. Hand-writing,⁷ 11. Visual acuity, 12. Memory for words after 55 minutes. Later the touch threshold, which was taken on the under part of the lower forearm with a von Frey hair, was discontinued on account of the impossibility of obtaining reliable results in a short period of time. The association reaction genus-species was also omitted through difficulty in finding sufficient reaction words of equal simplicity. In addition to the tests Levanzin was requested to describe all the dreams he had on the previous night.⁸ This was given before the visual acuity test. All the tests with the exception of that of visual

⁶ A few minor changes were introduced.

⁷ A superficial examination of the daily records revealed no change. A systematic examination of the data has not yet been made.

⁸ See appendix I.

acuity were made in a small room free from disturbing influences.⁹

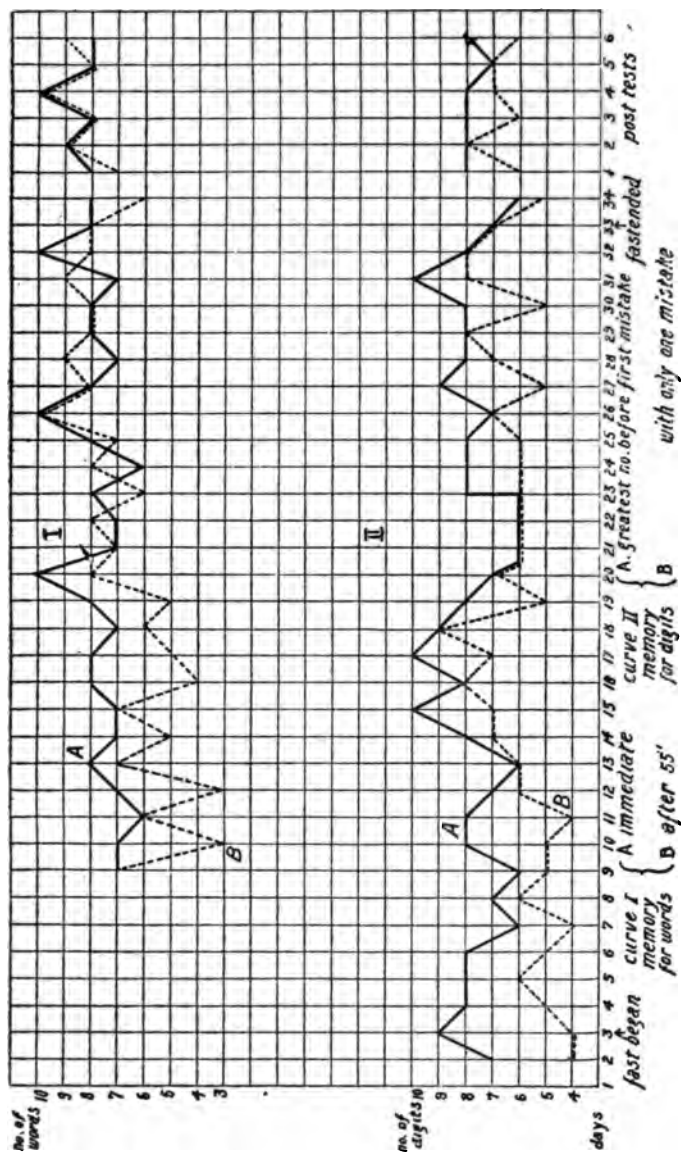
The general conditions of the experiments and the nature of the tests having been described, each test will now be treated separately, first as to the particular conditions and second as to the results.

Memory for Words

Ten one syllable words were chosen and these were read twice to the subject, who recalled as many as possible immediately after the second reading. After fifty-five minutes the subject again attempted to recall these words.

From the curves it will be seen that there are marked fluctuations, a circumstance which is always met with in mental tests and which will be found in all the curves. It will therefore be only possible to speak of general tendencies throughout. In the curve for immediate rote memory (IA) it will be seen that the poor record made on the eleventh day (the third day of the test) only occurs once again and that on the twenty-fourth day, while a perfect score of the ten words was made three times and all of them during the last two-thirds of the fast, so that although the initial records occur frequently toward the end, yet the curve as a whole shows a slight general improvement, but so slight that much significance cannot be attached to it. The curve (IB) indicating the amount of retention after 55 minutes, on the other hand shows a more or less steady improvement until near the end of the series and even when these last trials are included the general tendency of the curve is decidedly upward. In four instances and these all in the last two-thirds of the series the retention curve crosses the rote memory curve, which means that on these days the retention after the lapse of almost an hour was better than the immediate memory. Levanzin, upon being questioned was emphatic in his assurance that he never thought of the words in the interim, so that this relative improvement in retention was not due to any conscious repetition during the pause.

⁹It is much to be regretted that time and conditions prevented tests for the thresholds of audition and smell.



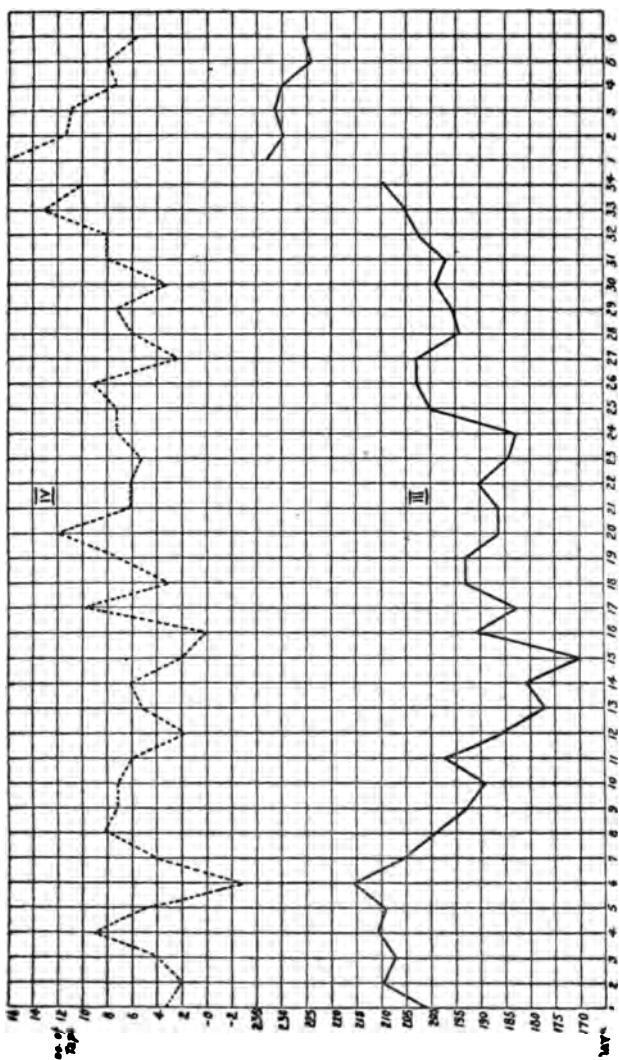
Tapping Tests

The instrument used was similar to the tapping-board described by Whipple.¹⁰ It consisted of a board 12 cm. square covered with aluminum. This metal is not very well adapted for the tapping-board, but it was selected for its lightness, it being thought quite probable that the tests would have to be made toward the end of the experiments with the subject lying down and the board resting on his chest. The stylus also had an aluminum point. The records were taken on a kymograph. The tapping lasted for thirty seconds and periods of ten seconds were marked off on the records. The subject being left handed used that hand. As he was over sensitive to cold during the fast he wore, beside a heavy woolen undershirt, a heavy dressing-gown, which added to the weight he had to lift. Neither the hand nor arm was allowed to rest on the table during the tapping.

The curve (III) shows a gradual improvement for the first six days when the maximum of the series—215 taps or about seven taps per second—was reached. The curve then descends for the next nine days when the minimum of 170 taps was reached. From this point to the end of the series there is a rise to a point just below the maximum. This rise is not, however, gradual, but consists rather of two plateaux, one of nine the other of seven days separated by decided jumps and followed by a gradual but very marked end spurt of four days.

The initial improvement can well be due to practice in using those particular sets of muscles, combined with increasing familiarity with the work. This same rise also occurred in the dynamometer tests. The drop, however, begins much sooner than in the dynamometer tests. In fact it ends in the former where it begins in the latter. One can therefore hardly say that it is a matter of muscular fatigue. The first explanation to suggest itself is a lessening in interest, and this is strengthened by the fact that the drop occurs at that time when he was most affected by the monotony of the routine work. In this test less

¹⁰ Whipple's Manual of Mental and Physical Tests. p. 101.



Curve III - no. of taps in 30" curve III - difference between 1st and 5th 10"

fast began

fast ended

later tests

depends for improvement upon the increase in muscular power than in the dynamometer tests, the main factor being the rapidity of action. We know that the rate of the reaction time is greatly affected by changes in attention and it is probable that the betterment in the muscular control, which we may assume from the results of the dynamometer tests did occur, was insufficient to offset this loss of interest. The results of the last days confirm this assumption, for here we undoubtedly have the effect of interest in an end spurt, which, notwithstanding muscular fatigue which was undoubtedly present at this time, brings the curve back to a higher level.¹¹ In regard to the two plateaux referred to above, it seems plausible to infer from what we know of the causes of plateaux in the learning process in acts of skill that these sudden rises to new levels are due to the learning of some new method or short cut. Here the most obvious short cut is the lessening of the height of the stroke.

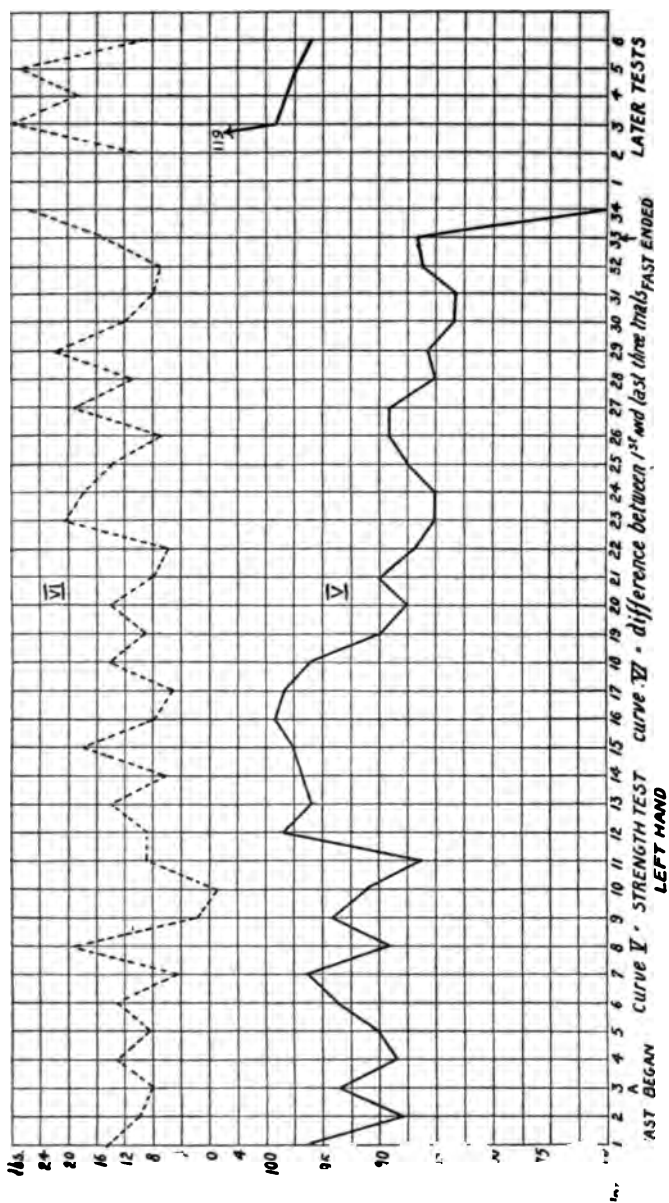
An examination of the difference curve (IV), which has been obtained by subtracting the results of the first ten seconds from that of the last ten, still further confirm the assumption of a wavering in interest. There is a gradual increase in the amount of this difference, which indicates fatigue. This increase is particularly marked toward the end when the records are improving, which means that the improvement is caused by a spurt during the first ten seconds.

In general it may be said that although initial lack of interest¹² and later muscular fatigue played a rôle, both factors being directed toward a decrease in the amount of work, yet the will impulse toward the end was sufficiently great to bring the curve back to its initial level and almost to its maximum.

¹¹ This is an error which is bound to occur with this form of tapping board. The writer has, therefore, recently constructed a board which regulates the height of the stroke, thus making it a constant factor.

¹² Against this suggestion is the fact that other tests did not show this lack of interest, but it is quite possible that the interest varied with the different tests.





Strength Tests

These tests immediately followed the tapping tests. The subject stood and received the dynamometer, one of the Collin type, from the experimenter, and pressing it returned it to the experimenter. The record was noted and the instrument returned. The interval between trials was about one second. Ten trials were made with the left hand followed by ten trials with the right.

Both in the right (VII) and left hand (V) curves there is an initial falling off, which is more marked with the former hand. The latter, however, continues to fall to the 11th day on which day it takes a decided drop, while the former declines more gradually to the ninth day, when it reaches its maximum. Both curves then rise to a maximum, which is reached by the left hand on the 16th day and by the right hand on the 12th day (the record of the first day not being considered in speaking of this maximum). The curves then fall, the left much more than the right, especially in the middle of the series, the former reaching its minimum on the 31st day. Both curves show a slight end spurt. This is, as a glance at the curve will show, merely a rough picture, there being decided rises and falls throughout.

In interpreting the curve it must be remembered that the left hand is the practiced hand and it can therefore be assumed that the muscles of that hand are the stronger. In fact the results make this more than an assumption, for the record of this hand is at all times decidedly better than that of the right hand. The initial falling off is what one must expect when the subject is not accustomed to the particular muscular exercise. There is a great exertion at first and the muscles, skin and subcutaneous tissue feel the usual strain for several days. Those muscles least accustomed to exercise are the most effected. It is for this reason that the right hand record drops more than that of the left hand. Then the muscles gradually recover and the effect of practice begins to appear. Acting against the practice is

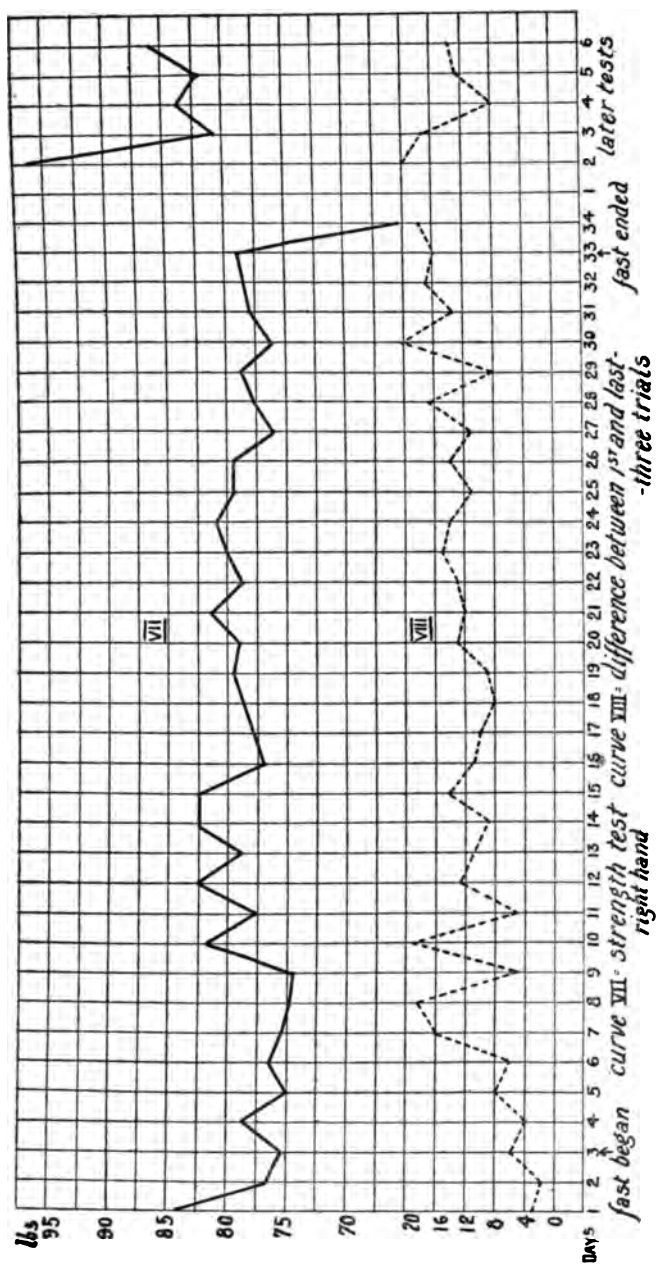
the increasing fatigue. The right hand being the unused hand gives practice more chance for its influence and although fatigue never allows the curve to reach its first day's record, yet the drop which soon begins is much more gradual, as has been pointed out, than it is with the left hand, where the effect of fatigue is more prominent.

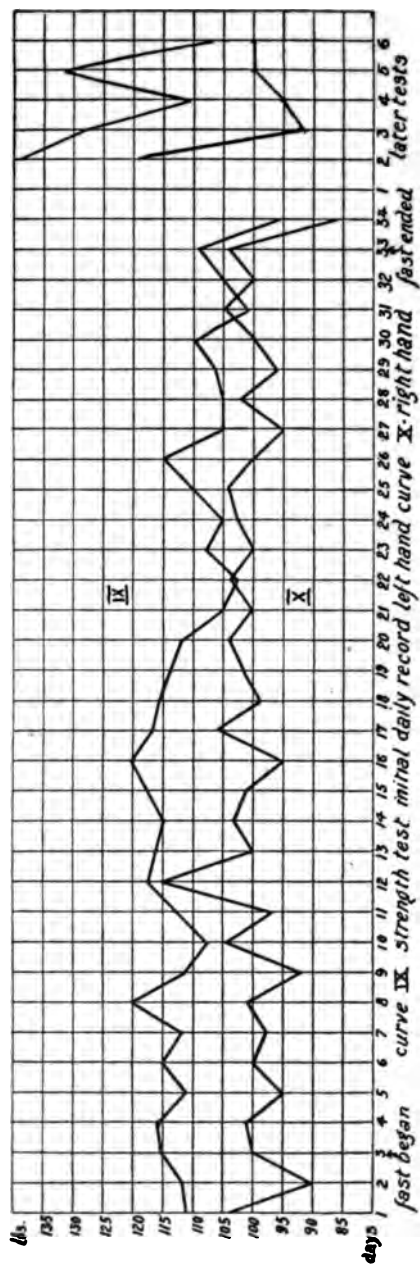
The difference curves (VI, VII), which were obtained by subtracting the average of the last three records of each day from the average of the first three helps to strengthen the conclusions just drawn. The rise of the difference curve at the same time as the fall of the main curve, means, of course, increasing fatigue, which shows itself in a greater and greater drop toward the end of the daily series. This rise in the two difference curves is relatively about the same, which means that the daily increase in fatigue is relatively the same for the two hands. Further, if we glance at curves (IX, X), we find additional indications in the same direction. This curve is plotted from the first of the daily series of ten trials. This trial is least effected by fatigue and shows therefore the greatest influence of practice. Here there is a gradual rise for the right hand until next to the last day, while the curve for the left hand begins to drop where it should according to our analysis.

In general we may therefore say that fatigue appears in both hands early in the series. The curve for the left hand drops far below the record of the first few days. The curve for the right hand shows less drop due to the greater influence of practice, so that the two curves tend to approach one another.

Tactual Space Threshold

A pair of dividers with wooden tips were used as an aesthesiometer. The threshold was found on the volar side of the fore arm, about four inches from the elbow. The points were applied on either side of a red ink dot which was made on the arm on the first day and renewed when necessary. The method of minimal change with ascending and descending series, was employed.





Five trials excluding one-point "vexier" trials were made at each distance. Four correct out of five was considered the threshold.¹⁸

For the first few days the curve (XI) keeps the high level of 7 cm. On the seventh day there is a drop to 5.5 cm., then a slight rise to a level of 6 cm. and a high threshold of 6.5 cm. on the 14th day followed by a fall to the minimum of 5 cm. on the 22nd day, which minimum is again reached on the 26th and 30th days. The final days show a rise to 6 cm. The decided drop on the seventh day may be due to adaptation to the experiment, which in this instance means the adoption of a definite and clear criterion of discrimination. The drop in the middle of the series after a more or less constant level may be due to a similar cause, that is a change to a better criterion. The rises in the latter part of the curve are never as great as those of the first part, although on the last day the curve again reaches 6.2 cm. This threshold had to be placed at 5 correct judgments as there was a jump from 3 correct judgments. This makes the threshold probably too high. If we omit the first day and compare the average of the period from the 7th to the 20th day with the average of that from the 21st to the 34th day we find a difference of .4 cm. in favor of the latter period. We may say then in general that there is an improvement, although very slight in the discriminating process, but that there is no end spurt, which latter from the very nature of the process under investigation is not to be expected.

Rote Memory for Digits

The usual rote memory test was employed. Increasing series of digits beginning with four digits were read aloud once by the

¹⁸ It had been intended to call three out of five the correct threshold, but this was not found feasible. The threshold is probably too high, but for the present purpose, where the change and not the absolute threshold is being investigated this does not matter.

The curve shows no record for the 4th, and 5th days. The experimenter was absent on these days and the physician, who kindly volunteered his services, did not deem himself sufficiently skilled in this particular test to undertake it.

experimenter to the beat of a metronome with one second intervals and were repeated as far as possible by the subject. The combinations of digits varied daily.

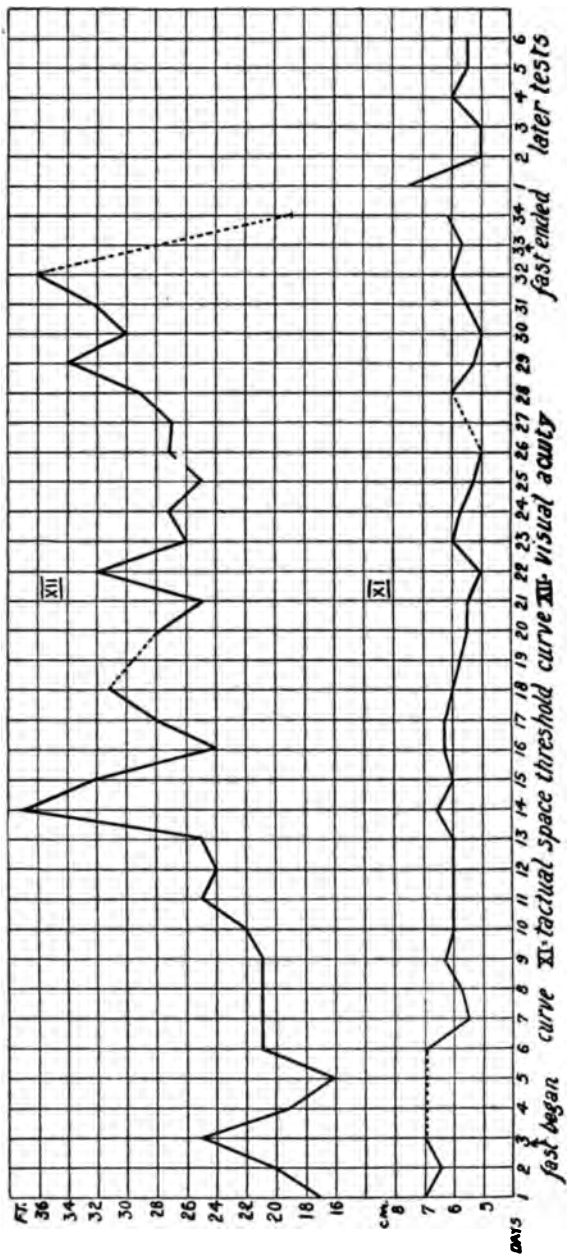
Curve II A is obtained by taking the last series that contains only one mistake, curve II B by taking the number which immediately precedes the one containing the first mistake. Curve A, which gives a picture of the rote memory process shows two apexes of maximal value near the middle and another on the 31st day. There is, however, a very low minimum in the second half of the curve and a decided drop from the maximum of the 31st day. One can, therefore, hardly speak of an improvement. The most that can be said is that the subject was, toward the end of the fast, again able to reach the maximum record of 10 digits obtained near the middle of the series. We see from the curve B that on the 3rd day a mistake was made at four digits, yet the retention is 9 digits; on the 11th day a mistake at four digits and a retention of eight, etc. It seems fair to assume from these results that curve B represents in a rough manner the degree of attention. It is only inattention that can produce results like the above. Curve B shows a decided rise to the eighteenth day, when it reaches a maximum and although it follows a lower level from this day it never reaches the minimum of the first third of the series. One may therefore say that there is an improvement in the state of attention, at least for this experiment, as the fast progressed.

Association Tests

The free association experiments consisted of the daily presentation of a list of twenty words which were selected principally from the lists prepared by Woodworth and Wells¹⁴ and with the exception of the list of May 9th, which was a repetition of that of April 11th they were all different.¹⁵ Several days after the tests were begun it was thought advisable, in order to

¹⁴ Association Tests, Psych. Monog. vol III, 5, 1911.

¹⁵ The lists will be found in appendix II. In a few instances the same word appears in two lists.

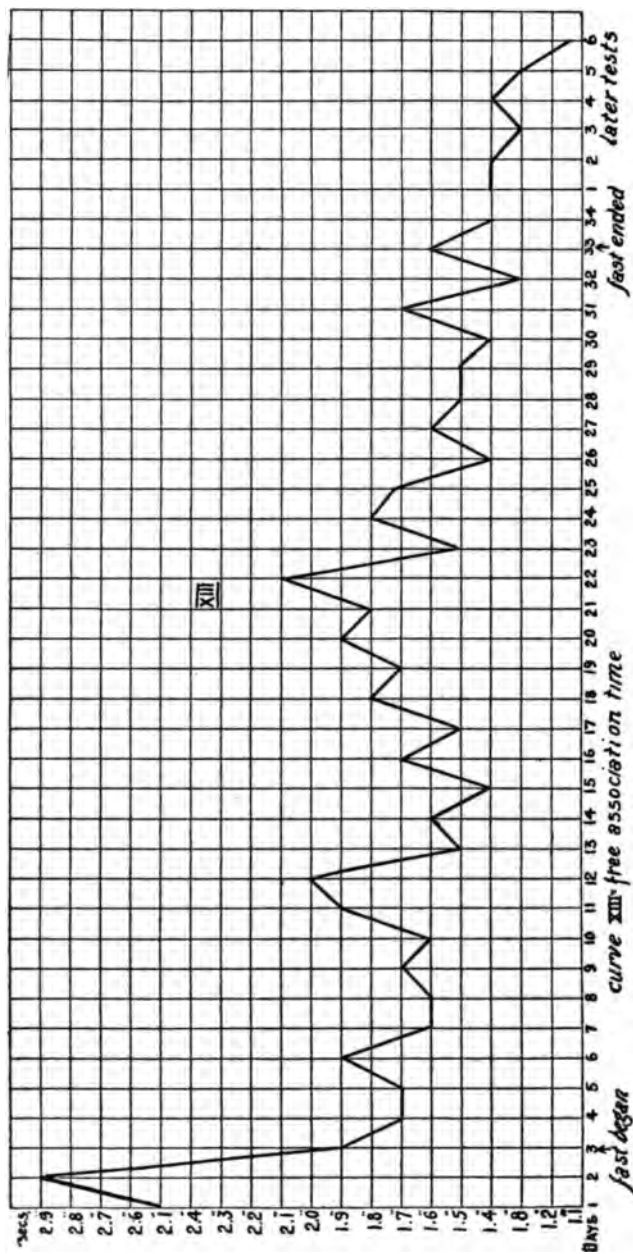


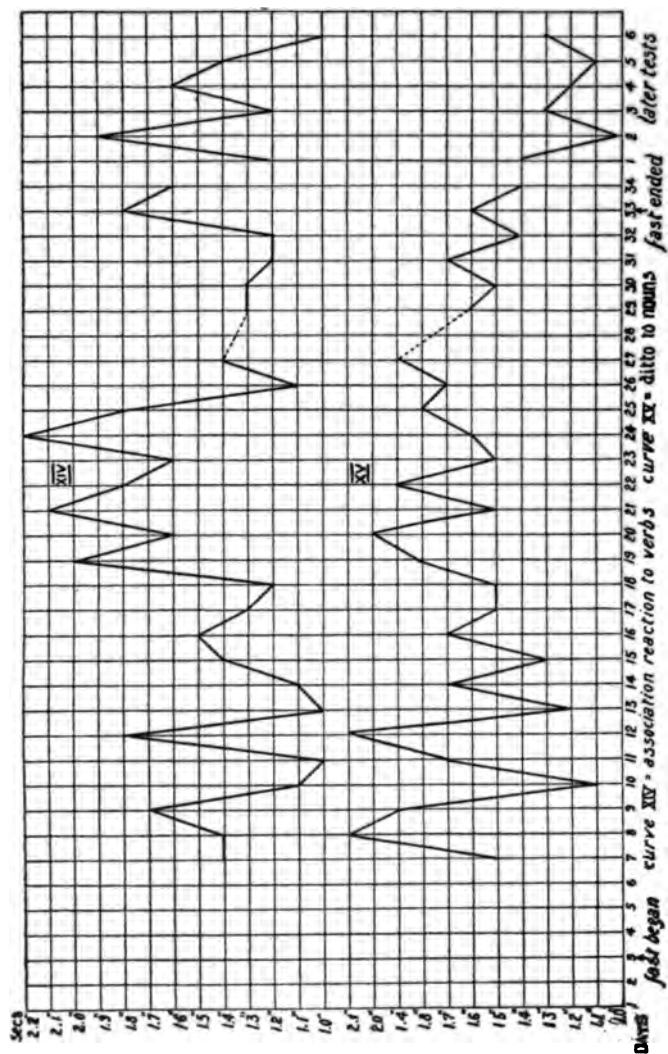
make the lists as uniform as possible, to have them composed of an equal number of verbs, concrete nouns, adjectives and abstract nouns, in the order given. This arrangement was adhered to from April 18th to the end of the tests with the exception of May 9th. The words were read aloud by the experimenter and the time taken with an ordinary stop-watch. The reproduction experiments followed these with only a pause of a minute. Although the subject was told that he need not repeat the same word, if it did not come at once, yet there is little doubt that his efforts were always directed toward that end. Levanzin had a good command of the English language although it is not his native tongue. At times, however, he had difficulty in finding the word he wanted. In such cases, he made a gesture as soon as the idea came to him and the watch was snapped at that time rather than when the English word was found. This method of procedure was not often necessary and it seemed a legitimate means of balancing the slight disadvantage he had as a foreigner. A reserve list was prepared upon which to draw when he did not understand the word of the main list.

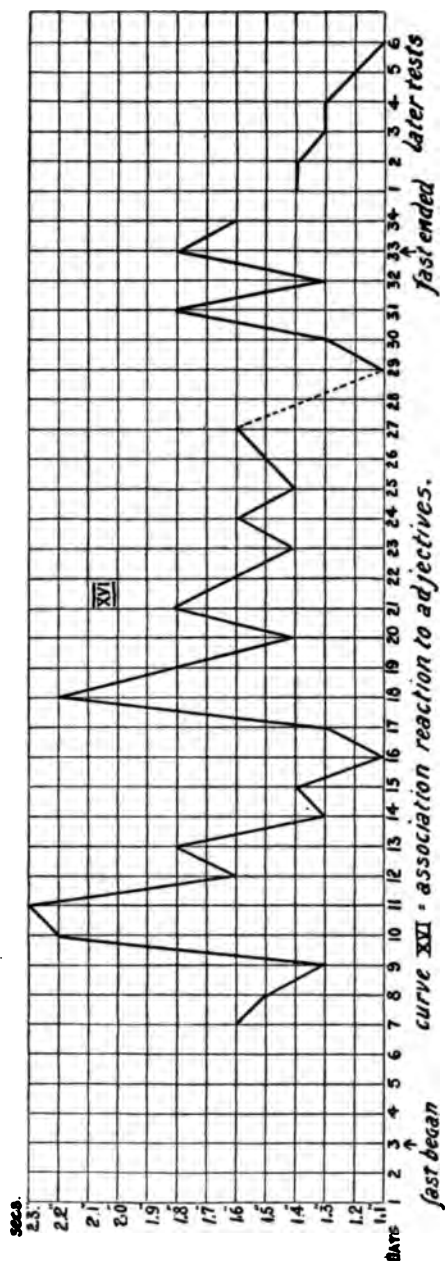
The curve (XIII) is plotted from the daily average. The average was used in order the better to include the influence of the long times, which might very well be of importance in these tests.¹⁶ The few exceptionally long times, such as 20 seconds, which may have been caused by emotional complexes, were not included.

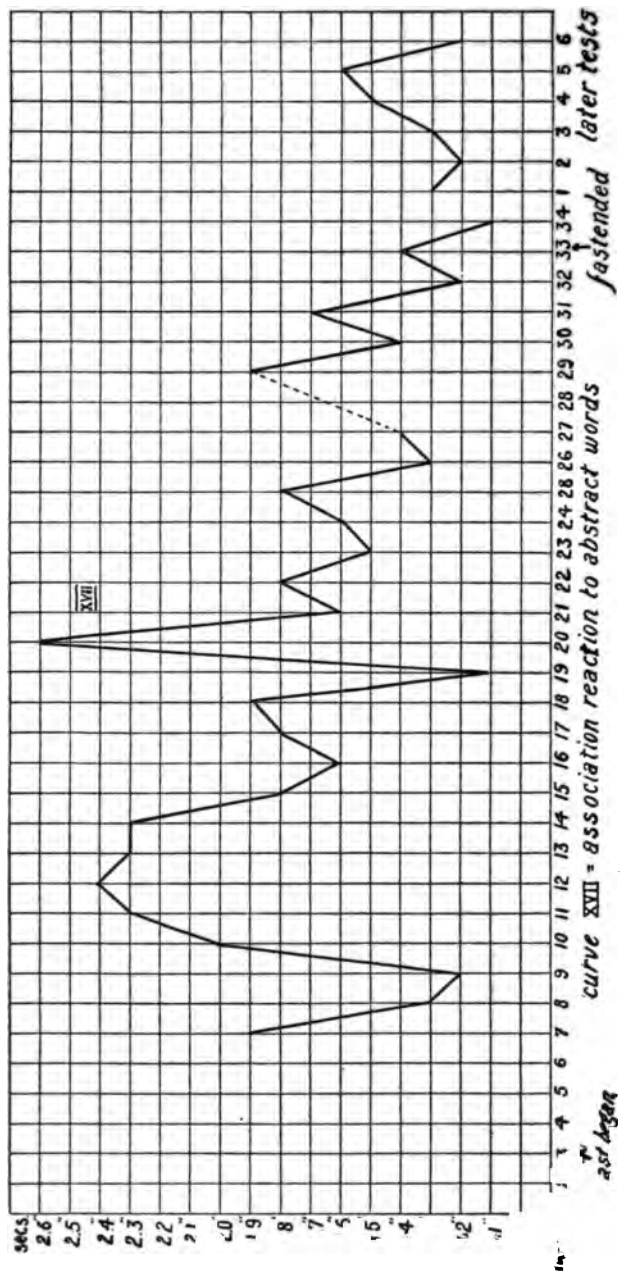
The curve begins with very long reaction times. Levanzin had never performed such tests before, so that the sudden drop on the 3rd day must be attributed to the practice improvement, which at this early stage could very well be sudden and of considerable amount, rather than to the fact that it is the first day of the fast. From this point the curve descends with a few breaks to the 15th day, when it reaches 1.4 sec. It then rises to the 22nd day when it reaches the maximum (if we do not consider the first few days) and then falls to the end of the series. On the second from the last day it reaches the minimum

¹⁶The median which was also calculated gave the same general curve.

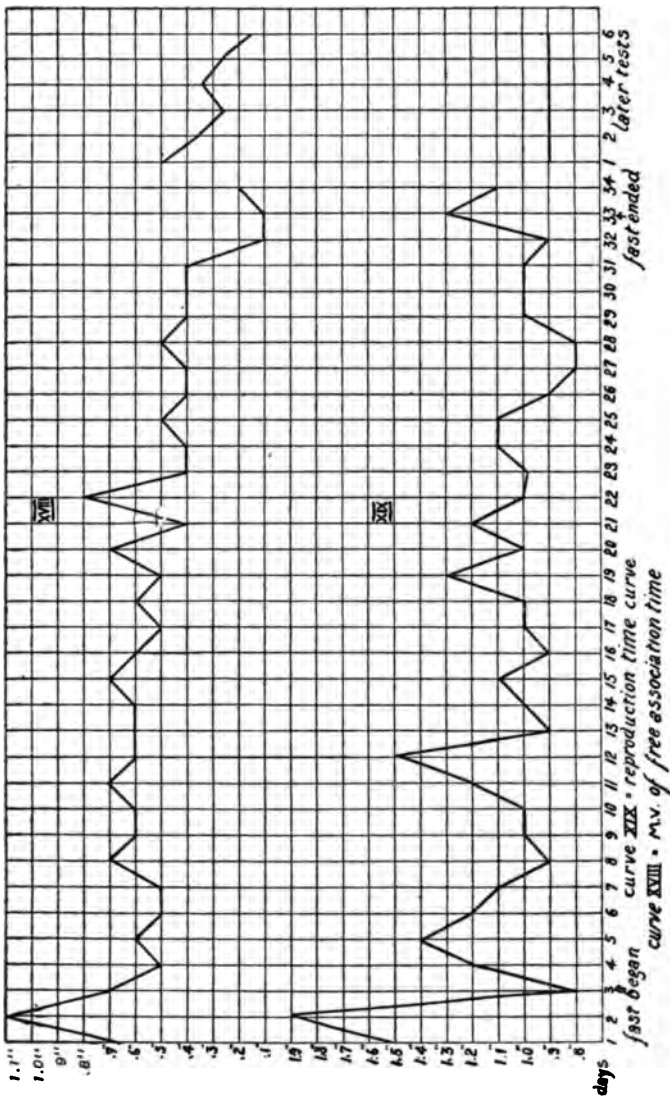




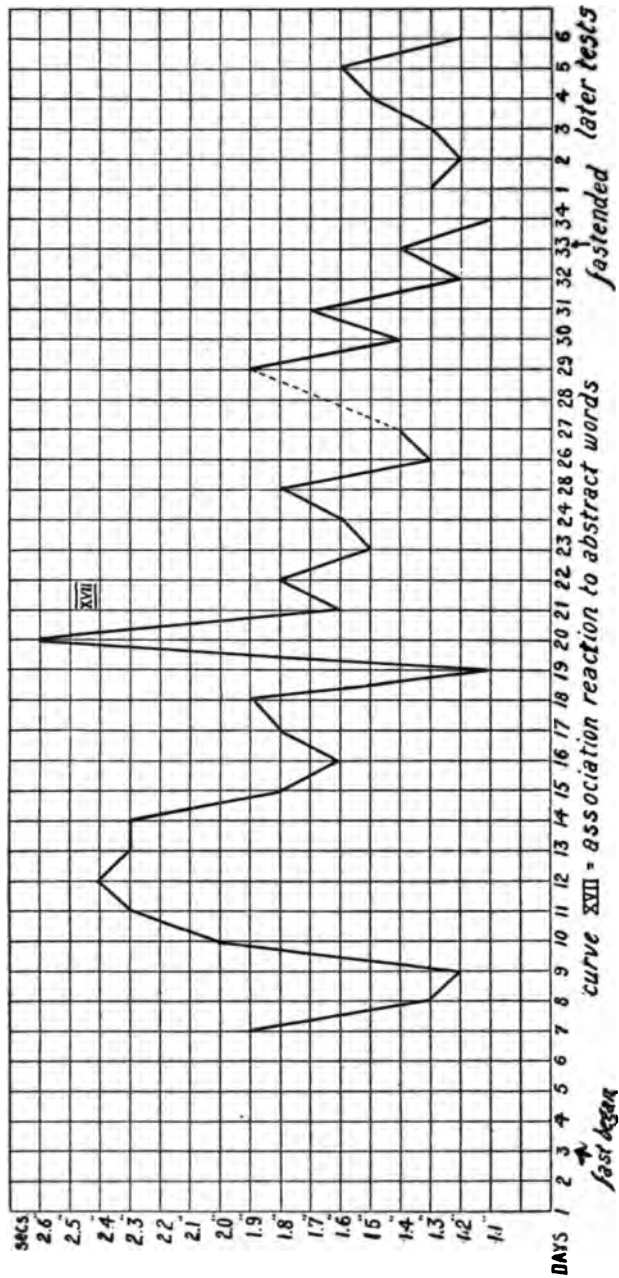


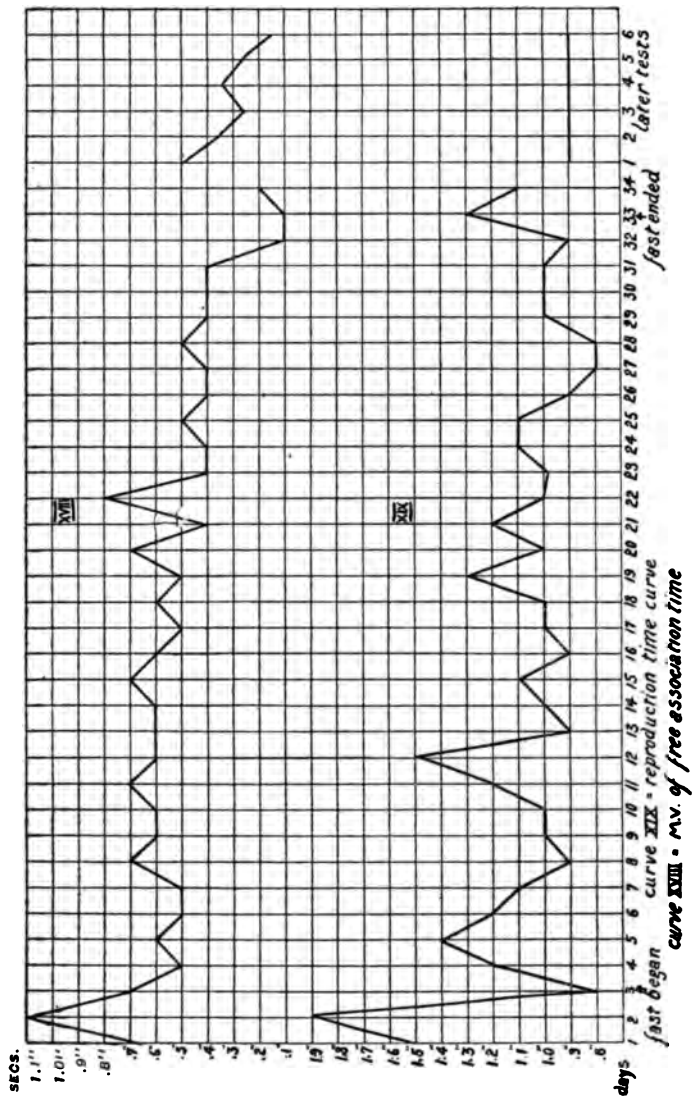


SKCS.



fast began curve XIX - reproduction time curve
 fast ended curve XVIII - M.V. of free association time
 later tests





of 1.3 sec. Also the record of 1.4 sec. is obtained three times in the second half of the series. If we include the first few days it can be said in general that there is a very decided betterment in the association times; and even if one calculates from the third day there is an appreciable drop. Especially interesting is the almost steady improvement shown in the last third of the curve.

In order to analyse the curve further, separate curves (XIV, XV, XVI, XVII) have been plotted for each of the four categories of stimulus words. It must be remembered that these curves begin on the seventh day, when this division into separate categories was first made. In consideration of the fact that the daily average is obtained from only five reactions too much importance must not be attached to sudden daily falls and rises, such as in the abstract series on the 19th and 20th days and in the adjective series on the 18th day, etc., but rather the convex shape of the verb curve, the rise in the middle of the noun curve, etc., must be considered.

It is evident that the rise in the main curve about the 10th to 13th day is caused largely by the noun curve and that the relatively greatest improvement at the end of the curve as compared with the beginning is in the abstract curve. On the other hand the verb and noun curves have several low averages in the beginning that were not reached again. In fact it is hardly possible to say that either of these curves show general improvement, certainly not the noun curve. An examination of the daily fluctuations in the curve shows that this becomes less as the tests progress.

The curve (XVIII) for the m.v. of the main curve shows a decided improvement as the fast progresses with a very low level on the last three days.

The reproduction curve (XIX) follows the tendencies of the association curve. There is the initial drop and many more high peaks in the first two-thirds of the series. If it were not for the rise on the last two days the general betterment would be more marked. The reactions were, on the whole rapid, averaging about 1 sec. and dropping as low as .8 sec. As the number of false

reproductions was very small (Table I), amounting to **only** twenty-three in 680 reactions or 3 per cent and never more than three in one list, an improvement or the reverse in this respect would mean little. At least one can say that the quality of the reproduction suffered no deterioration with the progress of the fast, but that retention was equally as good at the end as at the beginning.

The quality of the association reactions was of high grade throughout the main test (Table II). There were no senseless or pure sound reactions and very few repetitions. Synonyms, word-compoundings and misunderstood stimulus words occurred seldom and were scattered throughout the days. The word woman appears a number of times and man slightly less often. There was also evidences of a religious complex.¹⁷ An examination of the different categories did not show sufficient change to warrant an analysis or tabulation as to quality. It was thought that the introduction of words designating food might produce delayed reactions both with the word itself and with the words immediately following. This was not the case. For example on April 16th we find egg-white 1.4 sec.: on April 19th omelet-eat, 1.4 sec.: on April 21st fish-sea, 1.4 sec.: on May 7th candy-sweet, .8 sec.: on May 9th apple-fruit, .8 sec.: on May 10th, roast-meat, 1 sec.: on May 13th chocolate-sweet, 1 sec. None of these reactions were followed by unusually long reaction times. It might be of interest to mention at this point the unusually long reactions which point to complexes. On April 13th we find pulse-hand, 9 sec.: on April 21st death-eternal, 22.4 sec.: and on April 26th uncertainty-pendulum, 12.6 sec. These are the only extremely long reaction times. The next longest is 6 sec. All of these delayed reactions may be explained from the same cause. Levanzin had asserted that the chief factor for a successful fast was faith and confidence and absolute lack of fear. He thinks it is the fear combined with exposure which causes death in shipwrecks and other calamities where food is not obtainable and not the actual lack of food. We also find that those who fast frequently cover their mirrors in order that they

¹⁷ See for example the list of April 30th.

may not be disturbed by the evidences of emaciation. One of the further dangers in fasting is heart failure. If Levanzin's heart had shown alarming symptoms the fast would have been terminated at once. It does not, therefore, require a stretch of imagination to suppose that Levanzin would keep his mind from such subjects as death and uncertainty and that he would even avoid thought of the condition of his heart and that the mention of these words would cause hesitation.

The determined association reaction noun-verb was begun on the 8th day. The curve (XX) resembles that of the verb curve, except that the rise continues longer. It starts very low,—1.0 sec.—increases with rather large daily fluctuations and on the last day of the fast returns to 1.1 sec. A particularly disturbing factor in this series was the fact that there was an ever increasing difficulty to obtain appropriate words. At first the words had obvious associations. They were names of common objects, such as dog, gun, eye, etc., but more unusual words had to be employed in increasing numbers and there seems no doubt that this circumstance was at least part cause of the increasing length of the reaction time. It is even more important in the determined than in the free association experiments to have the quality of the words the same and not more difficult. For long series of tests the free-association experiments are much to be preferred.

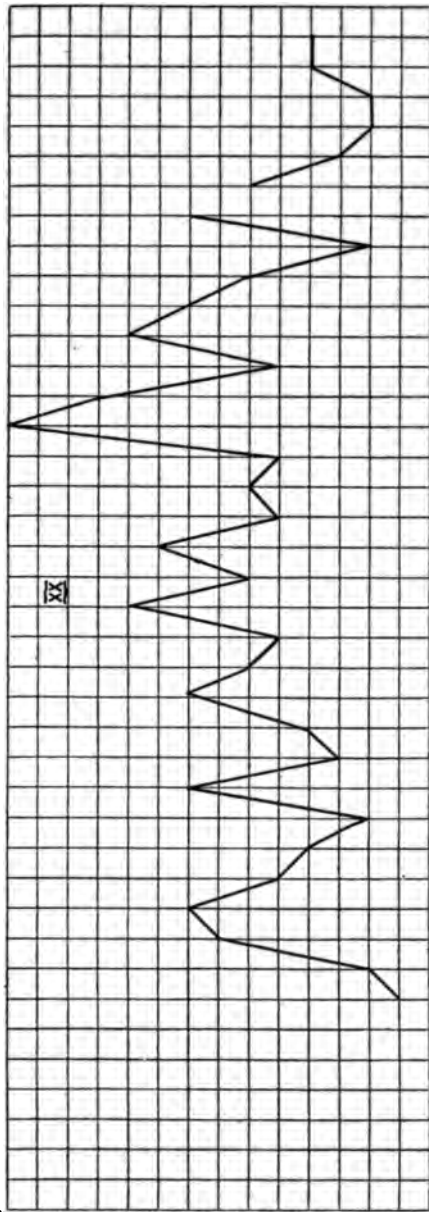
Cancellation Test

Special forms were made for this test consisting of type-written pied text of 100 a's and fifty of each of the other letters of the alphabet. A different combination was made each day so that the subject should not become accustomed to the order. Levanzin was requested to cancel all the a's. He used his left hand and the time was taken with a stop-watch. Special care was observed to have the illumination constant and the same pencil was employed.

The curve (XXI) represents the time for the completion of the task. As in some of the other curves so here we have the initial

secs.

2.3
2.2
1.1
2.0
1.9
1.8
1.7
1.6
1.5
1.4
1.3
1.2
1.1
1.0

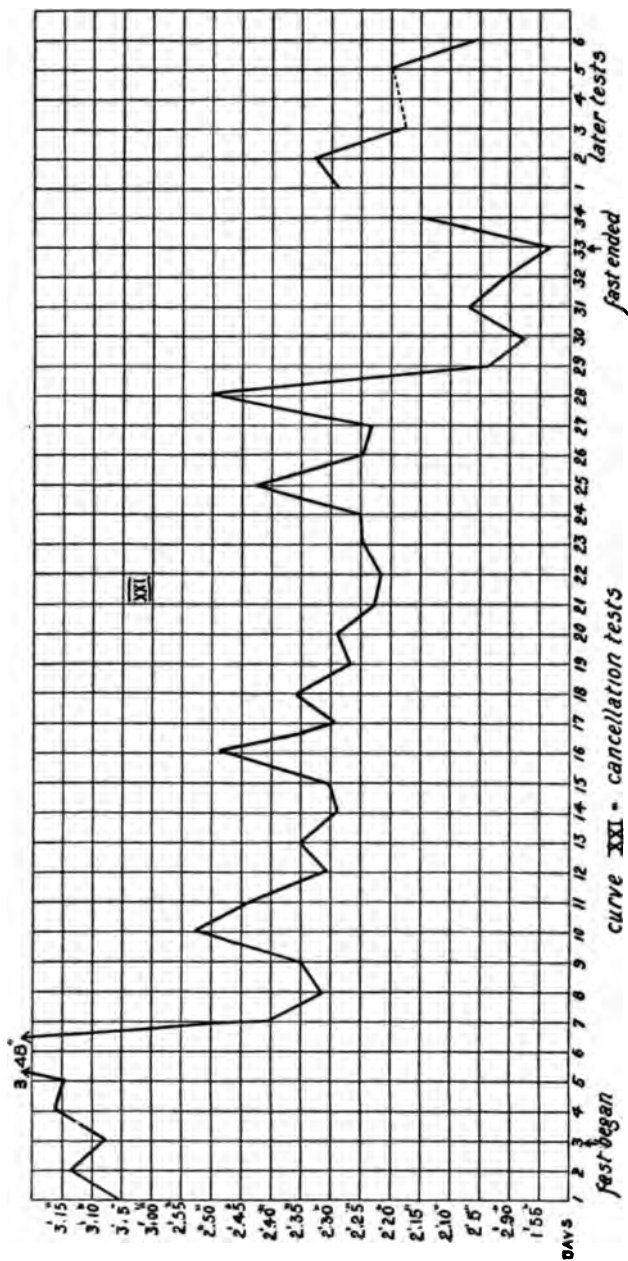


DAYS 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 1 2 3 4 5 6

curve **XX** - association reaction noun-verb

fastened

later tests



rise, which continues to the 7th day, when there is a sudden drop to a level which slopes slightly to another sudden drop on the 29th day and a very low level for the final days. The difference between the maximum of 3 min. 48 sec. on the 6th day and the minimum of 1 min. 53 sec. on the 33rd day is very considerable. The maximum is over double the minimum and even if we compare the minimum with the initial time of 3 min. 7 sec. or with 2 min. 43 sec. of the 7th day which is the first and largest practice drop, we still find a very considerable difference. There does not seem any doubt therefore, that there is very much of a betterment in the time as the fast progresses and that this decrease in the time continues to the end of the series. Nor is this improvement in time gained at the sacrifice of accuracy. At no time were there many mistakes made (see Table III). In fact the degree of accuracy was always so high that we cannot place any importance on the slight increase of accuracy in the last half of the series¹⁸ nor does the slight loss of accuracy at the minimum alter the significance of that result.

Visual Acuity

These tests were made in the large calorimeter room adjoining the small room in which the previous tests were conducted. The largest E which had been cut from the Schnellen test-card was used. This was held by the experimenter at the level of the subject's head when seated. It was illuminated by an electric lamp held by a second experimenter in front of the card and moved with it. The shades of the room were kept drawn during the experiment in order to have as far as possible constant illumination. The subject suffered from myopia and wore corrective glasses. A distance well within the threshold was first chosen at which the subject was asked to judge in what one of the four possible positions the E was being held. The experimenter put the card behind his back when he changed its position. After a few days of the tests it was thought that the subject might

¹⁸ There were 29 mistakes in the first half and 24 in the second half of the series.

be using the secondary criterion of the distance of the edge of the E from the edge of the card, the E not being exactly in the middle. The card was therefore mounted on a larger cardboard of the same color in order to obviate this possibility. On account of the surprising results both experimenters were at all times keenly attentive to the possibility of other secondary criteria, but none could be discovered. Ten trials were made at each distance, the card being moved from the observer in steps of one foot. That distance was considered the threshold which preceded the distance at which the subject made two mistakes out of the ten trials.¹⁹ The alteration in the position of the E followed no definite order, but every means was used in this respect to confuse the subject in order to remove all possibility of his guessing the position. Most of the judgments were made without hesitation, both at the very low and very high thresholds.

The curve (XII) represents the daily threshold in feet. There is a very rapid rise from the 5th to the 14th day when the maximum of 37 feet is reached. Then there follows a drop to 24 feet and a rise to 36 feet on the next to the last day of the fast. The 34th day shows a drop to 19 feet. The record of the 5th day is 16 feet which is the minimum; that of the 32nd day is 36 feet, which is one foot less than the maximum. This difference of 16 feet is very great for visual acuity. He sees twice as far at the end of the fast as he does at the beginning.

Later Tests

Owing to an attack of colic resulting from the nature of the first food taken after the fast and the subsequent withdrawal from the laboratory it was impossible to continue the tests during the recuperative period, as had been planned. Only by later tests for comparison could a decision be reached as to the efficacy of fasting. One year after the tests just described Levanzin volunteered as subject for a short series of tests. These were conducted at the Harvard Psychological Laboratory and ex-

¹⁹ Time prevented the threshold being taken in the reverse direction. The tests took five to ten minutes.

tended over a period of six consecutive days. It was not possible to arrange for them to take place at five o'clock as previously and ten o'clock in the morning had to be chosen. All the other conditions were observed as closely as possibly. The same tests with the exception of the visual acuity test and the hand-writing test were performed. Levanzin seemed in good health. He weighed about 126 lbs. which is somewhat less than he weighed when he began his fast. His physical appearance was, however, very much the same as on the day he arrived at the Nutrition Laboratory. He had remained in America during the previous year, engaged in medical studies, lecturing, etc., had not fasted again and had had no illness during that time. In coming to the laboratory he made a journey of four miles and had already had several hours work, having risen each day at five o'clock, exercised for half an hour and made several visits. The conditions previous to the tests are, therefore, hardly comparable to those of the former series. It is evident, however, that he was as strenuous if not more so than he had been up to the later hour of five o'clock of the previous tests.

The rote memory for digits (IIA) was somewhat poorer than it was during the latter part of the long series. It did not reach the maximum by two numbers, yet it did not show any poor scores. The curve (IIB) which represents the first mistake, or, as it was supposed above, the state of attention, shows an improvement over the latter part of the first series in that it does not drop as far. On the other hand the rote memory for words (IA) seemed as good if not better than during the fast. It reached the former maximum on the fourth day and never dropped below eight words. The memory after fifty-five minutes (IB) was as good as the immediate memory. From these results it may be concluded that the memory is still, after the year's interval, at about the level that it was at the end of the fast.

The curve (III) for the tapping begins considerably higher than the maximum of the fasting tests and although it drops somewhat, still it remains above the former maximum. The drop in the difference curve (IV) is caused principally by a

falling off in the initial spurt. This is concluded from the fact that the results of the last ten seconds vary much less than those of the first ten.

The results of the first day of the tactual space threshold cannot be used as a comparison (XI). The unusually high threshold was undoubtedly caused by inattention on the part of Levanzin, who admitted that he had been very much worried over an appointment he had been forced to miss and upon which his mind had been during these tests. Apart from this day the curve has the same form it had during the latter part of the previous trials. The second and third days show the minimum, which was last reached on the 30th day of the former trials.

The dynamometer used in the previous tests could not be obtained until the second day. There are, therefore, only five records. The curves for both the right (VII) and the left hands (V) begin with very high records and drop considerably on the second day, just as they did in the former series. These first records are very much better than any made in the previous trials. Even after the drop the right hand twice surpasses the previous maximum and remains close to it on the other days. The difference curves (VI, VIII) show that on the first day the high record for the left hand is made by a sustained effort. The right hand spurt causes fatigue toward the end. The large differences during the next three days for the left hand are caused by spurts followed by fatigue, that of the right hand by fatigue. It is seen that the strength of the muscles of the hand have very much increased since the end of the fast and judging from the first day's results is much greater than at the beginning of the fast. One acquires a knack in gripping the instrument and it may be that this is carried over from the former tests and makes these initial records higher than those of a year ago. In other words some of the effect of practice is still present and influences the results much more than it did when it had the opposing effect of fatigue.

The free association reaction time (XIII) begins at the low point of the last day of the previous series, on the third day it

reaches the shortest time of that series and again on the fifth day and on the last day it falls almost one-fifth of a second below this point. That is the curve continues the descent it began in the middle of the former series in as regular a manner as if a year had not intervened. Inasmuch as some practice is necessary after so long an intermission, it may be said that the reaction times are better than they were at the end of the fast. The m.v. (XVIII) was .5 sec. on the first day and .15 sec. on the sixth with an almost steady decline.

The average reproduction time (XIX) is .9 sec. for all the days. This is very low and although .8 sec. was reached three times in the former tests, it is safe to conclude that the reproduction times are at least as good as they were at the end of the fast. In fact the average for these days is better than for any six consecutive days of the previous tests. There was only one false reproduction and that was "wrong" for "bad." In view of the fewness of the trials little would be gained by an analysis of the results according to categories (XIV, XV, XVI, XVII). The noun and adjective curves are lower than the verb and abstract curves. The quality of the reactions is about the same. Evidence of a religious or mystic complex is as plain here as in the previous results. "God" was the reaction for "adore," "worship," "unseen," "mercy," "Divine," and "Infinite;" "supreme" gave "Being," "sacred" gave "church," "adorable" gave "saint," "life" gave "eternal," and "ornament" gave "church." There were no very long reaction times. In connection with the previous complex it may be mentioned that death was the reaction word for fear.²⁰

The reaction noun-verb (XX) begins at the average of the 32d day of the former series and on the third and fourth day reaches the minimum of the next to the last day of the long series. The average of these days is very much better than that

²⁰ It was thought that a year's intermission would make the old lists equivalent to new ones and as one would then be sure of having the lists of this series of the same quality with those of the former, the old lists were used on the first day, but seven of the twenty reactions were the same as those made a year ago, so that new lists were made.

of the last days of the fast series or even of the first days so that there is no doubt of an improvement in these reactions.

The cancellation test (XX) begins at about the point of the 27th day and the time gradually decreases, but at the sixth day has not reached the rapid time of the 33rd day. Judging from the slope of the curve one would expect it to do so shortly, however, so that one can conclude that the mental functions necessary for this test, are in about the same state they were at the end of the fast. There were only six mistakes, four of them being on the first day.

Correlations

It would be supposed that there should be very good and very poor days upon which all the curves would show proportionate increases or decreases or that at least similar tests, such as those of the higher mental-processes, would show similar variations. If we compare some of the crests and valleys however, we arrive at negative results. For instance, on the 22d day the association time (XIII) is long and both memory curves (IA, IIA) are in a valley, but the cancellation test (XXI) shows improvement, and the reproduction times (XIX) are not long. On the 16th day the left hand reaches a maximum in the strength tests (V), but the right hand (VII) shows no such result. Tapping (III) rises on that day, but it is still comparatively low, one memory curve has fallen (IIA) and the association time (XIII) has risen. On the 15th and 17th days the memory curve (IIA) is at a maximum and association time (XIII) is also lower, the cancellation test (XXI) is also low on these days, but the maximum of the memory tests (IA, IIA) on the 31st day finds the association times (XIII) longer. On the 12th day the curves for the strength tests (V, VII) have risen for both hands—it is the maximum for the right hand—the time for the cancellation tests (XXI) has shortened and memory (IA) is better, but the tapping record (III) has fallen and both association (XIII) and reproduction times (XIX) are at a peak. The considerable lengthening of the time of the cancellation

test (XXI) on the sixth day finds a betterment in most of the other tests, the tapping test (III) indeed, having reached its maximum on that day. The visual acuity curve (XII) rises abruptly to its maximum on the 14th day and although with a few exceptions the curves show a slight betterment, the rise is comparatively insignificant.

It must be concluded, therefore, that with the exception of this last day the daily fluctuations cannot be traced to any one cause such as a general bodily fatigue and depressed mood or vigorous and cheerful mental states, but that either there is a change in the one or more processes essential to the particular test that is showing the exceptional rise or fall or that there has been a momentary wave of fatigue or distraction or spurt, etc. A diary of the fast was kept in which every important incident was noted and it is possible that many of the fluctuations in particular curves or changes in general tendencies of several of the curves could be more or less satisfactorily explained. The following considerations, however, make such explanations of doubtful value. One cannot say in advance what the effect of visits or other changes in the general routine may be. Much depends upon the particular circumstances. Now if the results were better after a certain visit one could say that the subject was in a pleasant mood after the break in the monotony of the days and that his mind had been stimulated by agreeable conversation. If the results were worse on those days one could say with equal weight that the fatigue following the unusual exertion was the cause. Only the most reliable introspection on the part of the subject before and after each test could have given strength to such explanations and both the lack of time and training on the part of the subject made such a procedure impossible.

It did seem possible, however, to make an exception of the days that Levanzin took a drive or was allowed on the roof and that if the curves showed an agreement in their fluctuations on these days an unequivocal explanation could be found. The drives were taken on the 14th, 17th, 20th, 22nd, 24th, 29th,

31st and 32nd days; the visits to the roof on the 11th, 15th, 21st and 30th days. As was stated above there was no general agreement even on these days. In regard to the individual curves, however, the visual acuity curve seemed to show the influence of the drives. The best result in the visual acuity test was made on the first drive day and the curve always ascends on the drive days, although not always to a peak. It falls, however, on all but one day when a visit was made to the roof and that it rises on the drive days is contrary to what one would expect and is difficult of explanation, since the subject's eyes should if anything have been fatigued by the increased light. If there had been a stimulation of the central processes causing a heightened power of discrimination, this ought to have influenced the other curves as well.

General Summary and Conclusions

The fact that a human being could live for a month or longer without food had already been satisfactorily proven.²¹ In recent years Merlatti is reported to have fasted for fifty days and Dr. Tanner for forty days. The fast of Succi²² is most similar to that of Levanzin in that it was undergone for about the same length of time and under similarly strict scientific control, although never before had quite so many precautions been taken as in the case of Levanzin. Succi fasted for thirty days, but took pepton on the 27th day. Levanzin continued for one day

²¹ E. Bardier in his article "La Faim" (Ch. Richet's *Dictionnaire de Physiologie*. Vol. 6, p. 3) remarks in regard to voluntary and involuntary fasts: "on pourra se soumettre volontairement à un jeûne prolongé, comme l'expérience en a plusieurs fois été tentée, et endurer assez facilement les souffrances de la faim. Le besoin de manger sera d'autant moins douloureux, d'autant plus facile à supporter qu'il suffira d'un signe pour être mis en face d'un succulent repas. Au contraire, la faim sera beaucoup plus pénible ses manifestations beaucoup plus douloureuses, si l'on se croit dans un naufrage, dans une expédition,—voué à une inanition complète sans espoir de salut." On page 6 in reference to forced fasting he further says: ". . . la lutte que l'on est obligé de soutenir contre les causes mêmes de cette inanition augmente la sensation de faim."

²² *Das Hungern*, by Luigi Luciani. Translated into German by Dr. M. O. Fraenkel. 1890.

longer, absolutely nothing but 750 cc of distilled water passing his lips during that time. Both men remained in good physical condition throughout and seemed at no time to suffer any unusual discomfort. It was with difficulty that Levanzin was persuaded to discontinue his fast on the 31st day. Although Luciani doubted that Succi was mentally normal, general observations and the tests pointed to a sound mind in the case of Levanzin. Both men were, naturally, men of great determination and above all of implicit faith and confidence in their idea. Levanzin believed fasting to be a panacea for all ills and the very fact that he is of that type of man who can narrow his horizon about an idea and stubbornly resist all invasions, gave him the best equipment for the fight against the natural demands of the flesh. Such a type of mind cannot be called abnormal, although it is unusual. The feeling of hunger was at all times even during the first stages of the fast denied by Levanzin. This statement should not be disbelieved even though the general experience of most men is extreme discomfort, which those who fast tell us only disappears after the second or third day as in the case of Succi. With Levanzin and perhaps with other fasters this feeling of hunger may have been suppressed from the beginning by autosuggestion. The fact of the deep ingrained faith in the fast makes this plausible.²⁸

The condition of Succi's higher mental processes was only ascertained by general observation. These agree with those upon Levanzin. There was at no time any symptom of hallucination or lack of clearness in the thought processes. Luciani writes:

²⁸E. Bardier, in criticising Bernheim, writes: "Au sens où l'entend Bernheim, les jeûneurs qui se soumettent à l'inanition résistent facilement, tout simplement par le fait d'une auto-suggestion. Discutant en particulier le jeûne de Cetti, il admet que ce dernier-tout en n'étant pas un hystérique—s'est suggestionné. Il demeure convaincu qu'il conservait toute sa force physique, 'cela suffit pour réaliser le phénomène; l'idée fait l'acte; il s'exalte, il s'entraîne, il se nourrit de son idée, il se montre avec complaisance à ses visiteurs, il jouit de son triomphe; l'esprit domine le corps; etc.'..... Le jeûneur, par sa volonté, arrive à résister à l'habitude de manger; il obéit à sa conscience qui le somet à l'abstinence, mais certainement sa volonté doit être incapable de provoquer la suppression d'une sensation." Op. cit. p. 10. See also footnote p. 3.

"Am 13 Hungertage wollte ich seine Ausdauer bez. geistiger Anstrengungen auf die Probe stellen, indem ich ihm schwierige oder unlösliche metaphysische und theosophische Fragen vorlegte und beständig Einwürfe gegen seine Antworten erhob, in der Absicht, seinen Verstand zu ermüden. Ich muss gestehen, nicht bemerkt zu haben, dass sein Geist dabei mehr ermüdete als der jedes andern Sterblichen von gleichem Bildungsgrade und gleicher Begabung, wenn man ihn solchergestalt martert."²⁴ Levanzin is a man of a much higher level of intelligence and intellectual training than Succi. At all times during the fast he was very eager to enter into discussions upon abstract subjects such as the value of the Esperanto language, the political conditions in Malta, the possibility of mental telepathy and theories of spiritism as well as the value of fasting. It could not be observed that there was any diminution of his argumentative powers or lack of lucidity of expression. When aroused to counter argumentation he showed the same energy in reply at the end as at the beginning of the fast.

Succi's muscular strength as well as his sensory acuity was ascertained in a manner somewhat similar to the method employed for Levanzin and the results will be compared in the following summary and interpretation of results:

1. In the Dynamometer tests made upon Succi it is impossible to tell from the text how many trials were made daily. As the curves for the ten trials and for the initial trial for Levanzin are similar, the ten trial curve will be considered. It is safe to assume from lack of mention of the fact and from the nature of the curves that Succi was right handed. It will therefore be necessary to compare the curve of the right hand of Succi with that of the left hand of Levanzin.

It will be remembered that the strength of both hands was found to increase after the drop on the second day until the right hand (VII) reached its maximum on the 12th day and the left hand (V) on the 16th day, both curves then dropping steadily from this point, the right, however, less than the left,

²⁴ Op. Cit. Pp. 68-69.

for the left reached a minimum on the 31st day, while the right during the fast never dropped as low as the record of the 19th day. There is a very striking similarity between them and Succi's tests.²⁵ Both of Succi's curves also drop after the first trials and then rise again, his left reaching a maximum on the 14th, his right on the 20th day, as compared to the 12th and 16th days of Levanzin. Succi's curves then drop also, but the left drops more than the right which is the reverse of Levanzin's curves. With Succi both maximums are greater than the first day's records while with Levanzin this is only the case with the left hand. This agrees, however, with Levanzin's records for the initial daily trials (IX, X). Further Levanzin was able to make a spurt at the end of the fast with both hands, this spurt extending through several days. Succi was only able to spurt with one hand and that on the last day, the curve for the other hand remaining stationary.

Luciani attributed the rise of the curve alone to autosuggestion. It seems quite probable, inasmuch as both men believed that their strength would be increased by the fast, that this idea strengthened their determination and that they bettered their results by sheer "will power."²⁶ There is, however, another possibility which may be assumed without denying the influence of autosuggestion and that is that at least in the case of Levanzin, who was unused to such tests, the coördination of the muscles became gradually more perfect and further that these muscles, which were being exercised daily increased for a time in strength as they would have done under normal conditions, but in this case possibly to the detriment of other muscle groups. In both cases with both hands fatigue gained the ascendancy over practice effect and possibly over autosuggestion about the middle of the fast, causing the curves to drop. In the case of Levanzin's unpracticed hand, however, the effect of

²⁵ Op. cit. p. 55.

²⁶ E. K. Strong, Jr., in his paper entitled "The Effect of Various Types of Suggestion upon Muscular Activity" (Psych. Rev. 1910 Pp. 278) says: "The auto-suggestion tends most strongly of all the types of suggestion to heighten the maxima."

practice had more room to work and held the curve up longer than in the case of the practiced hand.

2. The tapping test (III) is also influenced by the condition of the muscular tissue, but there is another factor more essential here than strength and that is the reaction time. As in the strength tests so here there is a rise at first, but here it is of much shorter duration, the maximum of 215 taps in 30 seconds being reached already on the sixth day. The following considerable drop until the 15th day, at a time when the strength tests are showing more efficiency, may possibly be caused by a lessening in the interest for this test.²⁷ About the middle of the series this interest and increased effort for a good record may have returned, judging from the results, but fatigue had by that time set in and the curve, although rising until the last day is never quite able to reach the maximum of the sixth day; that is, there was some falling off in the rapidity of reaction, which judging from the results of the strength test is due rather to a change in the muscle tissue than to a change in the nervous arc.²⁸ From what we know of the effect of practice in such tests it is most probable that if it had not been for this increased muscular fatigue the curve would have reached an appreciable maximum at the end of the series. From the fact of the very

²⁷ See pp. 7 and 9.

²⁸ As the tapping tests preceded the strength tests the objection can not be raised that the hand was being unusually fatigued by these latter tests.

In reference to the tapping test under normal conditions Wells writes that "The objective fatigue phenomena which we note in the test are in all probability fatigue phenomena in the refractory phase or a lowered efficiency of coördination, equally a product of altered synaptic conditions; the sensations of fatigue on the other hand, may with squal assurance be ascribed to tissue changes within the muscles that take place as a result of their continued effort." (F. L. Wells. *Normal Performance in the Tapping Test before and during Practice, with Special Reference to Fatigue Phenomena*. *A. J. of Psych.* 1908, p. 473.) In the above tests the change in muscular tissue is due to emaciation, a fact that does not play a rôle in the test to which Wells refers. At no time did Levanzin speak of sensations of fatigue and judging alone from his facial and bodily expressions there is no data from which to assume that they were greater at the end than at the beginning of the fast. As to the synaptic conditions there is nothing in the test to point to a change.

small difference between the average of the first ten and last ten seconds on the sixth day when the maximum was reached, as compared with the great difference in the almost equally good result of the last day, it is evident that on the first day the good performance of the first ten seconds practically continues throughout (in both instances the best record was made during the first ten seconds) while on the last day the effect of practice as shown in the initial performance was counterbalanced toward the end by fatigue.²⁹ These results seem to cast further doubt upon Luciani's hypothesis of autosuggestion in the strength test, for surely autosuggestion should play as great, if not a greater, rôle in the tapping tests during those days in which according to the strength tests it would have to be assumed at work. The results of the tapping test are indeed directly opposed to such a theory.

To sum up it may then be said that although initial lack of interest and later muscular fatigue played a rôle, both factors being directed toward a decrease in the amount of work, yet the nervous impulse toward the end was sufficiently great to bring the curve back to its initial level and almost to its maximum.

3. The threshold for tactual space perception (XI) decreased somewhat as the fast progressed. It was on the average much better during the last half than the first half of the series. Similar tests were made upon Succi upon a number of different parts of the body, but only on three days, before the fast, on the 15th day and on the 29th day. On some parts of the body there was an increase on other parts a decrease. Luciani believed the difference in the three days due to differences in degree of attention. On that part of the body corresponding most closely to the spot used in these tests i.e. the lower third of the volar side of the forearm, there happened to be a rather large decrease in the threshold, the three thresholds being respectively 16, 11 and 10 mm.³⁰ Authorities differ as to whether practice lowers

²⁹ Wells writes "the true practice gain is one mainly in the initial efficiency of performance, as distinguished from the warming up gain, which shows itself chiefly in continued efficiency of performance." *Op. cit.* p. 478.

³⁰ *Op. cit.* p. 64.

the threshold in tests performed under normal conditions. Dresslar⁸¹ for example, found that practice had a considerable effect. Solomon⁸² found that if the subject is not informed of his errors there is no effect of practice. In the above tests the subject was never told of his mistakes and "vexier" trials were introduced at frequent intervals and in no special order, yet there was a lowering of the threshold. This may and probably is due to several causes. A physiological cause would be a decrease in the fat thus exposing the nerve endings and making them more sensitive. On the psychological side increased attention, which we find indicated in other of the tests would lower the threshold for discrimination. Also as the tests progress the image of the criterion used becomes cleared. From what is known of the process of perception, this is a most important factor in explaining the above effect of practice. The physiological change is the only one which could be attributed unequivocally to the fast. The central change occurs in series under normal conditions.

If, as has been often assumed, the tactual space threshold test is a measure of mental fatigue, then it must be concluded that there is no indication of such fatigue during the fast.

4. The visual acuity (XII) showed an astonishing betterment. From 17 feet as the distance of clear vision for the particular test card employed, the curve ascended rapidly to 37 feet on the 14th day and, although there is a falling off, 36 feet is the record for the last day of the fast.

If it were not for the maximum of 37 feet midway in the series, the improvement would be comparatively a steady one. One explanation that suggests itself is that the possible change in intra-ocular tension caused the eye-ball to change its shape. Unless his glasses were not the proper ones for him, however, a change in the eye should cause more rather than less difficulty as long as he wore his glasses. Further the suddenness of the

⁸¹ F. B. Dresslar. *Studies in the Psychology of Touch*. A. J. Psych, pp. 313-368. 1894.

⁸² L. M. Solomons. *Discrimination in Cutaneous Sensations*. Psych. Rev. p. 246-250. 1897.

rise seems to vitiate such a theory. A satisfactory explanation seems difficult to find. It might be said that the 37 feet record was made by chance. This also seems precluded by the fact of the number of previous steps in which 10 correct answers were given and from the evidence of confidence displayed by the subject.⁸³

Succi's eyes were examined with the ophthalmoscope and his acuity measure before the fast and on the 15th and 28th day of the fast, but no change was detected.⁸⁴ If Levanzin had happened to be measured on the third, sixteenth and one of the days toward the end of the series only, the change would have been thought as negligible as in the case of Succi. In all such tests where the daily fluctuation is considerable three tests in a month are not sufficient upon which to base a judgment as to the change in sensory acuity or higher mental processes.

5. The rote memory for digits (II) showed very little change. There is a slight suggestion of improvement during the first half of the series. Judging from the curve which indicates the point at which the first mistake was made (IIB), one can say that there was a gradual improvement in this respect, especially in the first half of the series, which is probably in part due to a betterment in the perception of the spoken word, but especially to an increase in attention, it becoming more sustained as the fast progressed. The rote memory for sense words (IA) showed a greater improvement than did that for digits. Here probably the practice effect consisted in the forming of associations between the words. The most marked improvement of all is in the retention after a longer period of time, i.e. after 55 min. (IB). This is probably also due in part at least to the more frequent forming of associations. In addition the repeti-

⁸³ The subject did not know whether he was right or wrong or how many correct answers constituted a threshold, so that the results could not have been prearranged by him. And if they could have been he would not have allowed such a good record already on the 14th day. The high threshold on the last day is obviously due to his unusually poor physical condition (when if at any time one might be justified in speaking of a lack of effort).

⁸⁴ Op. cit. pp. 66-67.

tion of the same task through so many days undoubtedly strengthened the determining tendency, i.e. the determination taken at the time of memorizing for the words to appear in consciousness again, it remaining either in consciousness or sub-consciousness during the interval. According to Levanzin's statement his mind did not revert to the task within the hour.

Experiments upon memory under normal conditions also show the effect of practice as evidenced by an appreciable increase in the memory span which may continue for a period of two months.⁸⁵

6. The Cancellation test, (XXI) which employs to a greater degree the higher functions of perception and attention shows the greatest improvement of any of the tests used. This improvement continues from the sixth to the last day of the fast. The accuracy is so high throughout the series that the slight improvement in the latter part of the tests is of no significance. Experiments have shown that fatigue affects the accuracy, so that again we have evidence against an increase in mental fatigue.⁸⁶

Besides an improvement in the above named functions, the increase in visual acuity may have been a factor in the results. On the other hand, from the results of the tapping-test and strength tests one must conclude that the betterment is in no degree due either to a betterment in reaction time or motor ability.

7. The free association time (XIII) is on the whole shorter during the latter part of the series. If it were not for a rapid drop in the middle of the curve after a rise similar to that in the

⁸⁵ T. L. Bolton, *The Growth of Memory in School Children*. A. J. of Psych. 1892, pp. 362-380.

G. Müller & F. Schumann. *Experimentelle Beiträge zur Untersuchung des Gedächtniss*, Zeit. für Psych. 6. 1894, pp. 81-190, 257-339.

W. H. Winch. *The Transfer of Improvement in Memory in School-Children*. B. J. of Psych. 1908, pp. 284-293.

⁸⁶ B. Bourdon. *Observations Comparatives sur la Reconnaissance, la Discrimination et l'Association*. Rev. Phil. 1895, pp. 153-185. A. Binet. *Attention et Adaptation*, Année Psych. 1900, 6. Pp. 248-404. C. Ritter. *Ermüdungsmessungen*, Zsch. für Psych. 1900, pp. 401-444.

tapping-test the improvement would be comparatively steady. The minimum of 1.3 sec. is reached on the day before the last day of the fast and should be compared rather with the 1.9 sec. of the third day than with the 2.5 sec. of the first day, when Levanzin was unaccustomed to the manner of reaction. Even when this comparison is made it is seen that the improvement is considerable. A separation of the curve into four curves corresponding to the four categories used, made a more minute analysis possible. All the curves (XIV, XV, XVI, XVII) show fewer high averages in the second half of the series, but it is only in the abstract curve and in less degree in the adjective series that there are more low averages in the second half of the curve. In fact in neither of the other two curves is the lowest average of the first half of the series again equalled. This seems to indicate that the betterment in the general average of the twenty words is principally due to a betterment in the reaction to abstract words. It is to be expected that the most difficult associations would show the greatest practice effect. In the noun and verb curve there is an almost steady rise in the middle of the curve corresponding to the rise in the middle of the main curve. It seems plausible to suppose that there is here as in the tapping test a falling off of interest and that this would manifest itself more readily in the easier tasks, where the reaction is likely to become more nearly mechanical.

The general improvement is also seen in the decrease in the variations of the reaction times. In all four curves the daily variation is much less in the second half of the series. Parallel with this is the decrease in the variations within each day, as is shown by the decided drop in the m.v. curve (XVIII).³⁷

Although the improvement in the reproduction time is not as great as in the association time yet it is noticeable, the average of the second half being lower than that of the first, although

³⁷ Wells conducted long series of association reactions with normal subjects and for all of them found an improvement in the reaction time. "Practice Effect in Free Association," *Am Jour. of Psych.* 1911. Vol. 22, pp. 1-13.

the very low time of .8 sec. was made on the second day as well as during the second half of the series.

The quality of the associations was good throughout, (Table II) and showed no striking change.³⁸ The reproductions were so nearly perfect from the first that nothing can be said in regard to them to support the results of the memory tests. One might add, however, that neither do they contradict those results.

The controlled reaction noun-verb (XX) shows an increasing lengthening of the time until almost the end of the series. It is quite probable that this was caused by an increasing difficulty in the stimulus words selected, a factor which could not well be avoided. No other reason suggests itself why these reactions should have taken a different course from that of the free association tests.

The present methods of testing mental capacity unfortunately do not permit one to make dogmatic statements as to the results of any such tests. In each one a number of functions are involved any one of which may have produced the variations which occur. For example in the cancellation test there is involved among other things attention and interest, apperception and discrimination, nervous impulse and motor discharge. When, however, as here, a set of tests are employed in which the same

³⁸ W. Weygandt's results are hardly comparable to those obtained in these tests (Ueber die Beeinflussung geistiger Leistungen durch Hunger, *Psych. Arbeiten*, 4, pp. 45-173). His subjects fasted for periods of only twenty-four and forty-eight hours at a time. This intermittent fasting seems to cause a much more pronounced disturbance to the organism than a prolonged fast. That there was greater exhaustion seems to be indicated by the fact that there was an increase in the associations by sound. He also finds that there was an increase in the outer as compared with the inner associations. (It is now admitted that such a classification of reaction words cannot be made without introspection.) Weygandt also found memory to be effected. The association time was not altered. Aschaffenberg studied the effect on association reactions of the exhaustion produced by a night's work without food or sleep. (*Studien ueber Associationen*, II Teil. Die Associationen in der Erschöpfung. *Psych. Arbeiten*, 2, pp. 1-83.) He too found a similar decrease in the quality of the reaction words. "Mit der Zunahme der Erschöpfung wirkt die zugerufene Vorstellung immer weniger durch ihre Inhalt; an dessen Stelle bestimmen der Klang und die Tonfarbe die Reaction."

functions are more or less active and they all show a similar trend, then a conjecture along general lines seems legitimate. And further when there is a very decided difference and it is known that a certain function is of prime importance then one is undoubtedly justified in ascribing the outcome of this test to changes in this function. It is desired to make it plain that no exact measurement is claimed, but merely that it has been possible by means of a number of selected tests to sketch an outline picture of the condition of Levanzin's psychophysiological organism.

It will be remembered that the tests range from those involving principally the muscle groups to those depending in a higher degree upon central factors. The tests depending most on the muscular reactions i.e. the strength test, showed a falling off. The tapping test which also involved the muscles but in which the rapidity of reaction was a more important factor showed no improvement. As soon as one turns, however, to the sensory discriminations one notices an increased efficiency, which is probably due either to a change in the peripheral organs, or central processes or both. Finally all the tests involving the higher processes of attention, perception and association show improvement. *In a word there was a loss in muscular strength due probably to loss of tissue, a possible gain in sensory acuity and a decided increase in the efficiency of all the central processes.* It would be premature to say that the improvement is the direct result of the prolonged abstinence from food, in as much as similar improvement has been observed in such tests under normal conditions due entirely to the effect of practice. *It can be stated, however, with some degree of certainty that the complete abstinence from food for thirty-one days had little effect upon the higher mental functions which were able to develop through practice very much as they would have done under normal conditions.* This agrees with the observations upon the physiological conditions. It has been found that during a fast the muscle tissues are the first to suffer and the nervous tissues the last. From these results it seems that up to the thirty-first day the nervous tissues have not suffered.

These results also confirm in part the general observations made by those fasting. It is frequently stated by them that they can do better mental work. The results show that at least they can do approximately as well, and it is not at all unlikely that some can do better, for it must be remembered that there is none of that sluggishness of the mental processes directly after eating, when the digestive processes are at their height and there is also absence of indigestion and the after effects of alcohol, caffeine and tobacco. That, on the other hand as has been often claimed, they are able to do more muscular work and that their power of endurance is greater is in this case at least not true. Probably the contrast of their actual results compared with what they expected would happen to a man without food makes the result seem greater than it is. The claim that the senses are more acute has been verified as to the visual acuity. It is hardly likely that the slight difference in the tactile space threshold would have been noticed by the faster.³⁹

The question remains as to whether prolonged fasting is beneficial or dangerous to the organism. This can only be satisfactorily answered after an exhaustive physiological examination extending over a long period of time subsequent to the fast. The tests made after the lapse of a year permit, however, of some conjecture in this regard concerning those functions at least which have been discussed in this paper.

The strength test shows a great improvement over the former record. Levanzin exerted a pressure considerably greater than at any time during the long series. The record for the tapping test is also above the maximum of the previous record. The association test shows a marked improvement and the reproduc-

³⁹ Levanzin stated that the heightened sensitivity for odors made walking on the streets of Malta during his first fast positively unpleasant.

The other senses were examined in the case of Succi and no appreciable change discovered. *Op. cit.*

Whipple, *op. cit.* p. 215, in speaking of the effect of practice in the aesthesiometer test remarks that Dresslar states "this practice effect is . . . rapidly lost, being reduced very definitely within eight days and completely lost within a month.

tion is also better, especially in that it varies less, and the retention of sense words has perhaps also slightly improved. The tactual space threshold and the rote memory for sense words are about the same as at the end of the fast. Only in the case of the memory for digits and in the cancellation test has the previous maximum not been reached, but both of these results show consistently good results. It may be stated in short that after an entire year's intermission the curves continued practically from the point they had previously reached if not considerably above that point without showing that loss of practice which might well have been expected. These improved conditions are, however, not necessarily traceable directly to the beneficial effects of the fast. In regard to the association tests Levanzin has undoubtedly become still better acquainted with the English language and in respect to the strength tests it must be noted that Levanzin has exercised his muscles daily according to his report. In general he has lead a careful life paying especial attention to his diet. There is also the possible effect of climate and his new surroundings with which to reckon. Finally and most important is the possibility that there was actually a greater effect of practice in the first series than appeared in the records but that it was concealed by certain opposing effects of the fast, so that the results of the later tests may not be quite what might be supposed from a comparison of the records.

It remains, however, an undisputable fact that, according to the tests made there was no lasting evil effect of the fast, either upon muscular strength or mental activity and that from one cause or another Levanzin was, if not in better condition, certainly in as good health after as before the fast.

The author takes this opportunity to express his appreciation of the kind assistance of Dr. F. G. Benedict, Director of the Nutrition Laboratory and of the advice in the selection of the tests given by Professor Raymond Dodge. Thanks are also due to Mr. Carpenter and the other members of the Laboratory for their ever ready aid.

APPENDIX I

Dreams

As has been already stated Levanzin was asked to recount the dreams he had had during the previous night. From these records those dreams are here given which pertain to food. It will be seen that at one time he ate, at another refused food, but in neither case was there evidence of anything but a normal emotional reaction. According to the Freudian theory this absence of an intense emotional state (there were no nightmares nor anything else in the records indicative either of mental or bodily distress) means that the will ("wish") to fast was too strong to allow of any serious conflict of ideas. A great part of the dreams are of a sexual nature and are not here given.

April 13th. I saw a basket covered with a white piece of cloth, which I imagined full of food. When I tried to uncover it several black rats jumped out of it and frightened me.

I dreamed I was passing down one of our streets in Malta with a paper bag under my arm containing cheese cakes for my daughter. I found myself in a state of mental excitement and after going a certain distance I found that the lower end of the bag was opened and the cheese cakes were gone. In their stead was a white hand.

April 19th. I dreamed I was in a shop and on the counter there was a very big ham of about ten feet diameter. The proprietor was riding on the top of it with a knife in one hand. "It is a very good one," he said. I answered, "I do not like it. Do you not know I am fasting?" Then a Friar came in and said, "I will take it in his stead, because I like it." He took it and swallowed it.

April 21st. I dreamed I had been for a walk in the country. I went to a country tavern and asked for something to eat. He gave me a beefsteak and some fried red fish. I ate them with relish and asked what I had to pay. He told me \$1.50 and asked if that was too much. I said I did not think so. In coming out of the tavern I saw a river full of these red fish and people were trying to catch them. I said, "You are fishing out all the fish and if you continue you will not have any more to eat."

APPENDIX II.

April 11, 1912

Stimulus Word	Reaction Word	Reaction Time
Paper	ink	2.2
Bright	light	2.
Yellow	lemon	1.8
Table	knife	1.2
Spoon	broth	2.8
Apple	stem	2.4
Sleep	bed	1.6
Room	door	1.3
Face	eye	2.0
Carpet	red	1.8
Animal	white	2.6
Rain	noise	5.
Teach	bench	2.
Doctor	knife	4.
Book	no. of pages	3.8
Store	glass window	1.6
Horse	tail	2.2
Island	trees	2.2
Journey	ship	2.2
Freedom	banner	2.0
Sweet	sugar	1.2
Average		2.3

April 13

Stimulus Word	Reaction Word	Reaction Time
Timid	rabbit	3.0
Pulse	hand	9.0
Mystery	religion	5.2
Savage	wolf	2.4
Spirit	angel	2.4
Teeth	to eat	2.6
Bargain	profit	5.0
Blunder	mistake	3.0
Temper	nervous	2.2
Abrupt	cascade	2.0
Harp	sing	2.0
Switch	machine	2.4
Wide	sea	2.2
Tailor	stuff	3.0
Income	money	1.5
Splendor	sun	1.8
(Salve) Solve	lip	2.5
Moon	silver	2.2
Frost	white	1.8
License	wine	1.8
Average		2.9

April 12

Stimulus Word	Reaction Word	Reaction Time
Round	table	2.0
Country	green	1.8
Silver	spoon	2.3
Rabbit	white	2.0
Chair	cushion	3.0
Glass	window	2.0
Flower	odor	2.3
Sun	brightness	3.2
Bread	white	2.3
Wood	hard	3.0
Well	water	2.4
Danger	sea	2.0
Tired	bed	2.0
Watch	gold	2.4
Marble	table	1.6
Iron	bar	3.8
Bridge	iron	2.8
Blind	dark	2.4
Pencil	wood	3.0
Candy	sweet	3.4
Average		2.5

April 14

Stimulus Word	Reaction Word	Reaction Time
Accept	a reward	2.8
Air	blue	2.6
Able	sailor	2.0
Abuse	drink	2.6
Address	letter	1.8
Blood	red	1.1
Bad	man	1.4
Age	90	1.2
Agree	wife	1.0
Boot	black	1.8
(Tall) Ball	tree	1.6
Balance	weight	1.6
Amuse	theatre	1.4
Bottle	ink	1.4
Band	brass	1.4
Climate	mild	.8
Bite	dog	1.5
Box	wooden	1.6
Contents	book	6.4
Boy	small	1.8
Average		1.9

April 15

Stimulus Word	Reaction Word	Reaction Time
Catch	bird	1.6
Brain	human	2.6
Broad	street	1.4
Courage	man	2.2
Cease	speak	2.2
Brick	red	1.6
Broken	glass	1.0
Culture	physical culture	1.6
Compel	servant	3.4
Cable	iron wire	1.4
Central	station	1.6
Crowd	people	1.2
Confess	priest	1.0
Carbon	carbon dioxide	1.8
Common	sense	1.0
Day	night	2.0
Control	engine	1.0
Chain	iron	1.0
Course	study	2.2
Delegate	apostolic	2.0
Average		1.7

April 17

Stimulus Word	Reaction Word	Reaction Time
Crawl	serpent	2.0
Clown	buffoon	2.4
Dizzy	headache	1.6
Distance	my country	2.0
Cure	physic	2.6
Corn	grass	1.8
Easy	chair	1.8
Distress	sorrow	2.0
Decorate	church	1.6
Copper	metal	1.4
Even	ground	2.4
Endurance	fasting	1.4
Decline	age	1.0
Cream	sweet	2.0
Firm	strong	3.4
East	west	1.0
Degrade	man	1.8
Corset	woman	1.0
Flat	floor	1.8
End	book	3.0
Average		1.9

April 16

Stimulus Word	Reaction Word	Reaction Time
Defend	Country	1.8
Deck	ship	1.2
Fresh	air	.8
Faculty	arts	1.0
Deduct	sum	1.4
Dinner	good	1.4
Flavor	odor	2.2
Displease	anyone	3.2
Dog	large	2.0
Good	man	.6
Fault	his fault	3.0
Egg	white	1.4
Green	tree	1.8
Fright	dog	2.2
Drive	horse	1.2
Fairy	tale	1.4
Hard	stone	.8
Function	ceremony	3.2
Profess	religion	1.4
Salt	sea	1.4
Average		1.7

April 18

Stimulus Word	Reaction Word	Reaction Time
Hit	hammer	3.0
Swallow	food	1.2
Suffer	pain	1.2
Build	house	1.3
Rubber	teeth	1.4
Food	good	1.0
Park	large	1.1
Boat	swim	1.8
Smooth	floor	1.1
Straight	way	1.8
Ugly	man	1.8
Gentle	woman	1.4
Naughty	man	2.0
Power	England	1.6
Strength	athlete	1.9
Charm	woman	3.0
Cost	money	1.0
Kindness	woman	2.2
Break	glass	1.2
Jaw	mouth	1.8
Average		1.6

<i>April 19</i>			<i>April 21</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Produce	field	1.4	Pinch	pin	1.4
Cry	baby	1.0	Satisfy	appetite	.8
Freeze	cold	1.6	Nourish	food	1.2
Follow	soldier	5.8	Drift	wind	.8
Smoke	pipe	.8	Abuse	drink	1.2
Rope	long	2.0	Ditch	deep	1.2
Omlet	eat	1.4	Tiger	fierce	1.0
Cap	head	1.0	Music	sweet	1.0
Burglar	thief	1.6	Fish	sea	1.4
Delicate	woman	.8	Death	eternal	(22.4)
Thick	paper	2.8	Soft	paste	2.4
Expensive	money	1.0	Ugly	man	1.2
Dark	night	1.0	Watchful	policeman	2.6
Unfair	unjust	2.0	Indecent	conduct	3.0
Purpose	scope	1.0	Haste	hurry	1.0
Glory	eternal	1.2	Comfort	good	2.0
Mischief	bad	2.0	Adventure	strange	1.2
Occasion	accident	1.0	Practice	long	1.8
Nuisance	wrong	1.6	Untrue	falsehood	1.6
Overcoat	dress	1.0	Merit	high	2.8
	Average	1.6		Average	1.6

<i>April 20</i>			<i>April 22</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Prefer	office	2.4	Eat	bread	2.0
Crush	crowd	2.0	Open	door	1.0
Allow	pension	1.6	Divide	reign	1.8
Drink	water	1.2	Fade	flower	1.6
(Solution)	salt	2.2	Travel	ship	2.0
Salute			Umbrella	rain	.8
Hip	thigh	1.2	Gift	gold	3.0
Lightening	thunder	2.0	Man	long	.8
Parlor	bedroom	2.4	Sailor	ship	1.2
Snake	serpent	1.0	School	teacher	1.2
Wicked	man	1.2	Dense	air	2.0
Rich	millionaire	1.8	Short	man	1.4
Clean	body	1.2	Weary	travel	1.6
Bashful	woman	1.0	Best	book	5.4
True	religion	5.2	Excuse	pardon	1.6
Exchange	money	1.0	Insult	bad	3.8
Style	literature	1.0	Prudence	woman	1.6
Power	gun	1.0	Caution	wise man	2.2
Result	good	1.4	Conceit	ambition	2.2
Nonsense	foolish	1.6	Captain	ship	1.4
Seed	plant	1.0		Average	1.9
	Average	1.7			

April 23

Stimulus Word	Reaction Word	Reaction Time
Collapse	sick	2.4
Excite	nervous	1.6
Begin	book	1.8
Prosper	progress	2.4
Hat	head	1.2
Sister	brother	1.0
Ham	meat	2.0
Crime	justice	2.8
Tight	shoe	2.0
Solid	stone	1.8
Cold	winter	1.6
Clear	sky	1.4
Hope	fortune	3.6
Dismay	fear	1.6
Offense	insult	1.4
Blunder	mistake	1.0
Future	time	4.0
Insist	persist	2.4
Trap	wolf	2.0
Oblong	square	1.4
Average		2.0

April 25

Stimulus Word	Reaction Word	Reaction Time
Join	chain	1.8
Clasp	hand	1.0
Advance	pretention	2.0
Argue	discussion	2.0
Mountain	large	1.0
House	beautiful	1.4
Neck	strong	1.0
Lamb	quiet	1.2
Hero	brave	1.2
Jealous	woman	1.4
White	snow	2.0
Serious	man	1.0
Vacant	space	1.0
Fertile	land	1.0
Reason	mind	1.6
Protection	government	1.8
Solemnity	festivity	1.0
Impudence	woman	3.8
Convenience	etiquette	3.0
Scratch	nail	1.6
Average		1.6

April 24

Stimulus Word	Reaction Word	Reaction Time
Restore	furniture	1.4
Impress	printing	1.8
Flirt	woman	1.0
Ask	question	1.2
Receive	letter	.8
Baker	bread	1.0
Athlete	strength	1.0
Cradle	baby	1.0
Bundle	hay	1.0
Elephant	trunk	1.0
Cheap	money	3.0
Black	dog	.8
Tender	meat	1.4
Prompt	answer	1.4
Ignorant	man	1.0
Confidence	familiarity	2.0
Jealousy	woman	.8
Honesty	good	4.2
Unbelief	atheist	2.4
Heroism	warrior	2.0
Average		1.5

April 26

Stimulus Word	Reaction Word	Reaction Time
Forget	memory	1.2
Dislike	people	1.0
Prepare	lesson	1.0
Admire	virtue	1.8
Protect	children	1.6
Starch	white	1.2
Mutton	meat	1.4
Ostrich	feather	1.0
Roof	house	2.0
Little	boy	1.0
Funny	buffoon	2.2
Gay	sun	1.2
Dead	black	1.2
Slow	worm	1.6
Solemnity	feast	1.6
Annoyance	fly	1.0
Constancy	virtue	3.2
Attention	mind	1.4
Uncertainty	pendulum	(12.6)
Average		1.4

<i>April 27</i>			<i>April 29</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Accuse	judge	1.8	Announce	news	1.2
Appear	star	2.0	Stain	ink	1.0
Polish	wood	1.2	Finish	lesson	1.4
Repeat	lesson	1.0	Drag	horse	2.0
Condemn	delinquent	2.4	Plead	case	2.0
Car	motor	1.8	Cork	bottle	2.0
Knee	leg	1.8	Toy	child	1.2
Cloud	white	1.2	Key	door	1.2
Fun	joy	1.8	Ox	horns	2.2
Violent	wind	1.2	River	water	1.6
Sour	acid	1.0	Rusty	iron	1.6
Dim	sound	1.0	Ungracious	bear	2.0
Condition	good	1.0	Irksome	science	2.4
Deceit	deceive	3.0	Equal	balance	4.0
Fraud	wrong	3.0	Late	hour	1.2
Brutality	animal	2.0	Accusation	importation	2.0
Cup	wine	1.2	Corruption	money	2.0
Equality	fraternity	3.0	Poverty	distress	3.2
Greasy	pole	1.2	Imposition	tax	1.0
Violet	odor	1.0	Adoration	Saint	1.4
Average		1.7	Average		1.8

<i>April 28</i>			<i>April 30</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Persuade	argument	2.4	Adore	Saint	2.2
Dig	ditch	1.0	Perish	ship	2.2
Get	money	1.0	Propose	marriage	1.4
Sting	bee	2.2	Uphold	politics	2.8
Preach	priest	1.0	Descend	stairs	1.2
Spice	pepper	.8	Slave	misery	2.8
Star	Venus	1.4	Violin	music	2.0
Ice	cold	1.0	(Path) Pot	country	2.4
Picture	beautiful	1.8	Chapel	church	1.4
Lip	red	1.4	Trumpet	sound	1.2
Easy	chair	1.0	Supreme	being	1.2
Unclean	dirty	1.4	Elegant	woman	1.6
Red	rose	1.0	Impudent	woman	2.0
Rotten	mud	2.0	Blame	offense	2.4
Hard	flint	1.0	Gain	money	1.0
Proposition	geometry	1.6	Idea	noble	1.0
Improvement	progress	1.0	Worship	God	1.0
Infamy	calumny	2.2	Elevation	Spirit	1.4
Competition (competence)	commerce	2.4	Noisy	metronome	2.0
Attraction	actress	2.0	level	ground	1.0
Average		1.5	Average		1.7

<i>May 1</i>			<i>May 3</i>		
Stimulus Word	Reaction Word	Reaction Time	Stimulus Word	Reaction Word	Reaction Time
Escape	prison	2.0	Guide	a traveler	6.4
Admit	argument	2.0	Care	a boy	2.3
Joke	play	3.0	Denounce	principles	3.8
Improve	mind	1.6	Drop	stone	1.4
Defy	enemy	1.2	Suspect	fault	2.2
Lamp	fire	2.0	Saddle	horse	1.6
Cabbage	green	1.0	Sleep	bed	2.2
Paste	soft	1.2	Fog	fruit	1.0
Poem	beautiful	1.0	Skin	animal	1.4
Spear	piercing	2.6	Earth	ground	3.2
Harsh	sound	1.2	Rough	weather	1.2
Unripe	fruit	1.0	High	mountain	1.2
Unwell	sick	1.0	Idle	servant	1.4
Vile	fellow	1.0	Humble	man	2.0
Admission	employment	3.0	Active	boy	2.4
Thankfulness	gratitude	2.0	Health	good	1.4
Dishonor	bad	3.6	Aim	noble	1.8
Intimacy	friendship	1.0	Fame	vain	2.8
Revenge	fault	3.4	Shame	wrong	2.0
Least	thing	2.6	Ability	great	1.2
Average		1.9	Average		2.1

<i>May 2</i>			<i>May 4</i>		
Stimulus Word	Reaction Word	Reaction Time	Stimulus Word	Reaction Word	Reaction Time
Deny	favor	2.0	Fast	long	1.4
Burn	fire	1.6	Dream	sleep	2.6
Paint	wall	1.8	Taste	food	1.2
Betray	faith	1.2	Cook	food	1.4
Dress	clothes	1.4	Mark	ink	1.0
Mouse	black	2.0	Sparrow	bird	1.0
Barn	corn	3.0	Foot	large	1.6
Song	beautiful	1.4	Spider	insect	3.2
Spider	feet	2.6	Forest	trees	1.0
Scarlet	fever	1.6	Stone	heavy	1.0
Beautiful	woman	1.4	Purple	color	1.0
Yellow	fever	1.8	Infamous	calumny	1.2
Modest	girl	2.0	Refined	art	1.2
Wealthy	man	2.0	Ungracious	bear	1.8
Justice	right	1.4	Center	circle	1.6
Trouble	bad	2.0	Awkward	gait	1.8
Quantity	large	1.6	Supremacy	authority	2.0
Reproach	fault	1.2	Constancy	perseverance	1.6
Energy	force	2.0	Time	quick	1.2
Crack	nuts	1.0	Gin	bad	1.0
Average		1.8	Average		1.5

<i>May 5</i>			<i>May 7</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Invite	guest	1.6	Wash	clothes	1.0
Pin	clothes	1.4	Elevate	thought	1.4
Tremble			Deceive	wrong	2.6
(Crumble)	bread	1.4	Ramble	about	1.6
Attack	enemy	1.2	Decay	reign	1.8
Wood	hard	2.0	Bible	holy	1.4
Dirt	nasty	3.2	Pencil	lead	1.0
Shoe	tight	1.2	Crown	king	1.0
Camp	large	1.8	Goat	milk	1.2
Cannon	big	2.6	Candy	sweet	.8
Ashamed	fault	1.4	Restless	not quiet	2.0
Unsafe	war	1.6	Simple	countryman	1.6
Raw	fruit	2.0	Reckless	man	1.2
Smooth	ground	1.2	Eternal	life	1.2
Fortune	money	1.4	Prosperity	fortune	1.0
Disdain	angry	2.0	Jealousy	woman	1.2
Refinement	art	1.8	Concealment	to hide	2.4
Activity	work	1.2	Advancement	progress	.8
Accident	misfortune	1.6	Rancid	butter	1.4
Scoff	offender	2.4	Honesty	good	1.0
Noisy	clock	2.0			
	Average	1.8		Average	1.4

<i>May 6</i>			<i>May 8</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Dishonor	sin	2.4	Deserve	merit	1.2
Remove	furniture	1.6	Wish	fortune	2.4
Injure	sword	2.4	Boast	glory	3.2
Plunge	water	1.0	Establish	manufactory	1.1
Murder	thief	1.4	Barber	razor	1.6
Garden	flower	1.0	Pebble	stone	1.4
Nut	crack	2.0	Heart	beat	1.2
Stem	heraldry	2.0	Machine	work	1.4
Crab	animal	2.0	Statue	marble	1.2
Pickle	burning	2.0	Certain	thing	2.0
Noble	gentleman	1.6	Natural	régime	1.8
Nice	fellow	1.2	Correct	grammar	2.0
Secure	keys	1.0	Dusty	street	.8
Blue	sky	2.0	Enormous	building	1.6
Swift	sparrow	1.4	Commandment	God	1.0
Disgrace	fault	2.0	Excitement	nervous	.8
Security	policeman	2.2	Restoration	food	1.6
Unhappiness	marriage	2.8	Density	mercury	1.8
Rhyme	poetry	1.0	Infirmity	sickness	1.8
Disaster	Titanic	1.2	Return	voyage	1.6
	Average	1.7		Average	1.6

<i>May 9</i>			<i>May 11</i>		
Stimulus Word	Reaction Word	Reaction Time	Stimulus Word	Reaction Word	Reaction Time
Paper	write	1.2	Plunge	water	1.0
Bright	sun	.6	Guess	enigma	2.4
Yellow	fever	1.4	Rescue	wrecked	1.8
Table	mahogany	3.2	Believe	God	1.4
Spoon	food	1.4	Carve	wood	1.0
Apple	fruit	.8	Door	house	1.8
Sleep	night	2.4	Barley	corn	1.0
Cut	animal	1.8	Eagle	bird	1.0
Face	beautiful	1.2	Chin	face	1.6
Carpet	ground	1.4	Pulse	beating	1.0
Animal	fierce	1.6	Alive	man	1.2
Rain	weather	1.8	Exquisite	sweet	1.6
Teach	lesson	1.8	Empty	barrel	1.2
Doctor	medicine	1.0	Bitter	quassia	1.8
Book	interesting	1.4	Lazy	fellow	.8
Store	goods	3.0	Modesty	virtue	1.0
Horse	animal	1.6	Immensity	God	1.6
Island	Malta	1.2	Preservation	alcohol	1.8
Journey	long	1.0	Prudence	virtue	1.2
Freedom	liberty	.8	Indiscretion	vice	1.2
Average		1.5	Average		1.4

<i>May 10</i>			<i>May 12</i>		
Stimulus Word	Reaction Word	Reaction Time	Stimulus Word	Reaction Word	Reaction Time
Roast	meat	1.0	Find	treasure	2.0
View	panorama	1.8	Praise	merit	2.0
Whistle	a whistle	1.4	Pump	water	1.0
Alarm	people	2.6	Try	lesson	1.8
Indulge	drinker	1.4	Guard	tower	1.8
Frost	white	1.4	Iron	metal	1.8
Cask	wine	1.0	Stomach	empty	1.8
Curtain	silk	1.4	Salmon	fish	1.0
Nurse	baby	1.2	Bath	water	1.2
Ivy	wall	1.4	Splinter	wood	1.2
Thankful	grateful	1.0	Unfit	unable	2.0
Steep	stairs	1.2	Ardent	fire	1.2
Unwholesome	air	1.0	North	south	2.2
Gentle	woman	1.4	Handsome	lady	1.2
Faithful	servant	1.0	Price	high	2.2
Conflict	nations	1.2	Appetite	good	1.2
Anger	bad	2.2	Fable	Aesop	2.0
Idleness	vice	2.4	Definition	grammar	1.8
Betrayal	traitor	1.8	Queer	sound	2.2
Denouncement	fault	2.0	Ingenuity	simplicity	1.4
Average		1.5	Average		.7

<i>May 13</i>			<i>May 15</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Distrust	enemy	1.8	Sin	bad	1.4
Run	along way	2.0	Applaud	merit	.8
Agree	friend	1.2	Astonish	marvel	1.6
Needle	thread	1.2	Rejoice	good news	2.0
Chocolate	sweet	1.0	Use	tools	1.2
Twig	tree	1.2	Spool	loom	1.4
Napkin	white	1.2	Sheep	fur	1.6
Hill	steep	1.4	Emerald	precious stone	1.8
Finger	hand	1.0	Wagon	coal	1.6
Pretty	girl	1.2	Cottage	college	1.6
Contented	happy	1.0	Naughty	boy	1.2
Absent	minded	1.8	Exacting	demand	2.6
Magical	lantern	1.4	Thirsty	man	1.2
Profane	words	1.2	Playful	boy	1.2
Introduction	to a friend	1.4	Impulsive	dashing	1.8
Amusement	theatre	1.2	Faithfulness	dog	1.0
Remorse	sin	.8	Provocation	insult	1.4
Calmness	quietness	1.2	Contentment	happiness	1.0
Nod	head	1.0	Religion	faith	1.0
Calculate	Numbers	1.0	Profanity	bad word	1.0
Average		1.3	Average		1.4

<i>May 14</i>			<i>June 2, 1913</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Shock	electricity	1.4	Adore	God	1.0
Sweat	heat	1.8	Perish	ship	1.0
Melt	snow	1.4	Propose	marriage	1.1
Stun	hit	1.4	Uphold	opinion	1.8
Hunt	deer	2.0	Descend	mountain	1.2
Maiden	woman	1.8	Slave	poor	2.2
Bag	sand	2.0	Violin	song	1.6
Belt	leather	1.2	Brook	river	1.0
Cake	sweet	1.2	Chapel	church	1.0
Unhappy	miserable	1.6	Trumpet	sound	1.0
Pure	blood	1.8	Supreme	Being	1.2
Disorderly	irregularity	1.6	Elegant	lady	1.2
Unemployed	poor	2.0	Impudent	boy	2.8
Wretched	miserable	2.0	Blame	fault	2.1
Indulgence	vice	1.6	Gain	money	.8
Agreement	friendship	1.2	Idea	beautiful	1.4
Advantage	benefit	1.2	Worship	God	1.6
Injury	blow	1.2	Comfort	pleasure	3.0
Outrage	war	1.6	Noisy	room	1.0
Rubber	teeth	1.6	Level	ground	1.0
Average		1.6	Average		1.4

<i>June 3</i>			<i>June 5</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Cover	hat	1.3	Oppose	enemy	1.2
Hasten	pace	1.0	Enter	house	1.2
Curse	son	3.6	Drive	horse	1.0
Hurt	wound	1.4	Lecture	public	2.2
Blush	young lady	2.2	Flag	wave	1.0
Island	Malta	.8	Ivory	white	1.0
Copper	mental	1.0	Bed	sleep	1.2
Water	flowing	1.0	Fountain	water	1.0
Lettuce	vegetable	1.4	Pie	lemon	1.6
Brandy	alcohol	1.0	Awake	morning	1.4
Unseen	God	1.0	Dull	night	1.4
Merry	happy	1.6	Many	friends	1.8
Sacred	church	1.4	Green	leaves	1.2
Excellent	exam	1.6	Divine	God	1.0
Adorable	Saint	1.4	Terror	enemy	1.2
Life	Eternal	1.2	Spite	hatred	1.4
Opposition	enemy	1.2	Advice	council	2.0
Intellect	mind	1.2	Contempt	enemy	1.8
Sorrow	grief	1.4	Dispute	question	1.2
Education	school	1.2	Telephone	friend	2.6
	Average	1.4		Average	1.4

<i>June 4</i>			<i>June 6</i>		
Stimulus	Reaction	Reaction	Stimulus	Reaction	Reaction
Word	Word	Time	Word	Word	Time
Caress	baby	1.4	Scold	child	1.0
Reduce	salary	1.0	Walk	street	1.0
Reward	behavior	1.8	Punish	criminal	2.2
Talk	English	1.0	Smell	odor	1.2
Touch	table	1.0	Send	letter	1.4
Street	long	1.0	Mill	flour	1.0
Cane	reed	1.2	Elbow	hand	1.2
Soap	soft	1.4	Milk	white	1.0
Cheese	English	2.0	Scissors	cut	1.2
Drum	sound	1.0	Moon	night	1.2
Happy	healthy	2.2	Quiet	night	1.4
Small	boy	1.0	Infinite	God	1.0
Difficult	lesson	1.2	Brave	soldier	1.4
Painful	wound	1.2	Ornamental	church	1.0
Grief	sorrow	1.0	Dreadful	fight	1.4
Thought	good	1.4	Chance	good	1.4
Credit	great	1.6	Quarrel	men	2.0
Fear	death	1.4	Conscience	good	1.2
Mercy	God	1.2	Scandal	bad	1.8
Sinful	man	1.0	Evil	bad	1.6
	Average	1.3		Average	1.3

June 7

Stimulus	Reaction	Reaction
Word	Word	Time
Irritate	nerves	1.0
Tame	animal	1.0
Feed	animal	1.2
Imagine	vision	1.0
Suffer	pain	1.0
Dinner	good	1.2
Raft	sea	1.2
Chart	fever	1.8
Glove	hand	1.0
Bird	sing	1.2
Afraid	lion	1.0
Blue	sky	.8
Anxious	desirous	1.2
Long	street	1.0
Audacious	hero	1.2
Expression	vocal	1.2
Mistake	great	1.2
Devotion	church	1.2
Errand	boy	1.0
Expense	great	1.4
Average		1.14

AN EXPERIMENTAL STUDY OF DECISION TYPES AND THEIR MENTAL CORRELATES

It is a matter of common observation that individuals differ greatly in decision *time*, *accuracy*, and *constancy* of or consistency in subjective decisions. It is generally supposed that the slow in decision are both more constant and more accurate than the quick. "Slow but sure" is a popular maxim. The chief purpose of this investigation was to determine the actual nature of these various decision types—to see, for example, whether "quick but sure" and "slow but unreliable" are not equally true of certain classes. In other words, it was desired to ascertain the relation between decision *time* and *constancy*, between *time* and *accuracy*, and also between the subjective and objective decision types thus determined. A further purpose was to determine the correlation subsisting between these decision factors (*viz.* : time accuracy and constancy) and other mental traits such as memory, association time, suggestibility, attention, etc.

The experiments were conducted in the Harvard Laboratory during the years 1911 to 1913. In all, nineteen subjects took part; three of them (A, C and L) continued through the two years' work. Twelve participated in the first year's experiments reported in the first part of this paper, and ten in the second year's experiments reported in Chapter II. All were members of the laboratory and trained in experimental methods. Five (D, F, I, O, and P) were women. Three of these were in the first year's investigation and two in the second.

In the first year's experiments on decision the materials used were cards upon which were printed the names of the objects to be decided upon from various points of view. Of these there were forty pairs, fifty groups of three each, and fifty groups of five each.¹ The following are some representative examples.

¹ All the pairs, twenty-five groups of three and twenty-five groups of five were selected from material prepared by G. W. Peckham, who had begun work upon this problem in the Harvard Laboratory, but found it necessary to discontinue the investigation.

Pairs: violin music, piano music; Hamlet, Macbeth; orange marmalade, baked apple; Hume, Kant; gunpowder, printing; camel, elephant. Threes: Poe, Kipling, Hawthorne; wealth, strength, power; literature, science, philosophy; baseball, football, tennis; Dante, Goethe, Shakespeare; X-rays, photography, telegraphy; fountain pen, penknife, watch. Fives: music, sculpture, painting, drama, poetry; peaches, pears, apples, grapes, cherries; chrysanthemum, lily, dahlia, carnation, rose; Rembrandt, Holbein, Titian, Rubens, Ingres; temperance, wisdom, justice, courage, truth; bicycle, safety razor, automobile, telephone, balloon. The material used for subjective decision in the second part of the investigation, and that used for accuracy of decision, and in the several tests, will be described under the sections dealing with these various experiments.

RELATION OF TIME AND CONSTANCY

The first experiments performed were to determine the relation between time and constancy in relatively subjective decisions. These experiments can be divided into three series. In the first series, consisting of forty experiments, the pairs of cards, described above, were exposed by tilting back a shutter behind which they had been placed. The subject was directed to decide as quickly as was compatible with sincerity, and to raise the right or left hand according as the decision was for the right or left card. The basis of decision, which varied with the nature of the concepts, was in each case given by the experimenter; thus: decide according to your preference, their greatness, their importance, or their usefulness. The time was taken with a stop watch, which was started at the same instant that the crank of the shutter was turned back, and stopped at the signal of the subject. The time and the decision were recorded. After five weeks the experiment was repeated in the same way. The decision and time were again recorded and compared with those of the first presentation.

The results of this series of experiments are shown in Table I. The number of changed decisions, the average time for the

first presentation, the average time for the second presentation, the combined average, and the number of first decisions remembered at the second presentation, are given for each of the nine subjects. The number of decisions changed varies from 1 for subject F to 13 for subject G; and the decision time from 1.46'' for C to 3.30'' for H. There is, however, no evidence of a correlation between time and constancy. The figures in the last column give some idea of the part played by memory in this experiment.

TABLE I

Subject	No. C.	Results with Pairs			No. R.
		A. T. ¹	A. T. ²	A. T. ³	
F	1	2.96	2.34	2.65	31
A	4(—1)	2.74	2.56	2.65	9
B	5(—2)	2.28	2.91	2.60	7
E	5	2.54	2.23	2.39	7
C	6(—1)	1.46	1.57	1.52	3
H	7	3.30	3.12	3.21	18
D	8(—1)	2.49	1.99	2.24	12
I	10(—1)	2.00	1.43	1.71	20
G	13(—3)	3.04	2.75	2.89	14

No. C. = Number of changed decisions. Those in brackets doubtful.

A. T.¹ = Average for first presentation.

A. T.² = Average for second presentation.

A. T.³ = Combined average.

No. R. = Number of decisions remembered from first to second presentation.

In order to eliminate the memory factor as far as possible, a more complex series of experiments was devised. This second series consisted of the fifty groups of three cards each. These were arranged by the subject *in order* of preference, greatness, importance, or usefulness. The method was otherwise the same as in Series I. The motor activity of arranging the cards was rendered approximately constant in the following manner: On tilting back the shutter the cards were exposed in an upright position between two slats. The subject removed each card and laid it down whether the presented arrangement was maintained or changed. This series was also repeated in four to five weeks, except in the case of subjects K, J and L; here the time between first and second presentation was seven to eight weeks. The

same directions for arranging the cards were given the subject on each occasion; but a change of the basis of decision sometimes occurred under the category specially mentioned,—e.g., *importance* might mean importance to self, to the world, to science, etc. The possibility of such changes was as far as possible avoided; but all differences due to these changes or to lack of knowledge of the material were eliminated. The results of this series are shown in Table II. The number of changed decisions, and the average, and the median for the first and for the second presentations are given. Also, in columns marked "Above M²" and "Below M²", the changes in decisions whose times are above or below the median in the second presentation are given. There are always more changes in decisions above the median. This may be explained by supposing that the uncertain decisions take the longest time, and are also the more liable to change.

TABLE II
Results with Groups of Three

Subject	No. C.	A. ¹	M. ¹	A. ²	M. ²	Above M ²	Below M ²
A	11	8.7	8.2	9.5	9.3	9	2
K	16(—6)	6.3	5.6	10.1	7.3	12	4
J	17(—5)	7.1	6.2	5.7	5.0	15	2
D	16	5.8	5.5	5.5	4.8	9	7
F	17	7.8	6.8	6.8	5.6	12	5
E	18(—1)	7.3	7.1	6.5	6.3	10	8
B	20(—3)	9.6	9.0	8.3	7.6	13	7
C	21(—4)	9.5	8.4	6.5	5.4	13	8
L	25(—7)	16.4	13.5	12.3	11.1	16	9
H	28(—5)	11.0	10.2	8.1	7.9	19	9
G	27	8.9	8.6	8.3	7.8	17	10
I	32(—2)	4.3	4.2	4.9	4.4	18	14
(Total).....						163	85
						66%	34%

No. C. = Number of changed decisions.

A.¹ = Average for first presentation.

A.² = Average for second presentation.

M.¹ = Median for first presentation.

M.² = Median for second presentation.

Above M² and Below M² = Number of changed decisions above and below the median in the second presentation.

In Brackets, doubtful decisions to be subtracted.

Table III-A shows how the subjects might be classified according to type. The types are arranged in groups with each other and some

subjects are difficult to place. The memory factor which vitiated the results of the first series (see table I) was almost negligible here, and entirely so in Series III, when the fifty groups of five cards each were presented and arranged as in Series II. These were also repeated in about five weeks and the results are summarized in Table IV in the same manner as in Table II. In Table III-B the subjects are classified according to type as a result of this series. For the most part they hold the same place as in III-A; but two subjects, E and F, are relegated from the constant to the inconstant class, probably confused by the greater complexity of the conditions. In the case of E, who was leaving the University, the second presentation for twenty-five of the fifty groups was given two weeks after the first instead of five. The number of changes would in all probability have been greater, had the time for him been the same as for the others. C, however, who is medium in time and constancy in III-A, takes comparatively very much longer to decide with groups of five; thus becoming slow in time and medium in constancy in III-B.

Subjects J, K and L are omitted in Table IV, because they were given only twenty-five groups of this series. Table V and III-C give the results of all the subjects for these twenty-five. On comparing Tables III-A and III-C it will be noticed that here again under the more complex conditions, one subject, K, is removed from the quick to the slow; and another, F, is removed from the quick constant to the slow inconstant. These were the twenty-five groups that were repeated with subject E in two weeks instead of five, and this probably accounts for his position in III-C as compared with III-B. It is worthy of notice that the slow subjects are on the whole more constant than the quick; but both slow and quick can be divided into constant and inconstant classes. The slow constant are, thus, more constant than the quick of the same class.

The classifications in Tables III-A, B and C were made more difficult by the fact that the medians of some subjects differ greatly from the averages. We have used the median as the basis of classification. This is quite satisfactory with subjects

whose decision time is uniform, for here there is a close correspondence between median and average; but sometimes when the decision time is variable the correspondence is not so close. The median is always less than the average, sometimes only slightly less, but in certain cases (e.g., that of K) very much less. This suggests another classification of the subjects, on the basis of the mean variation of their decision times, into the "Uniform" and the "Variable." The most variable would then be subjects K, C and F; and the most uniform, subjects A, G, B and E. It will be noted that the subjects who were removed from one class to another under more complex conditions, as mentioned above, are also variable subjects; and their variability in time and change of class may be results of the same factor. For under any given class some decisions are more difficult than others; and their general inability to cope with difficult situations prolongs the decision time or produces inconstancy, even in circumstances when less difficult decisions are quick and constant. In other words, there is a type of subject who, in simple situations (i.e., easy decisions), is very quick and constant; but who, when the decisions are more difficult, becomes relatively slower and more inconstant. This fact is of considerable significance in vocational guidance, and situations of practical life, for such subjects must be eliminated if a selection of *unfailingly* quick constant workers is to be made.

The method of estimating the amount of the change when a group of three or five was arranged in a different way on the second presentation needs some explanation. Not the absolute changes of position, but the relative changes were considered. Thus, in a group of five, A, B, C, D, and E, the greatest possible number of changes is ten. This would occur if it were completely inverted, E, D, C, B, A; for although C is in the same absolute position, yet A has changed relatively to B, C, D and E, B to C, D, and E, C to D and E, and D to E. In one group of five, then, any degree of difference from one to ten is possible, while in a group of three, the difference cannot exceed three.

TABLE III-A

<i>Type</i>	<i>Subject</i>	<i>Threes</i> <i>*No. C.</i>	<i>M.¹</i>	<i>M.²</i>
Slow constant	A	11	8.2	9.3
	B	17	9.0	7.6
Slow inconstant	H	23	10.2	7.9
	G	27	8.6	7.8
	L	18	13.5	11.1
Quick constant	C	17	8.4	5.4
	D	16	5.5	4.8
	E	17	7.1	6.3
	J	12	6.2	5.0
	K	10	5.6	7.3
	F	17	6.8	5.6
Quick inconstant	I	30	4.2	4.8

*The doubtful ones, bracketed in Table II, have been subtracted.

TABLE IV

Results with Groups of Five (50)

<i>Subject</i>	<i>*No. C.</i>	<i>A.¹</i>	<i>M.¹</i>	<i>A.²</i>	<i>M.²</i>	<i>Above</i> <i>M.²</i>	<i>Below</i> <i>M.²</i>
A	36	19.7	19.0	20.1	19.6	23	13
B	54	20.3	19.6	20.7	21.0	33	21
C	66	23.6	19.7	15.9	13.4	40	26
D	66	13.7	12.1	14.0	12.5	48	18
E	73	14.9	14.6	13.0	12.8	45	28
F	77	18.5	16.1	15.7	13.4	55	22
G	77	19.5	18.8	17.0	16.8	40	37
H	79	24.2	22.2	18.7	18.3	58	21
I	108	8.6	8.6	8.3	7.5	45	63
(Total)						387	249
						<u>61%</u>	<u>39%</u>

*All doubtful ones have been subtracted.

TABLE III-B

Fives—50 Groups

<i>Type</i>	<i>Subject</i>	<i>No. C.</i>	<i>M.¹</i>	<i>M.²</i>
Slow constant	A	36	19.0	19.6
	B	54	19.6	21.0
	C	66	19.7	13.4
Slow inconstant	H	79	22.2	18.3
	G	77	18.8	16.8
Quick constant	D	66	12.1	12.5
Quick inconstant	F	77	16.1	13.4
	E	73	14.6	12.8
	I	108	8.6	7.6

TABLE V
Results with Groups of Five (25)

Subject	No. C.	A. ¹	M. ¹	A. ²	M. ²	Above M. ²	Below M. ²
A	23*(6)	21.4	21.3	21.0	20.0	16	7
E	24 (9)	15.0	13.6	13.2	13.0	16	8
B	27 (8)	20.9	20.2	20.0	19.0	16	11
J	29 (5)	14.6	13.2	12.6	11.0	17	12
D	29 (9)	13.4	12.4	15.2	12.4	19	10
C	30(10)	19.1	17.0	11.6	12.0	16	14
K	31 (5)	32.4	22.4	34.3	27.6	21	10
H	36 (9)	23.1	22.2	18.1	17.2	26	10
G	35(11)	17.9	15.6	15.4	15.0	17	18
I	36 (9)	10.0	9.0	10.0	10.0	25	11
F	45 (5)	21.6	22.0	22.2	24.1	31	14
L	48 (4)	22.1	20.0	16.5	15.6	29	19
(Total).....						249 63%	144 37%

*In brackets are additional doubtful differences.

TABLE III-C
Fives—25 Groups

Type	Subject	No. C.	A. M.
Slow constant	A	23	20.2
	B	27	20.0
	K	31	25.0
Slow inconstant	H	36	19.3
	G	35	15.6
	F	45	23.2
	L	48	17.8
Quick constant	E	24	13.2
	J	29	12.1
	D	29	12.4
	C	30	14.5
Quick inconstant	I	36	9.5

A. M. = Average of M.¹ and M.²

No. C. = Changes, all doubtful ones subtracted.

It will be noticed that the time for the second presentation is generally less than for the first. In some cases (e.g., that of subject C) it is much less. This is what we would expect, but there are two striking exceptions: viz., subject K, whose time is much longer in the second case, and subject A. This lengthening of the time may be due to more or less conscious effort on the part of these subjects to be slow. The decrease in time in the

case of C is very much more than one would expect; and for H and L considerably more than the average. Here we have a type of subject that, having once made a satisfactory decision, is able, when placed in a similar situation, to make a similar decision much quicker than the first. Such a subject learns from experience, and performs his customary decisions as it were automatically. So long as the situation remains the same he becomes more and more efficient. This, however, does not prove that he will be more efficient in *novel* situations. On the contrary, the latter sort of efficiency may very well be an original endowment, that no amount of training can develop. The time, therefore, for the first presentation is of greater significance in classifying the subjects than that for the second. Here again the existence of these two types of efficiency in decision has considerable bearing upon vocational guidance.

The mean average decision time was determined for pairs, for groups of three, and for groups of five. The columns marked A in Tables I, II, and V were added up; and the respective sums divided by the number of subjects in each case. The results are as follows:

Mean average time for pairs.....	2.54
Mean average time for threes.....	8.56
Mean average time for fives.....	19.30

The ratio of increase is greater from pairs to threes than from threes to fives (3.37 and 2.14 respectively). The subjects whose decision times vary most from the average are: I, whose time for groups of five is 8.6, and K, whose time for the same is 32.4. The former is a "Uniform" subject, the latter a "Variable" one; and there is, therefore, no correlation between variability of decision time for any particular subject and variability from the mean average of all the subjects. We are speaking of variability in time only, not in decision; and the above has, therefore, no bearing upon F. L. Wells' statement that "Those who vary the least from their own judgments also vary least from the judgments of others."²

²"On the Variability of Individual Judgments," *Essays Phil. and Psych.* in honor of William James, p. 529.

A noticeable feature in the second presentation of Series II and III was the numerous memory illusions. These occurred with every subject but with some more than others. The women were generally more subject to them than the men. In each case the subject was asked whether he remembered having made the decision before, and if so whether he decided in the same way. In many cases the subject would vigorously affirm that he actually remembered arranging the cards in the same way before, when the previous arrangement was really quite different. There were also some fewer cases when the subject remembered that his decision was different, when it was really the same.

CONFIDENCE

Three of the subjects—J, K, and L—were asked to state the degree of their confidence in each decision. This was reported by the subjects as meaning either "The subjective feeling of satisfaction in the arrangement," or "The assurance of constancy." The same subject would alternate between these two attitudes from time to time. The subjective feeling is different in each case; but for our purposes, the correlation of constancy and confidence, we assume that they need not be distinguished; for the former always implies the latter, though the opposite is not always true. The degree of confidence was given as A, B, or C,—A being perfect confidence, C very little or none, and B medium. It is clear that the subjects might differ not only in the degree of their confidence but also in their way of describing it. The same degree of confidence might be described as A by one and as B or C by another. This, however, is a criticism that can be raised as well against the introspective method as a whole.

The results of this confidence test are shown in Tables VI-A and VI-B. The subjects are compared with one another; and the confidence of the decisions that were changed with that of the whole. The "numerical value" is obtained rather arbitrarily by valuing A at 2, B at 1, and C at 0; but perhaps it is not so arbitrary after all for A is introspectively perfect confidence (1), B medium (1/2), and C none (0); and to avoid fractions 2, 1,

and o are used instead. The average confidence is obtained by dividing the numerical value by the number of decisions.

The following points are noteworthy:

- (1) The more confident subjects are also the more constant.
- (2) The confidence is slightly less with the decisions that were changed. (See Tables VI-A and VI-B under "Average Confidence.")
- (3) One subject (J) is regularly less confident on the second presentation; the others are equally or more confident.
- (4) The decisions changed are divided fairly equally among A, B, and C; but the amount of the change is as a rule greater in C decisions than in B's or A's. There are, however, exceptions. (See tables VI-A and VI-B, last section.)

TABLE VI-A—(THREES)

		<i>Degree of Confidence in Total Decisions</i>			
<i>Subject</i>	<i>No. D.</i>	<i>P.¹</i>	<i>P.²</i>	<i>N. V.¹</i>	<i>N. V.²</i>
J	49	32A + 10B + 7C	26A + 15B + 8C	74	67
K	49	28A + 13B + 8C	29A + 16B + 4C	69	74
L	50	14A + 25B + 11C	14A + 29B + 7C	53	57
		<i>Degree of Confidence in Decisions Changed</i>			
J	10	6A + 3B + 1C	2A + 6B + 2C	15	10
K	9	3A + 4B + 2C	4A + 4B + 1C	10	12
L	16	4A + 6B + 6C	4A + 8B + 4C	14	16
		<i>Average Confidence, if A is represented by 2</i>			
		<i>Total Decisions</i>		<i>Decisions Changed</i>	
		<i>P.¹</i>	<i>P.²</i>	<i>P.¹</i>	<i>P.²</i>
J		1.51	1.37	1.50	1.00
K		1.41	1.51	1.11	1.33
L		1.06	1.14	.87	1.00
		<i>Average Amount of Change for A, B, or C Confidence</i>			
		<i>No. Groups Changed</i>	<i>N. V. of Changes</i>	<i>Ave. for A, B, C</i>	
J	10	6A	7A	1½	A
		3B	3B	1	B
		1C	2C	2	C
K	9	3A	3A	1	A
		4B	4B	1	B
		2C	3C	1½	C
L	16	4A	4A	1	A
		6B	6B	1	B
		6C	8C	1½	C

P.¹ and P.² = First and second presentations.

N. V.¹ and N. V.² = Numerical Value of confidence in first and second Decisions

No. D. = Number of Decisions.

TABLE VI-B (FIVES)

		<i>Degree of Confidence in Total Decisions</i>			
<i>Subject</i>	<i>No. D.</i>	<i>P.¹</i>	<i>P.²</i>	<i>N. V.¹</i>	<i>N. V.²</i>
J	25	14A + 6B + 5C	9A + 13B + 3C	34	31
K	25	10A + 12B + 3C	11A + 9B + 5C	32	31
L	25	3A + 14B + 8C	2A + 19B + 4C	20	23
		<i>Degree of Confidence in Decisions Changed</i>			
J	14	5A + 4B + 5C	2A + 10B + 2C	14	14
K	17	5A + 10B + 2C	7A + 5B + 5C	20	19
L	21	2A + 11B + 8C	1A + 16B + 4C	15	18
		<i>Average Confidence, if A is represented by 2</i>			
		<i>Total Decisions</i>		<i>Decisions Changed</i>	
		<i>P.¹</i>	<i>P.²</i>	<i>P.¹</i>	<i>P.²</i>
J		1.36	1.24	1.00	1.00
K		1.28	1.24	1.18	1.12
L		.80	.92	.71	.86
		<i>Average Amount of Change for A, B, or C Confidence</i>			
		<i>No. Groups Changed</i>	<i>N. V. of Changes</i>	<i>Ave. for A, B, C</i>	
J	14	5A 4B 5C	29	6A 11B 12C	1½ A 2¾ B 2¾ C
K	17	5A 10B 2C	31	6A 23B 2C	1½ A 2 3/10 B 1 C
L	21	2A 11B 8C	48	8A 20B 20C	4 A 19/11 B 2½ C

The degree of difficulty of the decisions was also obtained from the same three subjects, but only for the first presentation. The results are given in Tables VII-A and VII-B. D means difficult, M medium, and E easy. An arbitrary "numerical value" is obtained by valuing the difficulty of D at 2, of M at 1, and of E at 0; and the average difficulty by dividing the numerical value by the number of decisions.

It will be noticed that:

- (1) The subjects differ considerably in the amount of difficulty, and the least confident subject has the greatest difficulty in the threes but the least in the fives.
- (2) The easy as well as the difficult decisions are changed but the average difficulty of those changed is slightly greater than for the whole.

TABLE VII-A (THREES)

<i>Subject</i>	<i>No. D.</i>	<i>Degree of Dif.</i>	<i>N. V.</i>	<i>Ave. Dif.</i>
<i>Degree of Difficulty in Total Decisions</i>				
J	49	15D + 12M + 22E	42	.86
K	49	10D + 16M + 23E	36	.73
L	50	22D + 7M + 21E	51	1.02
<i>Degree of Difficulty in Those Changed</i>				
J	10	4D + 2M + 4E	10	1.00
K	9	3D + 1M + 5E	7	.77
L	16	7D + 3M + 6E	17	1.06

TABLE VII-B (FIVES)

<i>Degree of Difficulty in Total Decisions</i>				
J	25	8D + 7M + 10E	23	.92
K	25	13D + 4M + 8E	30	1.20
L	25	6D + 7M + 12E	19	.76
<i>Degree of Difficulty in Those Changed</i>				
J	14	7D + 5M + 2E	19	1.36
K	17	10D + 3M + 4E	23	1.35
L	21	5D + 6M + 10E	16	.76

ASSOCIATION

Two experiments were carried out to ascertain the association time of the subjects. There were nine subjects, six of whom were the same in each case and three different. In the first a list of fifty words were used. These were repeated to each subject who was directed to reply as quickly as possible with the first word that came to his mind. The time was taken with a stop watch visually; that is, the watch was started and position of the hand noted when the word was given, and when the reply was heard. This method was found difficult and inaccurate; and in the second experiment, which consisted of forty words, the watch was started when the word was given and stopped when the reply was heard. The results were very much the same except in the case of C, whose time in the second experiment is much shorter than in the first. Full results of both experiments are given in Table VIII.

number of cases investigated is probably too small to warrant any inference from these figures.

There is some evidence of a considerable correlation between association time and constancy. If we compare the deviation from the central tendency in the number of changes with the deviation in association time, we get for threes 17% cases of unlike signs, for fives 33 $\frac{1}{3}$ %. By reference to the Table in Whipple's Manual, we see that this signifies correlations of $+.860$ and $+.509$. It is scarcely probable that mere chance could give us these figures; and, though investigations on a larger number of subjects may alter them considerably, it is very likely that here we have evidence of a real correlation. We are, therefore, if we use the results of this very inadequate investigation, able to say that if a subject's association time is longer than the average, he is very liable to be more inconstant than the average; and similarly, that if his association time is shorter than the average, he is probably more constant; but in either case we can say nothing regarding the length of his decision time.

TABLE VIII-A

Subject	D.A.	D.D. ^a	D.D. ^b	D.C. ^c	D.C. ^d
B	+.25	+1.3	+.7	0	-3 $\frac{1}{2}$
H	+.25	+2.7	+4.6	+6	+5 $\frac{1}{2}$
F	+.25	-.5	-1.1	0	+14 $\frac{1}{2}$
G	+.25	+.6	-.1	+10	+4 $\frac{1}{2}$
I	+.15	-4.0	-11.0	+13	+5 $\frac{1}{2}$
J	+.05	-1.2	-5.0	-5	-1 $\frac{1}{2}$
K	-.05	-2.0	+12.8	-7	+ $\frac{1}{2}$
L	-.05	+8.1	+2.5	+1	+17 $\frac{1}{2}$
C	-.15	+1.2	+4.0	0	- $\frac{1}{2}$
A	-.15	+.4	+.1	-6	-7 $\frac{1}{2}$
E	-.15	-1.0	-4.7	0	-6 $\frac{1}{2}$
D	-.15	-2.5	-5.9	-1	-1 $\frac{1}{2}$
		50%	66 $\frac{2}{3}$ %	16 $\frac{2}{3}$ %	33 $\frac{1}{3}$ %

D. A. = Deviation from central tendency (1.15) in association time.

D. D.^a = " " " " (8.3) " decision time (3's).

D. D.^b = " " " " (19.6) " " " (5's).

D. C.^c = " " " " (17) " constancy (with 3's).

D. C.^d = " " " " (30 $\frac{1}{2}$) " " " (5's).

Numbers at foot of columns are percentages of unlike signs, by comparing with column 1.

SORTING CARDS

An experiment to test the rapidity of sorting cards was also performed. At first ordinary playing cards were used; but, since some of the subjects were more familiar with these than others, another pack of cards equally unfamiliar to all was substituted. This pack contained fifty-two cards, thirteen of which were marked A, and an equal number B, C, and D. The subject was told to sort them into four heaps according to the letters; and the time was taken with a stop watch. The results are shown in Table VIII, under column T. S. C. If the method of unlike signs be employed, as with association time above, to determine the correlation between time for sorting cards and decision time, it will be found that there are just 50% cases of unlike signs, and, therefore, the coefficient of correlation is zero.

MEMORY

Two sets of experiments were performed to test the memory of the subjects. The first was for immediate memory, and the second for memory over longer periods of time. In the former, nonsense syllables, words and figures were used. There were two tests with nonsense syllables. The first was to get the subject's memory limit; that is, the greatest number of syllables that could be remembered and repeated in order after two repetitions. The metronome was set to beat at one per second, and a syllable was exposed through a slot in a cardboard at each beat. Each list was given twice and the subject read the syllables aloud. After a five second pause, the subject was asked to repeat the syllables in order. Four syllables were given in the first list; and if these were remembered, five were given, etc. In Table IX, column 1, is shown the various limits. There is very little variation here, the limit being always four or five. Another test was therefore given. A list of four syllables was exposed twice in precisely the same manner, and after five seconds the subject was asked to repeat the list in order. The variations in this test were greater than in the first, as is shown by column "2," Table IX.

Two similar tests were given to determine the maximum number of

limit, the other to get the number remembered out of a list of fourteen. The method was the same as with the nonsense syllables, and the results are shown in columns L.² and "14," Table IX. The limit varies from five to nine, and the number remembered out of fourteen, from eight to fourteen.

Memory for figures was tested by exposing numbers of six digits and upwards. The subject read the digits aloud at the beat of the metronome (one per second). The number was read through in this way twice, and then after a five-second interval it was repeated from memory. If the subject was successful, a number of one more digit was given, etc., until the limit was reached. The results varied from eight to eleven, as shown in column F, Table IX. The "Total" memory ability of each subject is obtained by adding the results of these five tests; and the subjects are arranged in order according to this sum. The decision type of each subject is indicated by the letters opposite the general memory index. More accurate information regarding time and constancy can be obtained in Tables II, IV or VIII.

If we again apply the "Method of unlike signs" to ascertain the amount of the correlations between the general memory index and decision time, and also between the former and constancy, we get in each instance 50% cases of unlike signs, and, therefore, a correlation of zero. Further, if the memory for nonsense syllables

TABLE IX

Subject	Nonsense Syl.		Immediate Memory			Total	Decision Type
	L. ¹	9.	L. ²	14.	Figures		
F	4	3	7	8	8	30	S—I*
J	4	3	6	9	8	30	Q—C
G	5	4	6	9	8	32	S—I
A	4	5	5	10	10	34	S—C
B	5	3	6	14	9	37	S—C
I	4	7	5	13	10	39	Q—I
C	5	4	8	11	11	39	†Sor Q—C
K	4	4	8	14	10	40	†Sor Q—C
D	5	6	9	11	10	41	Q—C
L	5	7	9	14	9	44	S—I

S = slow.

Q = quick.

I = inconstant.

C = constant.

* = Q—C with threes.

† = Slow with fives.

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TABLE IX

Subject	Nonsense Syl.		Immediate Memory Words		Figures	Total	Decision Type
	L. ¹	9.	L. ²	14.	F.		
F	4	3	7	8	8	30	S-I
J	4	3	6	9	8	30	S-I
G	5	4	6	9	8	30	S-I
A	4	5	5	10	10	34	S-I
B	5	3	6	14	9	37	S-I
I	4	7	5	13	10	39	S-I
C	5	4	8	11	11	39	S-I
K	4	4	8	14	10	40	S-I
N	6	6	9	14	11	46	S-I
	7	7	9	14	9	48	S-I

With the constancy
to question of accu-
advisable to perform
decision. It proved how-
ent that would test accuracy

bles, for words, and for figures be taken separately, we get in all *three cases* a coefficient of zero between memory and *decision time*; but a coefficient of .587 between memory for words and *constancy* in decision; and of .809 between memory span for figures after five seconds and *constancy*. That is, if we compare each subject's deviation from the central tendency for *constancy* with his deviation from the central tendency for memory for figures, we find that there are 80% cases of unlike signs. By referring to Whipple's Manual, Table 9, we see that this means a coefficient of correlation of $-.809$; but since $+$ in the case of memory for figures signifies excellence, and in the case of number of differences, inconstancy, the relation between good memory and constancy is a direct ($+$) relation and not an inverse one. That is, the constant subjects have better memories for figures; and the same is true of memory for words.

Memory for a period of one week was also tested. Two lists of nonsense syllables of ten syllables each and one list of twelve words were given. The Lipmann-Marx memory apparatus⁴ was used, and the number of repetitions necessary to learn each list recorded. The same list was given one week later and the saving in repetitions noted. The learning was called complete when the subject could anticipate one word or syllable ahead throughout the list. Learning ability or immediate memory is shown by the number of repetitions necessary to complete the learning, and memory for one week is represented by the percentage of saving. The results are scarcely reliable, especially for the words, as only one list was given; but they are presented in Table X and can be taken for what they are worth. The average of the two lists of nonsense syllables is taken and the percentage gained is calculated. The subjects are arranged (X-A) in order of the saving for the nonsense syllables; but no definite relation is shown to time and constancy which are indicated in the last column. If, however, they are arranged according to the saving for words (X-B), the slow appear to have poorer memories than the quick. There was some appearance of a similar relation in the case of immediate memory; but an application of the "Method

⁴ See Bericht über die Arbeit von Konrad Lipmann in der Zeitschrift für Psychologie, p. 306.

of Unlike Signs" failed to reveal it. The same method now brings out a correlation coefficient of .92 between quickness in decision and memory for a period of one week; but it shows no correlation of memory for words with constancy; and in the case of nonsense syllables, no correlation of memory for one week with either constancy or time of decision. There are large individual differences in the number of repetitions necessary to learn the lists. These might be considered with Table IX as another test of immediate memory. No definite correlation is, however, brought out between the learning of either nonsense syllables or words, and decision time and constancy.

TABLE X-A
Memory for One Week
Nonsense Syl. Words

Subject	R. ¹	R. ²	% Gained	R. ¹	R. ²	% Gained	Decision Type
J	12	10	17	11	6	45	Q—C
L	6½	5	23	10	6	40	S—I
A	10½	7	33	7	7	0	S—C
I	8½	5	41	4	2	50	Q—I
B	10½	6	43	8	5	38½	S—C
K	14½	7	52	7	5	28	S—C (or Q—C)
C	12	5½	54	8	4	50	Q—C (or S—C)
F	25	7½	70	6	3	50	S—I (or Q—C)

TABLE X-B
% Gained on Words Decision Type

A	0	S—C
K	28	S—C (or Q—C)
B	38½	S—C
L	40	S—I
J	45	Q—C
C	50	Q—C (or S—C)
I	50	Q—I
F	50	Q—C (or S—I)

R.¹ = Repetitions required for the first learning.

R.² = Repetitions required for the second learning.

ACCURACY OF DECISION

The foregoing experiments on decision dealt with the constancy of the subjects in subjective judgments, but no question of accuracy was involved. It, therefore, seemed advisable to perform some experiment to test the accuracy of decision. It proved however, very difficult to devise an experiment that would test accuracy

of decision, and that would not at the same time involve a test of sensory acuity. The following method, which is not free from various objections, was adopted. Black paper cards, in which different numbers of small holes were made, were exposed two at a time by means of a drop apparatus, and the subject was asked to point as quickly as possible to the one having the greater number of holes, or to raise both hands if he considered them the same. Two cards (A and B) with twenty-five holes in each were used as standards; and the other cards, containing from twenty to thirty holes each, were exposed successively with one or the other of these. The standard was placed now on the right side, now on the left; and the apparatus was so placed that when the slide dropped and the cards were exposed, the light shone through the holes from a window in the rear. The time was taken with a stop watch, and fifty decisions in all were obtained. The results are shown in Table XI under A.

It might have been expected that the subjects would fall into four classes; viz., slow accurate, slow inaccurate, quick accurate, and quick inaccurate; and that these would correspond to the four types of subjective decision. As is shown by Table XI, the latter is not altogether true. The types in subjective and objective decision are indicated by the letters in juxtaposed columns. The quickness of the objective decision is based upon the median rather than the average, as the latter was sometimes greatly increased by a few unduly prolonged decisions.

The difference in the number of errors are, however, scarcely sufficient to justify a classification into accurate and inaccurate. Fifty more decisions were, therefore, obtained. In this case the cards used as standards contained fifty holes each, and the other cards from forty-four to fifty-five. An attempt was made to have the holes cover nearly equal group-areas on the cards. The method of procedure was the same as before. The results are shown in Table XI under B. There is even less correspondence here between the subjective and objective types; e.g., subject I, who is quick and inconstant in subjective decisions, is slow and accurate in objective, and A, who is slow and constant in the former, is quick and inaccurate in the latter, et

TABLE XI

Subject	E.	A.	M.	Type in Obj. Dec.	Type in Subj. Dec.
<i>A—Fifty Easy Decisions</i>					
D	13	1.50	1.4	Q—A	Q—C
C	13	1.56	1.4	Q—A	Q or S—C
A	13	1.98	1.8	S—A	S—C
I	14	2.07	1.6	Q—I or A	Q—I
F	15	1.78	1.6	Q—I	S—I (or Q—C)
B	16	2.76	2.2	S—I	S—C
<i>B—Fifty Difficult Decisions</i>					
I	16	3.8	3.6	S—A	Q—I
F	18	2.5	2.5	Q—A	S—I (or Q—C)
C	19	3.4	2.7	Q—A	Q—C
A	23	2.3	2.0	Q—I	S—C
B	24	5.7	4.3	S—I	S—C
<i>C—Twenty-five Difficult Decisions</i>					
K	7	7.7	4.3	S—A	S—C (or Q—C)
J	9	4.0	3.6	S—A or I	Q—C
L	10	3.8	3.0	S—A or I	S—I
<i>D—Twenty-five Dif. Decisions—All Subjects</i>					
Subject	E.	A.	M.	Type in Obj. Dec.	Type in Subj. Dec.
K	7	7.7	4.3	S—A	Q—C (or S—C)
I	8	3.8	3.6	S—A	Q—I
J	9	4.0	3.6	S—A	Q—C
C	10	3.4	2.7	Q—A or I	Q—C (or S—C)
L	10	3.8	3.0	S—A or I	S—I
F	11	2.5	2.5	Q—I	S—I (or Q—C)
B	11	5.7	4.3	S—I	S—C
A	14	2.3	2.0	Q—I	S—C

E. = errors.
A. = average time.
M. = median.

Q = quick.
C = constant.
I = inconstant, or inaccurate.

S = slow.
A = accurate.

Twenty-five of the difficult decisions were given to subjects K, L, and J, and the results are shown in Table XI-C. The times for all the subjects and the number of errors for the same twenty-five cards as were presented to these three are given in Table XI-D. An interesting result comes to light here; but it is also noticeable, though somewhat less evident, in the other Tables. It appears that *all those who were quick in subjective decisions are accurate in objective decisions, and the slow in subjective decisions are inaccurate in objective.*

This is a rather interesting correlation; but on second thought it is not so extraordinary as it at first seems. The individual who can narrow down his field of consciousness upon one problem or situation, and who can keep his attention from associated ideas,

will be quick in what we have been calling **subjective decisions**, for these are very rich in attractive associations. It is **not** surprising that such a mind will also be *accurate* in **objective decisions**, especially the kind we have required,—for here **suggestibility** and liability to illusions are prominent factors in producing error. It is very probable that suggestibility is the **basis** of this correlation, and that the suggestible person will be **inaccurate** in objective decisions as well as slow in subjective decisions of the kind we have been using here. In the second part of this paper it is shown that subjects quick in aesthetic decisions (**Q and K**) may also be very suggestible.

Those who were quick in subjective decision are not, as **might** be expected, always quick in objective. Indeed, the **opposite** seems true; for, taking the objective decisions by themselves, the accurate subjects are as a rule slow.

Their time, however, varies considerably, and this is the important result from the practical point of view; for it shows that in the same situation, there are some subjects who can decide **both** more quickly and more accurately than others. We have **all** along been speaking of Decision Types; but it must be clear to anyone who examines the figures that there are really no separate and distinct types into which each individual must fall; but that the types, so-called, grade into each other. It is often difficult to classify a subject according to type, because he is intermediate; and if the experiment were performed upon a larger number of subjects all the apparent gaps would no doubt be filled up. When we speak of types we are doing so merely for convenience sake, and not with the intention of adopting a "Multimodel Theory."

In all probability, this test was complicated by various spatial illusions, and illusions in the estimation of number. C. T. Burnet, in his study of the "Estimation of Number,"⁵ has pointed out the tendency of some subjects to over-estimate compact groups, and of others to over-estimate scattered groups. In the second series of decisions in our experiment, the group-areas are all the same, hence the holes would be close together in

⁵Harvard Educational Review, 1931, 1, 2, 10-11.

some than in others; and this was also true of the first series, though no attempt was made to have the group-areas the same.

Another illusion that tended to produce a constant error until corrected was as follows: When the holes were farther apart, it produced the illusion that the group-area was larger. The apparently larger group was then pointed to as having more holes, when in reality it was the same size and had less holes. This was pointed out by one of the subjects after it had been the cause of several errors in his own case. Afterwards it was noticed with other subjects.

If all the subjects knew of all the possible illusions, then the estimation of the amount of the illusion might still be regarded as an objective decision. But in this particular case some subjects knew more of the illusions involved than others. We, therefore, conclude that the liability of the subject to illusion was involved, perhaps as the most important factor. If this is true, then the above result would be stated thus: Those who are quick in subjective decisions are less liable to illusion; that is, less suggestible, as we have noted above.

CONFIDENCE

The confidence of the subject, whether A, B, or C, was ascertained for each decision, as in the subjective decisions reported above. Confidence in this case means "Degree of certainty that the judgment is correct." The "numerical value" and "average confidence" were ascertained as before. The results of this confidence test are summarized in Tables XII-A and XII-B. It will be noticed that the confidence of the subjects varies greatly; and that *the confident subject is, on the whole, not more apt to be correct than the inconfident one*. In fact, subjects F and I, who have the least average confidence, have the fewest errors in the fifty difficult decisions. On the other hand *the confident judgment of any given subject is more apt to be correct than that inconfident one*,—as is shown by a comparison of columns A. C. and A. C. E. There are great individual differences in this respect, some subjects having more confidence in erroneous judg-

ments as compared with the total, than others: e.g., differences between A. C. and A. C. E. for C and K are small, while for J it is very large. The results here are slightly different from what we obtained for subjective decisions; but they possess greater reliability, because obtained from a greater number of subjects. Of course, both may be correct; that is, confidence may be correlated with consistency in subjective decisions; and at the same time show no correlation with accuracy in objective ones.

TABLE XII-A

Subject	First Series of Decisions—Easy			A. C. E.
	N. E.	T. C.	A. C.	
C	13	85	1.70	1.38
A	13	59	1.18	.92
D	13	54	1.08	.23
B	16	39	.78	.50
I	14	24	.48	.29
F	15	22	.44	0.

XII-B

		Second Series of Decisions—Difficult			
		N. E.	T. C.	A. C.	
50 Dec.	C	19	73	1.46	1.16
	A	23	35	.70	.39
	B	24	17	.34	.17
	I	16	13	.26	.13
	F	18	5	.10	0.
25 Dec.	J	9	26	1.04	.11
	K	7	19	.76	.71
	L	10	15	.60	.30

N. E. = Number of errors.

T. C. = Total confidence—"numerical value."

A. C. = Average confidence.

A. C. E. = Average confidence for errors.

A comparison of the figures shows that no correlation between time in objective decisions, and association time or time for sorting cards is brought to light. Neither does there seem to be any correlation between memory ability and time; but a good memory is generally associated with accuracy. This is shown in Table XIII. It is also what we would expect; for we have seen that accuracy in a subjective decision is correlated with accuracy in an objective decision and also with good memory.

TABLE XIII

<i>General Memory Index</i>	<i>Obj. Dec. Type</i>
30	Q—I
30	S—A
34	Q—I
37	S—I
39	S—A
39	Q—A (or I)
40	S—A
44	S—A (or I)

CHAPTER II

RELATION OF TIME CONSTANCY AND SUGGESTIBILITY IN AESTHETIC DECISIONS

The investigation was continued partly to verify the results already obtained, but chiefly to study the effect of suggestion upon decision time and constancy, and the correlation of suggestibility with these factors. Ten subjects participated in the investigation; and three of these, A, C, and L, had also taken part in the last. The same apparatus was used and the same method followed as in the previous experiment, except that this time only groups of five were used and instead of arranging them in order of preference the subject selected only the best and worst of the group.

In the previous investigation the names of the things to be decided upon were printed upon cards and exposed to the subject; this time the actual objects were used. These were: fifteen groups of colors, fifteen of color combinations, twenty of geometrical figures, twenty of pictures (reproductions of famous paintings), twenty of picture postal cards, and only the last ten of words as in the first experiment. In all one hundred decisions were obtained and these were, all except three or four in the last ten, aesthetic decisions. The subject was asked to place the best of the group on his left, the worst on his right. The five members of each group were arranged for each subject in the same way. They were marked A, B, C, D, E, and presented in this order from the subject's right to his left. The decisions for best and worst were recorded, and we were thus able to calculate the influence of position upon the subject's decision. In doing this we were obliged to omit the results for the first fifteen groups of colors and for five groups of geometrical figures, because for them the order of presentation was not recorded. The results for the remaining eighty decisions are given in Table XIV. Under the column marked "Pleasant" are shown the number of times A, B, C, D, and E cards were chosen

as best by each subject; under the column marked "Unpleasant," the number of times each card was chosen as worst by each subject; and under the column marked "Total," the number of times each card was chosen either as best or worst by each subject. The totals for all the subjects in these three columns are given in the horizontal "Total" line at the foot. By referring to the total for the "Totals" column, it will be seen that B (that is, one card from the subject's right) had the preference for position, being chosen 351 times; while A (the extreme right card) was chosen least often, 308 times; but there is much less difference here than one would expect. The C, D, and E cards were chosen 314, 313, and 314 times respectively.

If we now turn to the totals for the "Pleasant" and "Unpleasant" columns, we find evidence of a pronounced tendency to choose the cards towards the E end of the line as pleasant, and towards the A end as unpleasant; e.g., E was chosen 183 times as pleasant, 131 as unpleasant; while A, on the other hand, was chosen 162 times as unpleasant, 146 as pleasant; and B 192 times as unpleasant and 159 times as pleasant. It will be remembered that E was always on the subject's left and A on his right, and that the instructions were: "Place the best card on your left and the worst on your right." We have here, therefore, evidence of an *inertia* or tendency to leave things as they are. This inertia varies greatly with different subjects. In the table we have arranged them roughly according to its amount, which is determined by the sum of the differences between the number of times A and E were chosen as pleasant, and also as unpleasant.

TABLE XIV

Subject	Pleasant					Unpleasant					Totals				
	A	B	C	D	E	A	B	C	D	E	A	B	C	D	E
O	14	14	18	11	23	16	17	17	19	11	30	31	35	30	34
R	12	18	11	16	23	16	19	15	17	13	28	37	26	33	36
C	11	13	22	18	16	18	16	13	20	13	29	29	35	38	29
N	14	15	16	18	17	18	20	11	17	14	32	35	27	35	31
A	14	18	15	20	13	20	17	14	17	12	34	34	29	37	25
Q	14	15	18	14	19	18	22	13	10	17	32	37	31	24	36
P	15	16	20	8	21	15	16	18	16	15	30	32	38	24	36
S	19	18	11	13	19	14	22	16	17	11	33	40	27	30	30
M	18	14	15	14	19	16	21	10	17	16	34	35	25	31	35
L	15	18	18	16	13	11	22	23	15	9	26	40	41	31	22
Total	146	159	164	148	183	162	192	150	165	131	308	351	314	313	314

It would seem, then, that the influence of position alone, judging from the totals for all subjects, is almost a negligible factor in this experiment. To be sure, B was chosen thirty-seven times oftener than any other card; but this predominance is almost entirely accounted for by its greater frequency in the "Unpleasant" group, where it appears twenty-seven times oftener than any other card. The influence of position *plus* instruction to place the best on the left and the worst on the right is, however, very marked, and, as we have pointed out, more noticeable with some subjects than others. This influence of position plus instruction may perhaps be explained in terms of suggestibility. If so, the most suggestible subjects would be most susceptible to it. This, however, does not seem to be the case; for a later arrangement of the subjects according to suggestibility does not follow the one given here. It may be, of course, that the later arrangement is at fault; but, on the whole, it would seem better to consider this result due to a tendency more pronounced in some subjects than others to leave things as they are when a decision would be difficult.

The influence of position alone, which may truly be called suggestion, also varies greatly with different subjects. This difference is brought out by a comparison of the figures for the various subjects in the "Totals" columns. In the case of each subject, the position least often chosen is subtracted from that most often chosen. The difference is taken as a numerical value of the influence of position, and in Table XV the subjects are arranged according to its amount. There seems to be an inverse correlation between suggestibility for position and the inertia discussed above; for in Table XIV, the subjects were arranged according to this inertia from the greatest to the least; here they are arranged from the least suggestibility for position to the greatest, and their relative positions are only slightly changed. This adds weight to the view that the influence of position plus instruction is not due to suggestibility, but to what we have termed inertia. The arrangement according to suggestibility for position does not, however, correspond to that later obtained

for suggestibility. This lack of correlation may arise because either one or both of the experiments are inadequate as tests for suggestibility. It is, of course, in perfect harmony with the view that there is no *general suggestibility*, but only varying *degrees of suggestibility* for different things.

In Table XV, column D, are given the differences referred to above; in column M, the position of the card most often chosen; and in column L, the position of the card least often chosen. It will be noted that the subjects differ considerably as to the position which attracts them most and least. Five of the subjects find B the most attractive position; three, C; and two, D. It is remarkable that neither A nor E, the two end cards, find a place. They occupy a mediocre position; for E occurs only twice and A once in the "least frequent" column. In the latter column, C occurs five times, each time being the least attractive position to a subject who found B the most attractive. B occurs in the L column not at all; and this position, that is, one from the right, has, therefore, the greatest attraction for the attention. If the instructions had been merely "select the best and worst" instead of "place the best and worst at your left and right," the attractiveness of the various positions might have been different; for the words "left" and "right" may have produced some effect upon the attention of the subjects.

TABLE XV

Subject	D.	M.	L.
O	5	C	A, D
N	8	B, D	C
C	9	D	=
M	10	B, E	C
R	11	B	C
A	12	D	E
Q	13	B	D
S	13	B	C
P	14	C	D
L	19	C	E

D = Difference between the number of Decisions for the card most often chosen, and the number for the card least often chosen.

M = Position most often chosen.

L = Position least often chosen.

Each of these hundred groups was presented to each subject for a second decision, after a period of six to eight weeks. Fifty

of them were presented in precisely the same manner as at first. The other fifty were presented each time with a suggestion to the subject to choose certain cards. The number of times that each card was chosen as best or worst in the first presentation was ascertained for each of the hundred groups. Sometimes the same card was chosen as best or worst by almost all of the ten subjects; e.g., colors, group 4, pleasant: 7A, 0B, 2C, 0D, 1E; unpleasant: 2A, 0B, 1C, 0D, 7E. This means that seven subjects chose A as pleasant; none, B; two, C; none, D; one, E; and similarly for the unpleasant. In other groups, a card that was chosen best by several subjects, was chosen worst by the rest; e.g., pictures, group 18, pleasant: 2A, 4B, 3C, 0D, 1E; unpleasant: 1A, 5B, 1C, 2D, 1E. In many groups, however, there was a much greater division of opinion; e.g., post cards, group 8, pleasant: 1A, 3B, 0C, 6D, 0E; unpleasant: 4A, 1B, 1C, 0D, 4E. Groups of the last kind were as far as possible, selected for "suggestion groups." These groups were presented at various intervals between "non-suggestion groups," sometimes alternately and sometimes two or three of one kind successively and then one of the other. With the non-suggestion groups the instruction was as in the first presentation: "Place the best on your left, the worst on your right"; but with the suggestion groups the following was always added: "The card most often chosen best is at your left, that most often chosen worst on your right." That is, it was explained to the subject that in certain groups the cards as presented to him would be arranged in a special order, so that the card chosen best by the majority of the ten subjects would be presented to him at the left, the worst on his right. This constituted a suggestion to the subject to leave the cards as they were, but he was left free to make a change if he so desired. This suggestion was adopted because it seemed to be one that could be used continually throughout the investigation, while its effect would remain practically undiminished.

The cards actually placed on the left and right in the suggestion groups were sometimes those chosen best and worst by the majority, sometimes not; e.g., in the illustration of a suggestion

group above, if a subject had previously chosen B as the best and E as the worst, he would be given D as the best and A as the worst; that is, a card that was really chosen the majority of times as best, on the left; and a tie for majority choice, on the right. If, however, the subject, himself, belonged to the class constituting the majority (that is, if he had chosen D as the best and A as the worst), he would be given B and E or C. The main object was never to place on the left and right the cards previously chosen by the subject himself as best and worst; the latter cards were always placed in positions B and D respectively. Thus, whenever the subject accepted the suggestion, he would have to change his previous decision to do so. We were thus able to calculate the effect of the suggestion, by comparing the number of changes in each subject's decisions for the fifty "non-suggestion groups" with his number of changes for the fifty "suggestion groups."

In Table XVI, the results are given for the fifty non-suggestion groups. The subjects are arranged according to the average decision time for the first presentation of the whole hundred groups. The average time for the second presentation of the fifty non-suggestion groups, and the number of decision changes in these groups are also given. In columns "a" and "d" are the number of changes in decision upon agreeable and disagreeable cards respectively. The "total" number of changes in the former is about the same as in the latter; but the individual subjects vary considerably. Subject R, e.g., has seven more changes in the "d" column, while Q and S have each nine more changes in the "a" column. This difference probably depends upon the amount of feeling produced by the agreeable or disagreeable cards. If the former tended to attract the attention more than the latter, there would be more differences in the choice of the latter, and vice versa. On the whole, the disagreeable cards were found by the subjects more unpleasant than the agreeable were pleasant, and thus the greater total number of differences in the "a" column is accounted for; but, as has been noted, there are marked individual differences here.

Turning now to a comparison of the total number of changes, we find that they range from 21 to 43. We have, then, as before, a constant and an inconstant class; but it is even more difficult here than in the previous investigation to draw any dividing line between these two classes. They grade into each other through a series of very small differences. The same is true of rapidity of decision. We get in column A. T.¹ a very great range of decision time—from 9.1" to 32.8"; yet it is difficult to draw a line separating the quick from the slow. There is a fairly large middle gap between C (14.6") and P (17.0"); but with a larger number of subjects all gaps would probably disappear; and the dividing line between the fast and the slow would be purely arbitrary.

TABLE XVI

<i>Subject</i>	<i>A. T.¹ (100)</i>	<i>A. T.² (50)</i>	<i>No. C= (a d)</i>		
R	9.1	10.4	43	18	25
Q	10.8	12.5	43	26	17
A	12.7	12.3	21	8	13
S	13.0	13.9	39	24	15
C	14.6	8.7	21	11	10
P	17.0	13.5	32	19	13
M	18.1	14.8	30	15	15
L	20.8	16.9	35	17	18
O	24.5	17.1	29	13	16
N	32.8	27.2	37	17	20
Total.....			168	162	

A. T.¹ (100) = Average time for the first Decision on 100 groups.

A. T.² (50) = Average time for the second Decision on 50 "Non-suggestion groups."

No. C= = Number of Decision changes in Non-suggestion groups.

a and d = Number of changes in Decision for agreeable and disagreeable cards respectively.

The mean average time for all the subjects is less for these aesthetic decisions than for groups of five in the last investigation—17.34 and 19.30 respectively. It will be remembered that in the last experiment the five cards were arranged in order, while here only the best and worst were selected. One would suppose that this difference in requirement would have effected a saving of more than two seconds; but probably the selection of the first and the last is the most difficult part of the arrangement. The actual saving may be really larger than the figures show (i.e., 2"); because for the only three subjects that participated in both inves-

tigations it is 5.2"; and the figures of Table XIX seem to indicate that the effect of practice upon decision time is very small if not altogether absent. The saving for subjects A and C is much greater than for L, whose time is only slightly quicker in the aesthetic decisions. Of course, the difference in time may be due to difference in material, as well as to difference in the decisions required. The time for arranging a group of three in the last experiment was 8.5". This, therefore, is about the difference we would expect between arranging a group of five in order, and selecting the best and worst from a group of five. The actual difference is about 3" less than this. It is, therefore, probable that the aesthetic decisions on actual objects would, other things being equal, require more time than the previous more or less logical decisions.

As in the previous experiment, the decision time for the second presentation is usually less than for the first; but here again there are two exceptions, R and Q. Subjects C and L both decrease their time about as much relatively as before, showing that this is a constant tendency with them, as no doubt to increase, decrease, or remain the same is with every subject. These individual differences are very difficult to interpret; but see the discussion of this subject above (pp. 8-9).

The results for the second presentation of the fifty "suggestion groups" are given in Table XVII. The subjects are arranged roughly on the basis of suggestibility. The purely suggestible are placed first; then those that likewise give evidence of contra-suggestibility, and finally the non-suggestible. The average times for the "suggestion groups" are in column A. T.²⁺. The 2 means second presentation, and the plus sign means "with suggestion." just as in Table XVI the "equals" sign means "without suggestion." In column C+ (a d), we have first the total number of changes in decision for the fifty groups, and then the number of changes in the decisions upon "agreeable" and "disagreeable" cards. Contrary to what we obtained in Table XVI, we see that the total number of changes is greater for the "d" column than for the "a" (220 and 203), which signifies that the sugges-

tion was accepted oftener in the case of the unpleasant than the pleasant choice; but here again there are some individual differences.

There is *less* variation in *time* and *more* in the number of changes here than in the "non-suggestion groups"; but we shall enter into a more detailed comparison of the two groups in the discussion of Table XVIII. Here we wish to bring out chiefly the method of determining the relative suggestibility of the subjects. The best way of doing this would seem at first glance to be to compare the results (especially the number of changes) for the suggestion groups with those for the non-suggestion groups. This procedure, however, presupposes that the number of changes would have been the same for each series, if there had been no suggestion; and we are not at all certain that with a limited number of cases this is true. A form of procedure somewhat less open to such a criticism is to consider the results of the suggestion groups by themselves. An illustration will best explain how suggestibility can be calculated in this way. Let us suppose that E, D, C, B, A, is a group, presented to the subject in this order from left to right; then E and A are the suggestion cards for best and worst respectively, B and D the subject's previous choices for best and worst respectively, and C the only neutral card. If B and D are selected as before, the subject is constant; but he produces a change by any of the following decisions or combinations thereof: (1) by accepting the suggestion; (2) by choosing C as either best or worst; (3) by reversing the suggestion,—that is, taking A as best and E as worst; and (4) by reversing his own previous decision,—that is, taking D as best and B as worst. The last possibility is practically negligible, as it occurred only once or twice in the whole investigation. No. 3 was taken as an evidence of contra-suggestibility; for the possibility of such a decision, if there were no suggestion, would be very low, since E and A were really chosen by at least three subjects, sometimes by many more, as best and worst respectively, and therefore most probably really differ in this way. This kind of decision is signified by the sign—; No. 2, showing non-suggestibility, by the sign =, and No. 1,

showing suggestibility, by the sign +. Changes in decisions for both best and worst are counted two; a change in either one of them, one. The results are summarized in column C+, Table XVII. Subject R, e.g., has a total of 51, changes, 38 due to acceptance of the suggestion, 9 to non-acceptance, and 4 to reversal. Since acceptance is due to the choice of either of two cards (E or A), and non-acceptance to the choice of only one (C), there would be, without any suggestion, twice as many acceptances as non-acceptances. Therefore, suggestibility is shown in proportion as the + number in column C+ is more than twice as great as the =. Subject R, e.g., shows considerable suggestibility, for his + number (38) is more than *four* times as large as his = (9).

TABLE XVII

Subject	A. T. ² + (50)	C+ (a d)		C+		
R	10.0	51	27 24	38+	9=	4—
Q	13.3	63	29 34	44+	15=	4— Sug.
P	14.1	38	19 19	30+	7=	1—
O	18.2	44	18 26	36+	4=	4—
L	19.1	52	27 25	35+	8=	9— Sug. and
C	10.0	39	19 20	19+	10=	10— Contra-sug.
M	16.7	46	19 27	24+	15=	7—
N	26.5	27	14 13	14+	8=	5— Contra-sug.
A	12.9	30	12 18	17+	10=	3— Non-sug.
S	14.1	33	19 14	22+	11=	
Total.....		203	220			

A. T.²+ (50) = Average Decision time for "suggestion groups."

C+ (a d) = Total Decision changes, changes in choice of agreeable and of disagreeable cards.

C+ = For explanations of this column see pages 34, 35.

Sug. = Suggestible.

Contra-suggestibility is shown to some degree by all the subjects except S and P. The former is also not at all positively suggestible, while the latter is to a considerable degree. Subject L is both negatively and positively suggestible, while C, M, and N are only negatively suggestible. It would seem, then, that negative and positive suggestibility need not be inversely correlated in the individual but that any degree of one can coexist with any degree of the other. The fact that the same subject may be both

positively and negatively suggestible is not at variance with ordinary observation. We all know individuals who alternate between periods in which they are easily influenced and periods of inflexibility.

There are, as we have intimated, other ways of estimating the suggestibility of the subjects from the results of our experiment. These are perhaps, less reliable than the one explained above, but they afford us very important auxiliary evidence regarding the status of the individual subjects; and also, since they depend upon a comparison of the results of the "suggestion" and "non-suggestion" groups, lead to a consideration of the influences of suggestion upon decision. This comparison is facilitated by the use of Table XVIII. Here the subjects are arranged according to the average time for the first decisions upon the hundred groups. In columns two and three are presented the average times for decisions upon the "non-suggestion" and "suggestion" groups respectively. In only two cases (R and N) is the time for the suggestion groups the shorter, and then only very slightly so. The other subjects differ considerably; for some (such as L, M and C) the time is relatively much longer for the suggestion groups; for others (Q, P, S, A, and O) it is only slightly longer. The lengthening of this time shows a general disturbance due to the suggestion, and probably an attempt to overcome it. It may, therefore be taken as an evidence of some degree of suggestibility; and it is noteworthy that it is most pronounced in the case of the subjects that show the greatest contra-suggestibility. That is, the process of resisting increases the decision time. A shortening of the time cannot, however, be taken as an evidence of the absence of suggestibility. It may, on the contrary, be explained as the result of an immediate yielding to the suggestion, as it seems to be, e.g., in the case of subject R. The subject who shows the least effect of the suggestion, viz., S, shows also the least difference in time.

The variation from the central tendency in decision time is greatest for the first presentation of the hundred groups, the limits being 9.1" and 32.8". It is less for the second presentation

of the "non-suggestion groups," where the limits are 8.7" and 27.2"; and it is least of all for the second presentation of the "suggestion groups," where the limits are 10" and 26.5". Hence the general effect of suggestion upon time in decision is to lengthen it, and reduce the variability of one subject from another.

Another method of calculating the relative suggestibility of the subjects is to compare the number of changes in non-suggestion and suggestion groups, as mentioned above (p. 34). These are given in Table XVIII, columns C= and C+ respectively. In only two cases (N and S) is the number of changes less in the C+ than in the C= column. The other subjects vary greatly, showing increases that range from 6 for M and P to 20 for Q. If we estimate the relative suggestibility of the subjects from the amount of the increase in the number of changes, the classification will be about like that of Table XVII, except that subject C will be more and P less suggestible.

As might be expected, the suggestions on the whole increased the inconstancy. It increased it, however, much more for some subjects than for others. The limits in the C= column are 21 and 43, while in the C+ column they are 27 and 63. This increase of inconstancy bears no marked relation to the inconstancy as determined by the fifty non-suggestion groups. Subject C, e.g., who was constant in the latter, has a relatively larger increase than subject R, who was inconstant; and the two subjects who actually decreased the number of changes were relatively inconstant. On the other hand, large *increases* were made by the relatively inconstant subjects Q and L.

In the second C+ column, we have presented the number of +, = and — changes as in Table XVII; and in column C=(+)C+, the total number of changes (+, = and —) for the suggestion and non-suggestion groups combined. A change in any of the latter groups is produced by the selection of any one of *three* possible cards (excluding the possibility of the subject reversing his own previous decision). All changes in non-suggestion groups are, of course, = changes. Taking then any

two decisions, one without and one with suggestion, we have four possibilities of = changes, three in the former, one in the latter; and two of + changes, both in the latter. That is to say, in the whole hundred decisions we should have, barring the influence of suggestion, twice as many = as + changes; and the degree of suggestibility will be proportioned to the amount that the + changes exceed half the = changes. If we arrange the subjects on the basis of this calculation, we shall, on the whole, maintain the classification of Table XVII; but the positive suggestibility of subjects C and M and the negative suggestibility of subject N are thus better brought to light.

TABLE XVIII

TABLE XVIII														
Sub- ject	(100) A.T. ¹	(50) A.T. ²	(50) A.T. ³	C=	C+		C+			C=(+)	C+		Sug.	
R	9.1	10.4	10.0	43	51	38+	9=	4-		52=	38+	4-	3+	
Q	10.8	12.5	13.3	43	63	44+	15=	4-		58=	44+	4-	3+	
A	12.7	12.3	12.9	21	30	17+	10=	3-		31=	17+	3-	2+	
S	13.0	13.9	14.1	39	33	22+	11=			50=	22+		=	
C	14.6	8.7	10.0	21	39	19+	10=	10-		31=	19+	10-	2+ -	
P	17.0	13.5	14.1	32	38	30+	7=	1-		39=	30+	1-	3+	
M	18.1	14.8	16.7	30	46	24+	15=	7-		45=	24+	7-	2+ -	
L	20.8	16.9	19.1	35	52	35+	8=	9-		43=	35+	9-	3+ -	
O	24.5	17.1	18.2	29	44	36+	4=	4-		33=	36+	4-	3+	
N	32.8	27.2	26.5	37	27	14+	8=	5-		45=	14+	5-	-	
	17.3	14.7	15.5	330	423	Total							
Mean of Averages														

A. T.¹ = Average time for first 100 Decisions.

A. T.² = Average time for second Decisions on 50 "non-suggestion groups."

A. T.³ = Average time for second Decisions on 50 "suggestion groups."

C= = Decision changes in non-suggestion groups.

C+ = Decision changes in suggestion groups.

For second C+ column and C=(+)C+, see pp. 37-38.

Sug. = Index of suggestibility.

There are thus three ways of calculating suggestibility if we omit the comparison of time, which has a rather ambiguous significance. If the suggestibility shown in any one of these ways is called 1+, an arbitrary index of suggestibility for each subject is obtained. This is given in column "Sug.," Table XVIII. 3+ means that suggestibility is shown by all three methods; 2+, by two; =, by none; 2+ - means suggestibility by two methods coupled with contra-suggestibility; -, contra-suggesti-

bility only, etc. The subjects seem to fall into four groups or types on the basis of suggestibility: (1) the positively suggestible; (2) the positively and negatively suggestible; (3) the non-suggestible; (4) the negatively suggestible. The last has only one representative (N) in this group of subjects. No. 3 seems to have only one also (S); but since for A the margin for suggestibility is very low in both methods in which it occurs, he should probably be placed in this type instead of No. 1. It is, of course, understood that these types are not distinct, but grade into each other as the "decision types" considered above.

There appears to be no correlation between suggestibility and decision time or constancy. There are quick and slow, constant and inconstant in the two types of suggestibility that are fairly represented; and this would probably also be the case with the other two, if there were more subjects. There may, however, be some significance in the fact that the contra-suggestible appear, on the whole, in the lower half of the column, and two of the most suggestible at the top. If so, it would mean that extreme quickness in decision time usually goes with suggestibility, and extreme slowness with contra-suggestibility, while the non-suggestible would be medium or fair in decision time. We have not, however, sufficient data in support of this view.

So far we have been considering the results of all the decisions combined. Let us now turn to a comparison of the results for the different kinds of objects decided upon, viz., colors, geometrical figures, pictures, post cards, and words. There were thirty groups of colors, ten of words, and twenty each of the others. Each subject's average decision time for each kind of material decided upon and also his average for the whole hundred are given in Table XIX. The central tendency of all the subjects for each series is also given at the foot of the table. On the whole, the subjects show a decrease of time from colors to geometrical figures, an increase from these to pictures, a still further increase to post cards, and a final decrease to words. The groups were presented to the subjects in this order; and if they were all of approximately equal difficulty, one would expect a decrease of

time from one series to the next due to practice. The increase can, therefore, be accounted for only by the greater difficulty of the decisions upon pictures and especially post cards. It is most probable also that practice would have very little effect upon decision times.

The above states the general tendency; but there are very marked individual differences in the way the subjects vary in time from one series to another. Subjects R, Q, and A vary from the central tendency in showing an increase of time from colors to geometrical figures; C and M, in showing a decrease from geometrical figures to pictures; S, in showing a decrease from pictures to post cards; and R and L, in showing an increase from post cards to words. That is to say, although on the whole geometrical figures are the easiest to decide upon, yet subjects R, Q, and A find colors, and M, words, slightly the easiest, and C, the pictures much the easiest. These variations are significant, for they seem to show that decision times vary relatively with the kind of material,—and this means that a person may be relatively quick in one kind of decision and relatively slow in another. Such variations are, however, the exception. One subject, R, shows increase for every series, and Q and A, for all except the last, while C and M show decreases for every series except the post cards. It is interesting to note that the former are also the subjects that show the *increase* in decision time for the second presentation, as are the latter those that show the greatest decrease. This correlation is no doubt due to the greater adaptability of the latter.

The coefficient of variability (column C. V.) is obtained for each subject by dividing the mean variation of his times for the five series by his average time for the whole hundred decisions. While this coefficient is probably not so accurate as that which could be obtained by finding the mean variation for each subject's times for the *hundred* decisions, yet it is sufficient for our purposes. The most "uniform" subjects are M and A, who have C. V.'s of .12 and .13 respectively; and the most variable are N and C, with C. V.'s of .36 and .31. Subjects A and C were also

"uniform" and "variable" subjects respectively in our first investigation; and we also note here, as we did then, that there is no correlation between a subject's variability in his own decision time and his variability from the average for all the subjects. This result is significant; for it is thus possible to obtain subjects who are not only quicker than the average but also more uniform in time. Variability in time is not correlated with decision time, constancy, nor suggestibility; but if we refer to Table XIV, we shall see that the subjects who have the greatest C. V., show as a whole the greatest influence of what we have there termed "Inertia." This is not surprising, for the "uniform" subjects are those that standardize their time by *easily* bringing into play in difficult situations large amounts of reserve energy; while the variable subjects either have not the extra energy to expend or find it difficult to force into activity; that is, their inertia is great.

TABLE XIX.

Subject	A.T. ¹ (100)	C. (30)	G. (20)	Pict. (20)	P.C. (20)	W. (10)	C. V.
R	9.1	6.9	8.1	8.8	10.5	15.7	.25
Q	10.8	8.1	9.2	12.1	12.6	12.3	.17
A	12.7	10.7	11.5	11.9	15.5	14.1	.13
S	13.0	11.6	11.4	17.8	16.0	11.4	.19 +
C	14.6	18.6	11.3	8.3	20.2	10.7	.31
P	17.0	14.1	12.5	16.7	25.2	19.1	.21
M	18.1	21.8	17.5	16.0	18.2	13.7	.12
L	20.8	18.9	14.3	21.1	26.1	28.4	.20
O	24.5	29.4	18.5	21.9	28.5	19.0	.19 —
N	32.8	26.6	20.1	36.2	57.9	20.5	.36
Mean Averages	17.3	16.7	13.4	17.1	23.1	16.5	

A.T.¹ (100) = Average time for the first decision on 100 groups.

C. (30) = Time for thirty decisions on colors.

G. (20) = " " twenty " " geometrical figures.

Pict. (20) = " " " " " pictures.

P. C. (20) = " " " " " post cards.

W. (10) = " " ten " " words.

C. V. = Coefficient of variability.

We pass now to a comparison of the results of the different series in constancy and suggestibility. In Table XX, we have summarized these results, combining the "suggestion" and the "non-suggestion" groups. Column 1 contains the changes of each subject for the whole hundred decisions; those in which the

TABLE XXI

<i>Subject</i>	<i>Constant</i>	<i>Inconstant</i>
A		
C	Pict. & Geom. Fig.	Post Cards
O	Pictures	Geometrical Figures
P	Colors	Geometrical Figures
L	Pictures	Geometrical Figures
M	Post Cards	Colors
N	Geometrical Figures	Colors
S	Geometrical Figures	Post Cards
R	Pict. & Post Cards	Colors
Q	Post Cards	Pictures

The influence of the suggestion is shown by the + numbers in Table XX; and, as explained above, the degree of suggestibility by the amount which the + numbers exceed half the = numbers. If we wish to compare the influence of suggestion in the various series, we must compare not the total numbers but their relation to the = numbers. Thus, for a total of 427 = numbers, there are 279 + numbers; that is, an excess of 66 over half the = numbers. The following are the estimated excesses at the same ratio and the actual ones:

	<i>C.</i>	<i>G. F.</i>	<i>Pic.</i>	<i>P. C.</i>	<i>W.</i>
Estimated	20.8	13.5	12.2	14.5	4.8
Actual	18.5	29.0	8.5	4.0	5.5

If the force of the suggestion were wearing off, we would expect a gradual decrease from colors to words; but the rate is really slightly greater for words than for colors; and we are, therefore, justified in saying that the very low figures for post cards and for pictures cannot be due to a diminution of the force of the suggestion. On the other hand by far the greatest suggestibility is shown in the case of geometrical figures. An explanation that suggests itself is that the decisions were on the whole more indifferent here; but, if inconstancy means indifference, then the post cards in that they show the most inconstancy are the most indifferent. Yet they show the least influence of suggestion. The greater influence of suggestion in the case of the geometrical figures must, therefore, remain unexplained.

Contra-suggestibility is shown by the — numbers, and here also their relation to the = changes must be considered for com-

parison. The following are the figures estimated from the ratio of 427= to 47—; and also the actual figures:

	C.	G. F.	Pic.	P. C.	W.
Estimated	15.0	9.7	8.7	10.4	3.4
Actual	11.0	13.0	5.0	13.0	5.0

The contra-suggestibility is large not only for the geometrical figures, which showed also the greatest effect of suggestibility, but also for the post card series, in which direct suggestibility was least. Contra-suggestibility is small for colors and pictures.

Many of the subjects show marked deviations from the general tendencies, so far considered. These variations are brought out better in Table XXII, where the series in which each subject shows the most suggestibility, the least suggestibility, and the greatest contra-suggestibility are given. The words are omitted. The individual differences here shown need not be further commented upon. An enumeration of the various series mentioned shows that there is most suggestibility for geometrical figures; and both least suggestibility and most contra-suggestibility for post cards. These results are in accord with those of W. D. Scott¹ and add weight to his view that suggestibility is not a general trait; but that there exist only varying degrees of specific *suggestibilities*.

TABLE XXII

Sub- ject	Most Sug.	Least Sug.	Contra-Sug.
A	Geometrical Fig.
C	Geometrical Fig.	Post Cards	Post Cards & Pict.
O	Pictures	Geometrical Fig.	Post Cards
P	Colors & Post C.	Pictures
L	Colors	Post Cards	Pictures
M	Post Cards	Geometrical Fig.	Post Cards
N	Geometrical Fig.	Pictures	Geometrical Fig.
S	Geometrical Fig.	Post Cards
R	Geometrical Fig.	Colors
Q	Post Cards	Colors

The subjects were not asked for introspection upon the influence of the suggestion during the experiment; but very often it was given gratuitously. Of the suggestible subjects, R denies

¹ W. D. Scott. Personal Differences in Suggestibility. Psych. Rev., 1910, pp. 147-156.

any influence, but Q, O, and P acknowledge it. Subject L, who is both positively and negatively suggestible acknowledges both tendencies in his introspection; S and A deny any effect of the suggestion; and the other subjects do not mention the fact at all. On the whole the introspection points to the correctness of our classification. At the end of the investigation the subjects were questioned regarding the influence of the "special arrangements," and whether they guessed the purpose of the experiment. Most of them said they did not think of the latter at all; and none of them ever thought they were being deceived regarding the card the majority had chosen as best and worst. All of them felt surprised that they should differ from the majority so often; but the introspection on the influence of the "special arrangement" differs from subject to subject,—in general, verifying our classification as to suggestibility. Some of the more suggestible subjects (e.g., O and P) said they often took the cards suggested because it was the easiest thing to do when a decision would be difficult. Subject M said he felt influenced against the suggested cards; and both C and N acknowledge considerable pride in differing from the majority; these three were classified as contra-suggestible. Subject S (non-suggestible) said he felt sometimes like defending the card the majority thought was *worst*. A knew more of the purpose of the experiment than the others; and his suggestibility is probably greater than calculated here. It is interesting to note that very often when a subject accepted the suggestions, he remarked that he remembered choosing the same card before, when of course he was really making a different choice.

The subjects were asked, especially in the first presentation, for introspection upon the decision itself; but this was not emphasized as the investigation has no direct bearing upon the introspective analysis of the "Act of Decision" itself. The material obtained, nevertheless, shows that the decisions were always brought about in one of the following ways: (1) it succeeds an immediate feeling attitude, which may be of various kinds but not further explicable; (2) the feeling is explicable,—

that is, the subject can tell *why* the object is pleasant or unpleasant, what factors in the situation produce it, etc.; (3) the feeling is aroused not by the object itself, but by something, more or less closely associated with it. The one invariable factor in these aesthetic decisions is the feeling. The nature of the feeling itself, the *kind of object* which produces it, and the kind of association which arouses it, all differ greatly from subject to subject. Many individual differences in this respect might be obtained by a careful study of the introspection and the nature of the objects chosen by each subject; but the results would scarcely repay the immense amount of work necessary to obtain them, especially as this is not an investigation in the psychology of aesthetics.

ACCURACY OF DECISION

The subjects were tested for accuracy of decision by the same method used in the last investigation. This time only cards containing from 44 to 55 holes were used for the comparisons; and the group areas were always the same, but the subjects were not informed of the latter fact. In the previous experiment we noted that certain illusions produced constant errors. In order to overcome this factor as far as possible, the subjects were always told whether their decisions were right or wrong, and were thus enabled to correct their judgments by experience. Two series of fifty decisions each were obtained. In one the time for each decision was taken with a stop watch; in the other a constant time of 5" was given for each.

The results for both series are given in Table XXIII. There is a very great variation in time: from 2.6" to 12.8"; and, contrary to our last investigation, there seems to be a pretty close correspondence between the times in the constancy of decision test, and those in the accuracy of decision test. In the Table the subjects are arranged according to time; and the first five here are, with one exception, the same as the first five in Table XVI, where they are arranged according to time in the constancy of decision test. This correlation may be due to the fact that the two kinds of

decision are more alike in this investigation. Aesthetic decisions in which the *actual objects* are compared are probably more like the decisions we have required for accuracy than are the more logical decisions of the first investigation.

TABLE XXIII

Subject	A.T.(50)	E. ¹ (50)	E. ² (50)	T.E.	C.V.
A	2.6	18	19	37	.23
Q	4.0	23	19	42	.32
R	5.1	17	18	35	.31
M	5.2	18	18	36	.33
C	5.5	18	17	35	.27
O	6.3	22	15	37	.26
P	7.3	24	19	43	.44
L	7.9	17	18	35	.38
N	9.0	24	28	52	.56
S	12.8	18	12	30	.47
		199	183	382	

A.T. = Average time for fifty decisions.

E.¹ = Errors for the fifty in which time was taken.

E.² = Errors for the fifty in which the time was constant (5").

T.E. = Total errors.

C.V. = Coefficient of variability.

In column E.¹ are the number of errors for each subject in the fifty decisions in which the time was taken; and in E.² the number for those in which the time was a constant quantity (5"). The number of errors is on the whole less for the latter. This is probably not because the time was allotted, but because some of these decisions were really easier than in the other series. The difference between the number of holes in the two cards presented was usually from five to 0; but in twenty-five pairs of this series, it was from 10 to 0. Corresponding to this *objective* difference, there was a greater *subjective* ease in the decisions; for in these twenty-five decisions there were a total of 76 errors, while in the other twenty-five of the series there were 107. In the series in which the subjects were told to decide as quickly as possible and the time measured, the errors for the second twenty-five numbered 103. This series was the first given, and we should have expected a decrease in the number of errors through practice. Hence, the increase from 103 to 107 errors for the same twenty-five decisions shows that the allotment of a constant time (5") on the whole *decreased* the accuracy. Of course the time given

(5'') hastened only the relatively slow subjects, while some of the quick ones were given extra time. The noticeable thing is that *the lengthening of the time for the quick subjects did not on the whole effect their accuracy, while the shortening of the time for the slow subjects seems to have, in some cases, actually increased their accuracy*; e.g., subjects O, P, and S, whose times are much over 5'', make fewer errors when compelled to decide in a hurry; N and L, however, make more. These statements are verified by the results for the twenty-five decisions that were the same in each series, as well as by Table XXIII. A possible explanation is that the most reliable judgments are based upon the immediate *feeling of difference*, that the time required for the development of this feeling is different with different subjects; and that, if left to themselves, some subjects tend to go by this feeling, while others over-rationalize and by trying to consider all the pros and cons in the situation greatly lengthen their time and on the whole decrease their accuracy. Such subjects do better if hurried; but of course there is also danger of over-hurrying; for if the time is so short that the feeling of difference has not completely developed, the errors may be again increased. All this is merely hypothetical, but the actual increase of accuracy with decrease of time observed in some cases adds weight to the view that there may be an *optimal judgment time*. V. A. C. Henmon says: "There may be an optimal time for judgment, varying with individuals and with varying stimuli and conditions, and judgments made too soon or too late are apt to be wrong."² This view could be experimentally tested by getting decisions on the same material with times longer and shorter than the time taken by each subject when left to himself.

The total number of errors in comparison vary from 30 for S to 52 for N. As before, we find no correlation between accuracy and time; and the four types,—quick accurate, quick inaccurate, slow accurate, and slow inaccurate, are, therefore, represented but not clearly demarcated.

As in the last investigation, we find some evidence of a correla-

² The relation of the time of a judgment to its accuracy, *Psych. Rev.* 18, 1911, pp. 186-201.

tion between quickness in subjective decisions (here aesthetic) and accuracy in objective; but this correlation is not as marked as before. It is shown by the most accurate and the most inaccurate subjects, but not by the intermediate. The most accurate subject in the list (S) is quick in subjective decisions and slow in objective; but capable of doing just as well if compelled to make the latter decisions more quickly. This subject is also the least suggestible. There is no perfect correlation brought out between suggestibility and accuracy; but three of the most suggestible subjects (Q, P, and O) are also inaccurate.

A coefficient of variability is obtained for each subject in the following day: The probable error is first found by arranging the decision times in order of magnitude, counting off one-fourth of the cases from each end of the series and halving the difference between the two values thus obtained.³ This probable error for each subject is then divided by his average decision time. The slowest subjects here have the largest coefficients. This was not the case in aesthetic decisions. Moreover, there is no complete correlation between the coefficient of variability here and that for aesthetic decisions. Subjects S and M, who have very low coefficients in the latter, have large ones here; and R and C are just the reverse. The other six subjects are about the same *relatively*; but the C.V. is, on the whole, larger in this experiment. There is some appearance of a correlation between variability in decision time and inaccuracy. There are three very variable subjects; two of these are the most inaccurate in the list; while, curiously enough, the other (S) is the most accurate. On the whole, therefore, we may say that variability is here, as in the previous experiment, an independent factor, not *definitely* correlated with either time, accuracy, or suggestibility.

As in the previous similar experiment, each subject was asked to state his confidence as A, B, or C, in each decision; the meaning of these symbols and the method of evaluating them has been described above. In Table XXIV are given in numerical values, the total confidence of each subject for the hundred

³ See Whipple's "Manual of physical and mental tests," page 18.

decisions, and his average confidence for correct decisions and for errors. The subjects are arranged in the order of their total confidence, with the most confident first. If this total confidence for each subject is compared with his number of errors in column T. E., the previous observation that "the *confident* subject is no more liable to be correct than the *inconfident*" is amply verified. In fact, two of the most confident subjects (P and N) are the two most inaccurate. The least confident subjects come next; and the subjects with *medium confidence* have the highest accuracy.

If now the average confidence for correct decisions be compared with that for errors, the former conclusion that a *confident judgment of any given subject is more liable to be correct than a less confident one by the same subject*, is also verified. The difference between the mean average confidence for correct (1.1) and for incorrect (.8) decisions is, in general, less than might be expected; but this difference is much greater for some subjects than for others. The most confident subjects show the least differences; and the greatest differences are shown by A, O, and L,—relatively inconfident subjects. In other words, a subject who is by nature very confident is just about as confident when he is wrong as when he is right; while one who by nature is of medium or little confidence varies greatly from his correct to his incorrect decisions. In the latter case the probability of being correct increases with the confidence; in the former it does not.

Henmon in his work on judgment studied the relation of confidence to accuracy, and concludes that "There is a positive correlation on the whole between degree of confidence and accuracy; but the degree of confidence is not a reliable index of accuracy."⁴ This is in perfect accord with our finding that the confident judgment of a given subject is more liable to be correct than an inconfident one by the same subject. Henmon, however, did not distinguish between confident and inconfident subjects; but only between confident and inconfident judgments; and, therefore, his conclusion is quite compatible with the statement made above that the confident subject is not more apt to be correct than the inconfident

⁴ Ibid. p. 100.

TABLE XXIV

<i>Subject</i>	<i>T.C.</i>	<i>A.C.C.</i>	<i>A.C.E.</i>	<i>T.E.</i>
C	146	1.5	1.3	35
P	139	1.5	1.3	43
N	125	1.4	1.1	52
R	93	1.0	.8	35
S	92	1.0	.7	30
L	86	1.0	.5	35
Q	86	1.0	.6	42
A	77	1.0	.3	37
M	76	.8	.7	36
O	72	.9	.4	37
Average Con. for each Dec.	99	1.1	.8	

T.C. = Total confidence.

A.C.C. = Average confidence for correct decisions.

A.C.E. = Average confidence for errors.

T.E. = Total errors in accuracy of decision test.

SITUATION TEST

In connection with the experiment on accuracy of decision, it will be well to state the results for the "Situation Test" (or A, E, O, U Test). This test was originated by Professor Münsterberg in the interest of the ship service; and is described in his "Psychology and Industrial Efficiency."⁵ It aims to determine reliability of judgment in an emergency; and, therefore, like the experiment just reported tests degree of accuracy and rapidity in decisions. The comparison of the number of holes in two cards is, however, a relatively simple decision, while the A, E, O, U Test introduces a much more complex situation. There are more factors to be eliminated and the experience is more *baffling* than in the comparison test. The latter may probably be considered as a test for accuracy and rapidity of decision in ordinary, everyday life, while the former tests the same factors in an emergency. Of course, these two traits may or may not be correlated. It would not be unreasonable to suppose that many people who are accurate and quick in ordinary life affairs would lose their heads completely in an emergency. We shall, therefore, not be surprised if the results of these two tests are not identical.

The results are given in Table XXV. The product of the time multiplied by the number of errors in our experiment on

⁵ Pp 83-86

accuracy of decision is also given for comparison. As expected, there is not a close correspondence; but the largest products in both experiments are in the lower half of the columns and the smallest in the upper. There are four exceptions to this: L, O, A and R. None of the ten subjects have products less than 1661 in the situation test, while according to the author of the test, the perfectly reliable should make a product of 400 or less. The importance of considering not only the product but also the time and the number of mistakes in estimating any individual is well illustrated by this table. Subject O, e.g., who has the fewest mistakes, is disqualified by the great length of time; and Q, who has the shortest time, is disqualified by the number of errors. On the whole, neither the products just considered, nor the number of errors, nor the time in this test corresponds completely to the same factors in the comparison test.

TABLE XXV

Subject	Product	Situation Test II		Test I	T x E in C
		Time	Mistakes		
L	1661	151"	11	18.2"	276.5
Q	1890	105	18	21.2	168.0
M*	2040	120	17	20.0	187.2
O	2130	355	6	23.0	233.1
C	2190	146	15	15.4	192.5
A*	2300	230	10	16.4	96.2
S	2440	244	10	16.4	384.0
P*	2646	126	21	18.2	333.9
R	2814	134	21	24.0	178.5
N	3300	150	22	41.6	468.0

Product = Time x number of mistakes.

Test I = Time for sorting the cards into four groups,—A, E, O, U.

T x E in C = Time x errors in experiment on comparison of number of holes.

* = Subjects who had taken the test before.

If the number of errors alone be considered, an interesting correlation between them and suggestibility is revealed. This is shown in Table XXVI. It will be seen that with one exception the non-suggestible make the fewest errors; those who are both positively and negatively suggestible come next; and the most mistakes are made by those who are positively suggestible alone or negatively suggestible alone (N). This correlation is not unexpected, when the actual experience in performing the test is

considered. This shows that when a card is selected as having more of any given letter, there is a tendency to look for and hence find the same letter predominating in the next card. This tendency is greater for and not so easily overcome by the suggestible subjects. Subject O's exceptional position can be explained by supposing that she was able to overcome the force of this suggestion by greatly prolonging the time. The situation test thus becomes a good test for a certain kind of suggestibility; but this may be no objection to it as a test for reliability in emergency; for those who lose their heads in an emergency may always be suggestible people.

This correlation with suggestibility is shown but not so well by the errors in the comparison test (Table XXVI). We have remarked in Chapter I that this test brings out a liability to illusions, which is probably similar to the kind of suggestibility brought out by the "Situation Test." This liability to illusions tended to produce constant errors there; but in the later similar experiment this tendency was partly overcome by informing the subject of the correctness or incorrectness of each decision. Thus another factor was introduced which no doubt affected the results considerably.

TABLE XXVI

<i>Subject</i>	<i>E.S.</i>	<i>E.C.</i>	
O	6	37	Suggestible
S	10	30	} Non-suggestible
A	10	37	
L	11	35	} Suggestible and Contra-Sug.
C	15	35	
M	17	36	
Q	18	42	} Suggestible
P	21	43	
R	21	35	
N	22	52	Contra-Suggestible

E.S. = Errors in "Situation Test."

E.C. = Errors in Comparison Test.

The method of evaluating the mistakes in the situation test deserves some consideration. In some cards the predominating letter occurs twenty-one times, in others eighteen times, in others:

sixteen times and in still others fifteen times, out of a total of forty-eight. Hence, a mistake in the first card is much more serious than in the last, and this must be taken account of in evaluating the results. The method adopted was to count the mistakes in these cards as 4, 3, 2 and 1 respectively, on the assumption that their difficulty varied inversely as these numbers. Now this is the assumption that should be further investigated. It might be done by giving the test to a large number of people, and then estimating the difficulty of the respective cards by the total number of mistakes made in each. Such a record was kept of the ten subjects and the results are shown in Table XXVII. On the whole, the number of mistakes made in the 21, 18, 16, and 15 cards were 3, 30, 17, and 28 respectively; but since there are twice as many 18 and 16 cards as 21 and 15, the figures for equal numbers of cards would be 3, 15, $8\frac{1}{2}$ and 28. Thus the 18 cards are really more difficult than the 16, since there were more mistakes made in them. This seeming paradox is explained by the fact that in the 16 cards a number of consonants are intermixed with the vowels; and, since the total number of letters is the same, the number of vowels from which the predominating one is to be selected is less, and thus the selection easier. Therefore, in making the calculations for Tables XXV and XXVI, mistakes in 16 cards have been valued at 3, those in 18 at 2, instead of the opposite. This, however, is only a provisional correction, and the results for many more subjects should be considered to obtain an accurate value for each card.

Supposing such a value were obtained, it might still be rendered invalid for some cases by another circumstance: viz., individual differences as to what card is found most difficult. The presence of consonants in the 16 cards may, in some cases, have the effect of increasing instead of decreasing the difficulty. This seems to have been actually the result in the case of subject P, who made 4 mistakes in 16 cards, to 1 in 18. For some subjects again, the subjective difficulty may not increase in proportion to the objective differences; for others it may. If on further investigation such individual differences turn out to be very great, the only way of evaluating the mistakes would be for each subject on the basis

of his own errors; and then comparison of one subject with another would be difficult or impossible. Other individual differences occur in the letters which are found most difficult. Cards in which the O or U predominated were usually found easier than those in which A or E predominated; but for some subjects the opposite might be true. Such differences, however, would not make any difference in the method of evaluating the mistakes; for the numbers are equally distributed among all the letters.

TABLE XXVII

<i>Subject</i>	<i>21</i>	<i>18</i>	<i>16</i>	<i>15</i>	<i>Total</i>
O	0	1	0	4	5
L	0	3	1	2	6
A	0	4	0	2	6
S	0	2	1	3	6
M	1	3	2	1	7
C	0	3	2	3	8
P	1	1	4	3	9
Q	0	3	3	3	9
R	0	4	3	4	11
N	1	6	1	3	11
	3	30	17	28	
	3	15	8½	28	

ASSOCIATION TEST

The subjects were tested for association time as in the first investigation. In all, forty reactions were obtained for each subject, and the time taken with a stop watch as before. The results are given in Table XXVIII. The times for the aesthetic decisions, and for the accuracy of decision test are also given for comparison. The correlation between aesthetic decision time and association time is more marked than that shown between "logical" decision time and association time in the last investigation. If the method of unlike signs, which considers only the nature (+ or —) of the deviation of the individual subjects from the central tendency and not its amount, be again applied here, it will be found that there are only two cases (L and Q) of unlike deviation,—that is, 20% cases of unlike signs; and this signifies, according to Whipple,⁶ a correlation of about .8 between association time and aesthetic decision time. The correlation between

⁶"Manual of physical and mental tests," page 46

time for comparison of the number of holes in two cards and association time is, however, less marked,—there being four cases of unlike signs. The time for sorting cards, as obtained in Situation Test I, follows the association time pretty well. The only marked exception is subject R, who was not used to handling cards and felt awkward about it. Subject N's time was greatly lengthened for the same reason. This is the nearest approach we have to a reaction time test; and the indications from it are that reaction time would be directly correlated with decision time.

TABLE XXVIII

<i>Subject</i>	<i>A.</i>	<i>M.</i>	<i>A.D.T.</i>	<i>C.T.</i>	<i>T.S.C.</i>
C	.94	1.0	14.6	5.5	15.4
S	.96	1.0	13.0	12.8	16.4
R	1.04	1.0	9.1	5.1	*24.0
A	1.06	1.0	12.7	2.6	16.4
L	1.09	1.0	20.8	7.9	18.2
P	1.24	1.2	17.0	7.3	18.2
N	1.29	1.3	32.8	9.0	*41.6
Q	1.32	1.2	10.8	4.0	21.2
O	1.35	1.2	24.5	6.3	23.0
M	1.61	1.5	18.1	5.2	20.0

A. = Average association time.

M. = Median association time.

A.D.T. = Time for aesthetic decisions.

C.T. = Time for comparison of the number of holes.

T.S.C. = Time for sorting cards, as shown in Situation Test I.

*. = Not used to handling cards—felt awkward about it.

In the last investigation, a correlation between quickness of association time and constancy in decision was brought out (see page 15). This provisional conclusion is not verified by the results of this experiment; but it must be remembered that the decisions were upon different material, and the results for constancy not so satisfactory and definite as in the previous case. Then there were many more experiments to obtain a constancy index and the method employed—arranging the whole five cards in order of preferment—is a much subtler means of bringing out the relative constancy of the subjects; but, of course, not so well adapted to the work on suggestibility as the one used in the second instance. The result here cannot, therefore, be considered as a refutation of the previous.

If now we compare the association times with the errors in the

accuracy of decision test (Table XXIII), we shall find that the quick subjects are, on the whole, the most accurate. There are only two cases of unlike deviation. It would seem then that, though quickness of association time does not surely guarantee quickness in the comparison of the number of holes in the cards, it does point more certainly to accuracy in this test. It may very well be that inaccuracy in the comparison test and slow association time depend upon a common factor; and this factor may be inhibition, delayed or erroneous apperception, or some other of the many factors that lengthen association time. We know from our association experiment that the slow subjects had many associations as quick as those of the quickest subjects, but reported many more obstructions, inhibitions, etc., so that these account, in part, at any rate, for the longer times. Such inhibitions and other irregularities occurred most frequently with subjects O and Q; to a considerable extent with M, N, P, and L; and almost not at all with the other subjects.

MEMORY TESTS

The method of testing the memory of the subjects was slightly different from that used in the previous investigation. The material was again figures, nonsense syllables, and words. Memory for figures was tested as before, except that the subject repeated each number from memory immediately instead of after a five-second interval. In testing memory for nonsense syllables and words, the Lipmann-Marx memory apparatus, which exposes the words successively at a very regular rate, was made use of. Lists of twelve nonsense syllables, and of eighteen unassociated words were constructed; and immediate memory efficiency or learning ability was measured by the number of repetitions required to memorize these lists. The memory was taken as complete when the subject could anticipate one word or syllable ahead throughout the list. In the case of the words the subjects were told to use artificial associations to help them in memorizing; but to prevent these as far as possible with the nonsense syllables. The object of this was to obtain a measure of association strength in the former case, as well as receptivity in the latter. Having

learned a list of nonsense syllables and a list of words, the subject was occupied for 25 minutes in the accuracy of decision test described above; and then asked to write down first all the nonsense syllables and then all the words he still remembered. His memory for a period of 25 minutes was thus measured by the number of words and syllables he still remembered. A test of memory for figures, for words, and for nonsense syllables was given each subject every week for three weeks; and the average efficiency for the three tests taken as the measure of the subject's memory ability for each kind of material.

We shall first consider the memory span as measured by the number of digits reproduced immediately after two repetitions. Every subject showed improvement with practice in this test; and some were able to reproduce one figure more at each test than at the last; but only the averages for the three tests are given in Table XXIX. These results partly verify our conclusion regarding memory span in the last investigation. As before, there is no correlation between memory span and decision time for either kind of decision; but there seems to be a correlation, not so marked as before, between memory span and constancy,—for the most inconstant have poor memory spans. Moreover, there is here shown a considerable correlation between accuracy of decision and memory span. There is probably only one exception (subject R), and this would mean a correlation coefficient of .9 or over. We have observed above that there is probably a correlation between suggestibility and inaccuracy. If this is so, then suggestibility, inaccuracy, and poor memory span ought to go together. The fact that the four most positively suggestible subjects have also the poorest memory spans affords some confirmation of this supposition. Since memory span for figures is closely related to span of attention, and a broad span of attention means the ability to take in all the details of a situation, which is essential to correct evaluation and decision, we would naturally expect this correlation between memory span and accuracy of decision. The accuracy spoken of here, as elsewhere in the discussion of memory, is for the comparison of the number of holes in pairs of cards and for the situation test. The results

for the latter show less evidence of correlation with memory in every instance.

TABLE XXIX

<i>Subject</i>	<i>M.S.</i>	<i>C.</i>	<i>A.</i>
L	11 $\frac{1}{3}$	35	35
C	11	21	35
S	10	39	30
M	9 $\frac{2}{3}$	30	36
N	9 $\frac{1}{3}$	37	52
A	9 $\frac{1}{3}$	21	37
Q	9	43	42
P	9	32	43
R	9	43	35
O	8	29	37

M.S. = Memory span for figures.

C. = Constancy, number of changes in 50 aesthetic decisions.

A. = Accuracy, number of errors in 100 decisions.

The immediate memory or learning ability of the subjects, as measured by the number of repetitions required to memorize the lists, is shown in Table XXX. The average number of repetitions for three lists of twelve nonsense syllables each, and for three lists of eighteen words each is given for each subject; and then these figures are added to get an index of learning ability. The number of repetitions for words and those for nonsense syllables correspond, on the whole, pretty well. The largest figures for each are in the lower halves of the columns; but there are some important variations. Some of the subjects were accustomed to work with nonsense syllables, and, therefore, had some slight advantage; these are marked with an asterisk. No correlation is brought out between learning ability and decision time, constancy or accuracy, except in the case of repetition for words taken alone. Here a slight correlation between quick learning and accuracy is shown, but subject N is a marked exception—very inaccurate, yet learning very quickly. The correlation mentioned is important, for in this test the subjects were told to make use of artificial association to aid them in learning. It means, therefore, that rapidity in the formation of associations is correlated with accuracy, while mere receptivity, as tested by nonsense syllables, is not; and this is a similar correlation to that brought out in the

association test where it was found that quickness of association time was correlated with accuracy of decision. There is, therefore, no doubt that the speed and certainty of association is an important factor in assuring accuracy in decision. Learning ability for nonsense syllables is not at all correlated with memory span for figures; but, as expected from their mutual correlation with accuracy, the learning of words is. There are only two cases of unlike deviation (signs): subjects C and R, the former good in memory span and poor in learning,—the latter the opposite.

TABLE XXX

<i>Subject</i>	<i>L.</i>	<i>W.</i>	<i>N.</i>	<i>E.A.</i>
R	13½	5½	7½	35
*N	16	7	9	52
*O	16½	10	6½	37
M	17½	6½	10½	36
*L	18½	7½	11	35
P	19½	9	10½	43
S	24½	8½	16½	30
*A	26½	11½	15½	37
C	28½	13½	14½	35
Q	32	16	16	43

W. = Number of repetitions for lists of eighteen words.

N. = Number of repetitions for lists of twelve nonsense syllables.

L. = Learning ability, obtained by adding the two above.

E.A. = Errors in accuracy of decision test for comparison with column W.

* = Subjects who had previous experience with nonsense syllables.

The memory of the subjects was also measured by the number of words and syllables remembered after a period of twenty-five minutes. The results (given in Table XXXI) flatly contradict the correlation noticed in the last investigation between memory over a period of one week and rapidity of decision. There it was observed that the subjects quick in decision had better memories than the slow. Here, on the contrary, the slow subjects have, with only one exception, better memories than the quick. Of course, both results may be true since one experiment was on memory for a week and the other on memory for twenty-five minutes; and a subject who remembers well for twenty-five minutes may not remember relatively as well for a week.

As before no definite correlation is brought out between

memory and constancy or accuracy. The two subjects, however, who have the best memory for words (L and S) are also accurate subjects. We would expect from previous considerations that strength of association (memory for words) would show some correlation with accuracy, and that retentiveness, as tested by memory for nonsense syllables, would not. We would expect also that memory for words would correspond pretty well with memory span for figures; and this is true, with the exception of subject C, who is very good in the latter and very poor in the former.

In tests for memory it is very difficult to keep the conditions the same for all subjects, especially where, as in this case, the subjective interests of the subjects are not the same. Each subject does best in the test in which he is most interested or likes best. This is true of any test; but it is probably not so serious an objection as might be supposed for the interest may follow the ability instead of the reverse as is usually supposed. Objective conditions were always kept as near as possible the same. The subjects were asked not to think of the material learned during the 25-minute interval, in which they were occupied, as stated above, in the accuracy of decision test. In the first test they did not know they were going to be asked to write what they could remember after 25 minutes; and, as the results show that they did as well in it as afterwards, we conclude that they succeeded in following the instructions in the second and third tests.

TABLE XXXI

<i>Subject</i>	<i>W.</i>	<i>N.</i>
L	18	10 $\frac{2}{3}$
S	18	9 $\frac{2}{3}$
N	17	9 $\frac{1}{3}$
M	17	9 $\frac{1}{3}$
P	15 $\frac{2}{3}$	9 $\frac{1}{3}$
A	15 $\frac{2}{3}$	8 $\frac{1}{3}$
R	15 $\frac{1}{3}$	8
Q	15 $\frac{1}{3}$	7
O	14 $\frac{2}{3}$	6
C	9	3

W. = Number remembered out of eighteen words after 25'.

N. = Number remembered out of twelve nonsense syllables after 25'.

ATTENTION

The relation of attention to decision time, constancy and accuracy is probably of more importance than any of the relations so far considered; but there are as yet no adequate tests for attention and the work on this point is therefore even more meager than on the others. *Span* of attention is probably fairly well tested by the work on memory span for figures. Two other tests were now made: the cancellation test (A-Test) for *duration* of attention, and the dot-counting test for concentration of attention.⁷ Both of these tests involve other factors as well as attention; but there can probably never be a test for attention alone; for attention means attention to something and therefore involves at least sensory and perceptual factors.

The A-Test is chiefly a test of visual perception and recognition. It, of course, requires attention, but the results are ambiguous. A poor showing may be due to defects of perception or slow reaction rather than poor attention. Each subject was given only one test. A page containing thirty lines of pied text was used. There were 51 A's to be cancelled. Efficiency was measured by the time taken to perform the task and the number of errors. Whipple gives the formula $A = \frac{C - W}{C + O}$ to compute

the index of accuracy.⁸ C = the number of letters crossed; W = the number wrongly crossed; and O = the number erroneously omitted. Since none of the ten subjects had any of the W class, the number of letters crossed out of 51 can always be taken as the index of accuracy. Also, to compute a single index of efficiency, Whipple gives the formula $E = \frac{S}{A}$, in which E = the

desired efficiency, S = the time, and A = the accuracy index; but, since the accuracy varied so little and the time so much, the efficiency as computed by this formula corresponds exactly to the time, which we have therefore used in our statement of the results in Table XXXII.

⁷ For description of these tests see Whipple's Manual, pp. 254-273.

⁸ Ibid. pp. 260-261.

TABLE XXXII

<i>Subject</i>	<i>Time</i>	<i>A.</i>	<i>S.T.</i>
L	110"	47	1661
P	131	50	2646
M	136	51	2040
A	160	51	2300
Q	162	50	1890
S	172	51	2440
O	177	51	2130
C	191	49	2190
N	227	50	3300
R	252	47	2814

A. = Number cancelled out of fifty-one.

S. T. = Products in the situation test.

These results show no relation to time, constancy or accuracy in the decision experiments, but they correspond fairly well with the products in the situation test. This is probably because in both tests the perception and recognition of letters are important factors. There is probably also, as in the situation test, some relation to suggestibility; for the mistakes are all made by suggestible or contra-suggestible subjects.

The dot-counting test also involves other factors than attention; and these are, according to the introspection of the subjects, chiefly visual schematism or ingenuity in grouping the dots and immediate memory for these groups. Each subject was given two tests; in the first there were 50 dots, in the second 57. The errors were counted by taking the difference between the given and the true numbers. The total errors and the average time for the two tests are given for each subject in Table XXXIII. The O or U signifies that the mistakes were made in over- or under-estimating the true number. There seems to be no correspondence between time for dot-counting and time for the cancellation test. In fact, the quickest in the latter was the slowest in the former; but there is not a general inverse relation. There is also no relation brought out between the results for the dot-counting test and time, constancy, or accuracy in the decision test; nor is there any relation, as in cancellation, to the situation test. On the whole, however, the least suggestible subjects are the quickest in dot-counting, though not always the most accurate.

These tests may probably have been successful in showing the

relation of the factors tested to decision type; but it seems to us that attention was not the most important of these factors, and that the results cannot be considered as any indication of the relation of attention to decision. In all probability attention is a much more significant and crucial factor at least in accuracy than these results would show; and its real place is more likely better revealed by the tests for memory span.

TABLE XXXIII

<i>Subject</i>	<i>Time</i>	<i>E.</i>
S	16.5'	1—O
M	20.0	3—U
A	21.0	5—U
N	22.0	2—U
O	25.5	1—U
R	36.5	7—U
Q	37.0	1—U
P	53.5	1—O
C	63.5	1—U
L	78.0	2—O

E. = Errors for two tests.

O. = Over-estimations.

U. = Under-estimations.

ORIGINALITY TEST

The last test we have to report had for its object to bring out the originality or inventiveness of the subjects. The following method, suggested by a paper of Professor Royce's on the "Psychology of Invention,"⁹ was adopted: First a circle was presented to the subject, who was told to draw another geometrical figure as different as possible from this; then a rhombus with diagonal was presented and the subject asked to draw *anything else*, as different as possible; and finally five concepts were given one at a time and the subject was to write other concepts as different as possible from these. The degree of originality is shown by the ability to break away from the impressions or ideas, and by the time of the performance. The tendency in the first test would be to draw a square, in the second to draw some other geometrical figure, and in the third to write the opposite of the given concepts, which were: mind, cause, substance, man, and heaven. If this tendency were followed out it would indicate a

⁹ *Psych. Rev.* 1898, p. 100.

routine mind, incapable of breaking away from the dominance of the ideas and impressions borne in upon it; while the further it were departed from, the greater the degree of originality. Since the ordinary associations of these perceptions and concepts may not be exactly the same for every subject, as the ones mentioned above, it was necessary to take the introspection of the subjects into consideration in estimating the amount of departure from the ordinary. Of course, this estimation was no doubt also influenced by the subjective prejudices of the experimenter; but Table XXXIV gives the relative standing of the subjects as near as it could be estimated from the records. The subjects are arranged in order of originality with the most original first.

TABLE XXXIV

<i>Subject</i>	<i>O.</i>	<i>Time</i>
L	16	228"
S	15	43
A	11	107
N	10	84
M	10	61
C	10	64
O	10	60
P	8	56
Q	4	87
R	2	48

O. = Index of originality.
Time = Total for all the tests.

The originality index was calculated in the following way: a complete departure from the above-mentioned ordinary tendency, shown by both the introspection and the objective records, was in Tests 1 and 2 valued at 5; a complete submission to it at 0; and various degrees of success at overcoming it were represented by values from 0 to 5. In Test 3, thorough success in getting away from the usual ideas was valued at 10, 2 for each concept. The figures given in column "O" of the table are, therefore, the values attained out of a possible 20. In general, those who were original with the impressions in Tests 1 and 2 were also original with the ideas in Test 3; and we, therefore, give only the total for all tests in the table. The same can be said of the time, and for the same reason only the total time for the three tests is given.

As we anticipated, the results in this test are inversely related to the results previously obtained for suggestibility. The most original are the least suggestible; next in order come those who are also contra-suggestible; and least of all the positively suggestible. There is only one exception to this: Subject L, who has shown the greatest originality, is both positively and negatively suggestible in the decision test; but his originality was only attained by the sacrifice of a great deal of time; while S, the least suggestible subject, is both quick and original in this test. Of course, we might follow our method in the decision test, and say that there are both quick and slow original, and quick and slow non-original types; for it is hardly fair to say that difficulty in overcoming the tendency, as shown by a lengthening of the time, if rewarded by final success, indicates a lack of originality. It is surely one type of originality—the originality of the thoughtful, persevering man as distinguished from that of the genius. This difficulty, however, shows some suggestibility, and if the test is taken merely as a test for suggestibility, the lengthening of the time must certainly be taken into consideration as a mark of that trait, for it appears that a suggestible person may be original but cannot be quick in his originality. If the time as well as the work done is thus evaluated, this test for originality becomes truly a very good test for suggestibility. The suggestible subject will succumb to the suggestion of the ordinary association with the idea or impression given, or will take a *very long time* to overcome it; while the non-suggestible will quickly break away from it.

Other correlations with originality are just the inverse of those previously noticed with suggestibility. Hence, the most original subjects are the most accurate in the comparison test and have the fewest mistakes in the situation test. This correlation is strengthened by subject L's position at the head of the list in the originality test.

All the tests given were very successful in bringing out large individual differences; but too few subjects were examined to obtain any adequate information regarding the correlation of the various traits. Several correlations have been hinted at; but the

results in this respect were largely negative. It may be that on further investigation with a larger number of subjects correlations may be obtained where we have found none; and those we have suggested may be proved erroneous; but until refuted by more efficient investigations, we shall assume that these results point in the right direction. They seem to verify two important statements made by Thorndike¹⁰: (1) that the variations in mental traits are continuous and cluster around one central tendency or type; and (2) that in mental traits correlation and not compensation is the rule,—that is to say, efficiency in one trait is very likely though not certain to be correlated with efficiency and not deficiency in another.

¹⁰ Educational Psychology, 1910, chapter X.

CONCLUSIONS

The very nature of the investigation renders this paper extremely difficult to summarize; but we shall endeavor to select from the results scattered throughout a few conclusions that may be regarded as of chief importance.

(1) There is no correlation between time and constancy in either logical or aesthetic decisions. Hence, we are able to speak of four decision types: quick constant, slow constant, quick inconstant, and slow inconstant. These types, however, grade into each other, so that there is no natural dividing lines between quick and slow, and between constant and inconstant.

(2) There is no correlation between time and *accuracy* in objective decisions, so that here also any time can be combined with any degree of accuracy. This fact has some practical significance; for it shows that subjects both quick and accurate exist. They are, however, probably rarer than the mediocre in time and accuracy, and their rarity no doubt increases with their efficiency.

(3) There is no correlation between time in subjective and time in objective decision, nor between constancy in the former and accuracy in the latter. That is to say, the consistent subject may be consistently inaccurate; and the subject slow in matters of taste or opinion may be quick in objective situations. There seems, however, to be some evidence of a correlation between quickness in subjective decisions and accuracy in objective,—a correlation probably based upon the concentration of attention required in both traits; but it is not sufficiently marked to warrant serious discussion.

(4) Some subjects who come under the quick constant class in simple decisions, change their type under more complex conditions, becoming slow or inconstant or both. If this change of type also occurs for time and accuracy in objective decisions (and the results for the comparison and situation tests seem to show that it does), the fact is of considerable importance; for the individual thus changing would be successful in situations requiring simple

acts of decision, but would fail in any calling that demanded decisions in relatively complex situations.

(5) Subjects differ greatly in the variability of their decision times; so that there are "uniform" and "variable" subjects; but the variability in the decision times of any given subject has no relation to his variability from the mean average time for all the subjects. Hence, subjects can be found who are both much quicker than the average and also "uniform." The subjects "variable" in time are as a rule the same ones that change their type under more complex conditions. The coefficient of variability is greater in objective than in aesthetic decisions, and there is no correlation between the C.V.'s in the two tests. The C.V. is an independent factor, not correlated with time, constancy, or accuracy in decision.

(6) In general, practice has no marked effect upon decision times; but there are individual differences in this respect—some of the more adaptable subjects quicken the time.

(7) The decision time for the second presentation is, as a rule, less than for the first; but for some subjects it is relatively very much less than for others. These subjects probably learn very quickly from experience, and their efficiency increases rapidly with repetition; but their efficiency in novel situations as shown by the time for the first presentation is not altered. This is important practically; for such subjects would be successful in an occupation presenting very few situations with routine decisions in each, but might fail in any vocation which demands quick decisions upon always novel situations.

(8) In subjective decisions the confident subject is more apt to be constant than the inconfident; but in objective decisions the confident subject is not more liable to be correct. In the latter the medium subject has the greatest accuracy, while the over-confident and under-confident are both inaccurate. The over-confident, however, varies little in his confidence from right to wrong judgments, while the under-confident are much more confident for their correct than for their incorrect decisions. In both subjective and objective decisions the confident judgment of any given subject is more apt to be constant and accurate respectively than

his inconfident judgment; but the difference is less than might be expected.

(9) In subjective decisions there was more inconstancy with judgments which the subjects described as difficult. The same thing is shown by the fact that there were more changes in decisions where times were above the median, and hence more difficult, than in those below it.

(10) The subjects differ relatively in constancy with the different materials used in the aesthetic decisions. On the whole, there are most changes with the post cards and least with the pictures. They also differ in respect to the material which they find most difficult to decide upon. This is shown by their relative differences in *time* with the different materials.

(11) The accuracy of the objective decisions remained on the whole about the same when the time was allotted. Of the slow subjects, some were more inaccurate when hurried, while others actually did better; and the longer time did not increase the accuracy of the quick subjects. These facts may favor the theory of an "optimal judgment time"; or it may be that the slow who were more accurate when hurried belong to that class of individuals who, while quite phlegmatic in ordinary life, excel when called upon to fill an emergency.

(12) The general effects of suggestion upon decisions are to lengthen the time and increase the inconstancy; but these effects are much more pronounced with some subjects than with others. The suggestion also reduces the variability of the subjects from their central tendency in time and increases it in the number of changes.

(13) Subjects can be roughly classified into four types on the basis of their suggestibility: (1) the non-suggestible, (2) the positively suggestible, (3) the positively and negatively suggestible, and (4) the negatively suggestible. These types merely signify certain degrees of the trait; for there is probably no absolutely non-suggestible subject, just as there is no purely positively or purely negatively suggestible person.

(14) There is a correlation between suggestibility and accuracy. The less the suggestibility, the greater the probability of

accuracy. This is shown by the number of errors in the comparison test, as well as by the number of mistakes in the "situation test."

(15) The results for the "comparison test" and the "situation test" are by no means identical. The latter presents a more complex situation; and subjects who are equally excellent in simple decisions may be quite different in more complex ones.

(16) The situation test seems to be successful in bringing out the mental trait intended; but the method of evaluating the results and comparing the subjects requires further study and correction.

(17) The best material for testing suggestibility appears to be geometrical figures; for here the suggestion was most readily accepted. The post cards were the poorest material for bringing out suggestibility, but the best for contra-suggestibility. There were, however, marked individual differences as to the material which brought out the most or least of these traits.

(18) The suggestible subject *may* or may not be introspectively aware of his suggestibility. He may think his decision is unbiased or even remember that he decided the same way before, while his memory plays him false and he is really accepting the suggestion.

(19) Inertia, or the tendency to leave things as they are, exerts some influence on decision. This inertia is much greater with some subjects than with others. There is also some indication of its correlation with *variability* in time.

(20) Position also influences decision. This influence is one kind of suggestion, and of course varies in amount with different subjects. There is also considerable variation in the position that is found most attractive. Position B,—that is, one from the right, is the most favored.

(21) Association time shows some slight correlation with time in aesthetic decision; and also with *accuracy* in objective decision; in that the quickest in association time are the more accurate.

(22) In conformity with the above, we find that there is a correlation between rapidity of learning lists of words and accuracy; but not between the learning of nonsense syllables and accuracy. In the former, association is the chief factor; while in

the latter, receptivity or plasticity is a more important factor than association.

(23) Memory span for figures is also well correlated with accuracy and very slightly with constancy. It is also correlated with suggestibility,—the most suggestible having the poorest memory spans.

(24) Memory over periods of time, and time for sorting cards show no definite correlation to decision time, constancy, or accuracy.

(25) The results of the A-test are correlated fairly well with the products in the situation test; but neither the A-test nor the dot-counting test brought out any correlation with decision time, constancy, or accuracy. The least suggestible, however, make the fewest mistakes in the A-test; and are also the quickest, though not the most accurate, in the dot-counting test.

(26) The most original subjects are the least suggestible; but a suggestible person may be original, if he has the perseverance and will to overcome the suggestion; and this probably requires some contra-suggestibility.

(27) The most original subjects are the most accurate in the comparison test, and have the fewest errors in the situation test.

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CHAPTER I

INTRODUCTION

It is perhaps more true of music than of any other art that the critics of one generation are the laughing stock of the next. The question of acceptable harmonic progressions has been a bone of contention between composer and listener from everlasting to everlasting. It matters not with what period of musical history we concern ourselves, there is always evidence that the great composers were being continually taken to task on the ground of impossible harmony. Even Mozart was attacked by Hans Georg Nägeli¹ for certain progressions in his "Jupiter" symphony; Beethoven became notorious for outraging the tender feelings of theorists of his day; fifty years later Wagner was struggling to persuade the world to endure harmonies which were to become shortly afterwards a chief resource for popular nights at the symphony. In our own time Strauss set out by appalling his hearers with a "wild experimentation" which a London critic described as "discordant for the mere sake of discord";² one by one he has converted his opponents until, as Newman says,³ "Any one who now thinks *Tod und Verklärung* a tough nut to crack is looked upon as a hopeless conservative." But the end is not yet. More recently Sibelius has given concert-goers a start with his Symphony Number 4, of which W. J. Henderson writes: "He has swallowed the disjointed sequences, the chord of the minor second, and all the Chinese horrors of the forbidden fifths." Another critic remarks, "It is as inconsequential as the ravings of a drunken man." Doubtless the critics of Sibelius will be making their apologies ten years hence.

What is the ultimate meaning of all this? For the answer to this question, practical music will have to turn to theory, and

¹ Jahn, *Life of Mozart*, III, 34.

² Newman, *Musical Studies*, 290.

³ *Op. cit.* 291.

especially to the theory of consonance. How far an answer may be expected from this quarter we shall see at the conclusion of the present discussion. But, first, we shall be obliged to review and, to a certain extent, to revise the theoretical situation.

CHAPTER II

THEORIES OF CONSONANCE

The problem of consonance is one that may be attacked from the various standpoints of mathematics, of psychology, and of physiology. The last-mentioned branch of consonance theory may, however, be eliminated from consideration here, since the little that it has at present to offer is of very questionable validity. Of the other two, it was the mathematical aspect that most concerned theorists from the time of Pythagoras to that of Helmholtz, whereas during the last fifty years attention has been given increasingly to the psychological aspect.

The mathematical standpoint is best represented by the philosophers Descartes, Leibnitz, and more especially Euler,⁴ all of whom were impressed in true Pythagorean fashion by the general significance of number relations. The fact of greatest moment to them was the correspondence between the physical datum of greater or less simplicity in vibration ratios and the conscious experience of greater or less consonance. Thus for the 1:2 ratio we have the very consonant octave; for the 2:3 ratio the less consonant fifth; for the 4:5 the imperfect consonance of the major third; and so on to the harshest dissonances. According to Euler the human mind perceives the commensurability of pitch numbers as such, and derives pleasure from discovering the law of their arrangement.

These correspondences between the physical and the conscious are unquestionably facts of fundamental importance, but they leave the conscious phenomena themselves no better understood than before. Further progress in the minute observation of the mental facts was inhibited in these investigators by their strong bias for the delights of "arithmetical mysticism," as Helmholtz expressed it.⁵ Such a psychology as they worked out was merely mathematics with a new name. The changed

⁴ Helmholtz, *Sensations of Tone*, Ellis' translation, 229 ff.

⁵ *Sensations of Tone*, 229.

standpoint of the present day is voiced by Professor Max Meyer when he says,⁶ "Neither the physicist nor the physiologist can prove by physical or physiological laws why we must enjoy certain combinations of tones. It is the psychologist's task to determine the aesthetic laws which describe the subjective as well as the objective conditions of aesthetic enjoyment."

But when we have claimed that consonance is essentially a problem for psychology, we have not yet limited its method of treatment. As psychologists we must, of course, regard our given conscious phenomena as so many facts to be described in detail and related to each other wherever possible. But this may be done with only a minimum of reference to the past or the future. We may simply characterize present facts in terms of other present facts. On the other hand, our prevailing interest may cause us to regard our subject-matter as merely a phase in a course of development. We may seek to clarify existing conscious phenomena by reference to their probable mode of genesis. The importance of the latter method as complementary to the first is fully recognized in general psychology, but the literature on the subject of consonance betrays as yet a rather marked bias for the first. Any given experience of consonance is usually taken for granted, as given once for all, and the nature of its ancestry is too little considered as a possible source of new light. This one-sided viewpoint will become apparent as we examine briefly the four leading psychological theories of consonance, those of Helmholtz, of Lipps, of Krueger, and of Stumpf.

HELMHOLTZ

The present-day psychology of consonance and dissonance begins practically with Helmholtz. The views set forth in his "*Tonempfindungen*," while by no means new at every point, represent the first thorough-going effort to embrace all the facts under a consistent theory. So tenaciously has this theory held on that in spite of repeated attacks from a very considerable num-

⁶M. Meyer, *Contributions to a Psychological Theory of Music*. Missouri University Studies, No. 1.

ber of critics, such as Oettingen, Mach, Riemann, Engel, Melde, Lipps and Stumpf, it is even to-day the popularly understood explanation of consonance and dissonance. But Stumpf gave it, for the large majority of theorists at least, the final *coup de grâce* in the first chapter of his "Konsonanz und Dissonanz."⁷ Suffice it here to give a brief statement of Helmholtz's position, and a summary of the objections which have seemed to make necessary its abandonment.

The psychological principles invoked by Helmholtz,⁸ in explaining the difference between consonance and dissonance, were two: similarity and sensory roughness. Consonances are characterized positively by the similarity of two tones due to their possession of identical overtones, and negatively by the absence of perceptible beats. Dissonances are characterized positively by their rough sensory quality due to the beating overtones of the two clangs, and negatively by a dissimilarity due to the lack of identical overtones. This theory has the great advantage of simplicity in that it finds its criteria in fairly obvious characteristics of the sensations themselves, and not in some more obscure processes supposed to be aroused by the sensations. But its critics have raised well-nigh unanswerable objections at every point.⁹ To the statement that dissonance is due to beating overtones, it is answered:

1. That dissonance can be produced without beats, as when two simple tones of 700 and 1000 vibrations are sounded together. Here there are no overtones, and the fundamentals give 300 beats, which is above the Helmholtzian limit of perceptibility, i.e., 132. By putting together a number of such intervals, it is possible to form chords as free of beats as the most consonant chord in music, and yet more dissonant than the most extreme discord.¹⁰

2. That dissonance continues to be perceived as such when two tuning forks are placed, one at one ear another at the other,

⁷ Akustik und Musik, I, 1-19.

⁸ Sensations of Tone, 182-228.

⁹ Stumpf, Musik und Akustik, I, 1-10.

¹⁰ Stumpf, op. cit., 7, 8.

and both sounded too weakly for beats to be produced. The same is true of the cases of double hearing, where one ear hears a tone slightly higher than the other. Beats are out of the question here, but the dissonance is sometimes most painful.

3. That the greater awareness of beats in the lower region of the scale should mean a change in consonance corresponding to pitch changes. Such is not the case.

4. That the consonance of an interval might be expected to vary according to the timbre of the instrument, since different timbres mean different overtone relations. But this again is demonstrably not the case.

5. That in experiments on mistuning intervals made by pure tones, it has been found that a variation of four vibrations could be detected. This could not conceivably be true if judgments of purity were made possible only by characteristic overtone effects.

To the proposition that consonance is determined by the similarity between the two clangs, due to their possession of identical overtones, the answer is made that this cannot apply to simultaneous clangs, because the only possible effect of two identical overtones in such a case is that of mutual reinforcement.^{11, 12} When the same overtone occurs in two clangs, sounded simultaneously, what will appear to the ear is simply one relatively loud overtone in the total sound complex. The Helmholtz theory, accordingly, makes it necessary to limit the explanation of consonance to successive notes; in short, it involves a transfer of the meaning of consonance, making it identical with melodic relationship. Meyer, who has given more attention to melody than any other experimenter, makes a sharp distinction between melodic relationship, which has to do only with successive tones, and consonance, which has to do only with simultaneous tones.¹³ He shows in detail, for example, how Wagner's interest in a variety of melodic relationships was often at variance with the usual requirements of consonance. Melody is clearly seen to be

¹¹ Stumpf, *op. cit.*, 15-19.

¹² Krueger, *Differenztöne und Konsonanz*, *Archiv für die Ges. Psych.* I, 214-216.

¹³ *Contributions to a Psychological Theory of Music*.

one kind of ultimate relation, consonance another, and the one is not to be explained by the other.

Thus neither the identity of overtones nor the roughness of beats between overtones prove to be adequate criteria of consonance, and with their failure the Helmholtz theory falls to the ground, leaving the problem to be solved by his critics.

LIPPS

Lipps, whom we may mention next after Helmholtz, sets out from a single principle.¹⁴ Consonance, according to his theory, rests on the rhythmic accord of unconscious excitations. Stated more in detail, his explanation is as follows. Each single physical vibration causes a separate unconscious process which Lipps describes as micro-psychic. This micro-psychic process is the imaginary middle term between the physical stimulus and the conscious effect. There is a one to one correspondence between the physical stimuli and the micro-psychic processes, but any number of the latter may fuse into an uniform conscious experience. Given two tones of 200 and 100 vibrations respectively, we obtain two series of unconscious experiences, one of 200 separate excitations, the other of 100. Now since 2:1 is a simple rhythmic form, we have a micro-rhythmic experience of a simple and therefore pleasant sort, fusing into the conscious effect of the octave consonance. If micro-psychic processes are excited in such a way that every third member of one series corresponds with every second of the other, we get a less simple micro-rhythm and a less consonant effect in consciousness,—the fifth. Beyond a certain point of complexity the rhythm appears in consciousness as the characteristically unpleasant effect of dissonance.

It is obvious at once that the "psychic" effects with which Lipps deals are not given in any state of awareness, but must be deduced from the conscious effects they are intended to explain. This is an instance of the "explanation" of the conscious by the subconscious, an explanation which, as Professor Münsterberg has shown, amounts to nothing more than an ac-

¹⁴ *Grundtatsachen des Seelenlebens*, Ch. XI.

ceptance of the problem itself as its own solution.^{15, 16} Such a method of procedure is so far removed from laboratory experiment that it seems to mark a retrogression from Helmholtz back to the speculative vagueness of his predecessors, and for this reason it has met with rather caustic treatment at the hands of those for whom psychology ceases to be scientific when it ceases to be empirical. As psychologists we are permitted to describe only the facts; hence, when Lipps goes beyond the realm of psychic experience to one of unconscious, unverifiable facts, his position is interpreted as a capitulation on the part of psychology. Meyer, for example, speaks of the Lippsian law of tone relationship as "linguistically derived" from a "pseudo-experience of micro-rhythm."¹⁷

More specific are the criticisms of Stumpf and Krueger, who meet Lipps on his own ground with a view to proving his deductions at fault. As against the Lippsian assumption that the tone sensation *c'*, corresponding to 256 vibrations, is due to the fusion of 256 separate unconscious waves, Krueger points out¹⁸ that the stopping of a number of holes in a siren does not alter its characteristic quality of tone; in other words, that irregular interruptions of the physical succession of vibrations are without effect on the singleness of quality of the tone as consciously experienced. And even granting the reality of unconscious rhythm, it may be urged that the 5:6 ratio of the minor third and the 5:8 of the minor sixth make very good consonance, but very bad rhythm as we ordinarily think of the latter. More difficult still is the situation when we pass from intervals to chords.¹⁹ The 4:5:6 ratio of the major triad, considered as rhythm, would be so confused that it might as well be 7:8:9. And there is the further objection, stressed by Stumpf²⁰ that

¹⁵ H. Münsterberg, *Psychotherapy*, Ch. VI.

¹⁶ H. Münsterberg, in *Subconscious Phenomena*, I.

¹⁷ M. Meyer, *Unscientific Methods in Musical Esthetics*, *Journal of Philos., Psych. and Scientific Methods*, 1904, I, 707-715.

¹⁸ Kreuger, *Differenzttöne und Konsonanz I*, 218, *Archiv für die Ges. Psych.* I.

¹⁹ Krueger, *op. cit.*, 220.

²⁰ Stumpf, *Konsonanz und Dissonanz*, 28 ff.

no matter how simple the tonal ratio, yet, owing to differences of phase, we hardly ever have physical coincidence of vibrations. These differences of phase are without effect on the consonance of the interval, but they would completely alter the character of the rhythm.

In order to meet such objections Lipps, after having assumed a whole realm of unconscious micro-rhythms, is obliged to assume further that there are possibilities in it which do not exist in the realm of conscious rhythm.²¹ By the time micro-rhythm has been specially adjusted to meet all these requirements it remains with little more psychological significance than Euler's "unconscious recognition of number relations" proposed nearly two centuries ago.

KRUEGER

The theory of Krueger, which we may next consider, although in point of time it actually followed that of Stumpf, is a return to the more scientific method of Helmholtz, in that he attempts to refer consonance and dissonance to simple psychic facts verifiable by experiment.²² But whereas Helmholtz emphasized largely the effects of overtones, Krueger attaches the greatest importance to difference tones.²³ He sets out with the general statement, deduced from his experiments, that the five difference tones of two simultaneous tones are to be calculated by always subtracting the two remaining smallest vibration numbers.²⁴ Thus for the ratio 1 : 2 the only possible difference tone would be 1, which would coincide with the lower generator; for the ratio 4 : 5 the difference tones would be 1, 3, 2, 1, 1, of which three fall together. An examination of each particular case of consonance and dissonance shows that not only do all the dissonant intervals contain difference tones which beat, but each one contains, as a result of the closeness of its difference tones, at least one mistuned unison,—that is, two tones lying so near

²¹ Tonverwandtschaft und Tonverschmelzung, *Zeitsch. für Psych.* XIX, 1-40.

²² Differenztöne und Konsonanz, *Archiv für die Ges. Psych.* I, 254 ff

²³ Krueger, *op. cit.* 255 ff and II, 1-20

²⁴ *Op. cit.* I, 270

together that they cannot be distinguished as two, but give an intermediate tone of a peculiar blurred character.²⁵ Consonances, on the other hand, are without exception free from difference tone beats, and contain only pure unisons, since the difference tones either fall exactly together or at least the distance of a third apart. The psychic effect of this physical difference is the clarity of the consonances as contrasted with the blurred and heterogeneous quality of the dissonances. A further significant physical difference is that the number of separate difference tones in the consonance is comparatively small, because of the five at least two fall together. On this account the consonances approximate the effect of unison more nearly than the dissonances.²⁶ Since the unison is the earliest and most familiar tonal experience of the ear, an approximation to this appears as the psychic effect of familiarity, an effect which is enhanced by the fact that the consonance itself is more frequently heard than the dissonance, and is therefore more familiar on its own account.²⁷ Taken together, all the characteristic relations of difference tones to consonant and dissonant intervals bring it about that the consonances are relatively clear, simple, and familiar; the dissonances are blurred, complex and strange.

This theory has found an uncompromising critic in Stumpf. The position taken by him in his first article, entitled "Differenz-Töne und Konsonanz,"²⁸ is one from which he has receded but little in all the controversy which has followed.^{29, 30} He begins by raising the question of fact as to the relation of difference tones stated by Krueger. Passing that by, he argues that even if Krueger's findings be admitted, they still fail to provide an adequate criterion of consonance and dissonance as we distinguish them in musical experience. Krueger's physical test of a perfect consonance is, as we have seen, the absence of beats and of

²⁵ Op. cit., II, 15.

²⁶ Op. cit., II, 38.

²⁷ Op. cit., II, 42.

²⁸ Stumpf, *Zeitschrift für Psych.* 1905, III, 30-39-40.

²⁹ Krueger, *Psych. Studien*, I, II, III, V.

³⁰ Stumpf, *Zeitschrift für Psych.* 1906, IV, 1-10.

intermediate tones among its difference tones. But Stumpf³¹ calls attention to the fact that the interval 8:11, an extreme dissonance, gives the difference tones 3, 5, 2, 1, 1. The smallest interval here is a fifth, hence there can be no difference tone beats or intermediate tones. It should therefore be a perfect consonance! Stumpf³² cites a large number of more or less dissonant intervals, such as 11:15, 13:18, 12:17, 7:10, in all of which the smallest difference tone interval is a major third. All should, therefore be consonances, but not one of them is heard as such. He shows further the possibility of constructing triads which contain no difference tone beats, but yet are dissonant in every interval. Such, for example, 3:7:10; 7:10:17; 10:17:27. And, lastly, it is possible to rule out difference tones altogether by sounding two tuning forks very softly one in one ear, the other in the other, but this makes absolutely no difference as to the consonance of the interval.³³

Not only does the elimination of the effects described by Krueger fail to alter the characteristic quality of a dissonance, but the artificial production of corresponding effects in a consonance does not alter its consonance. Stumpf points out that if we play the triad c-e-g, and play in addition to it low beating tones in the base, the triad keeps its characteristic quality.³⁴ The properties of the lower part of the tone complex do not go over into those of the upper part. Exactly the same is true of the colliding overtones on a piano, which do not make a given triad less consonant. In short, the criteria of consonance must be peculiar to the tones as such, and not due to any external feature superadded to them.

STUMPF

There remains Stumpf's own theory, which at present enjoys a wider recognition than any other. As against Lipps, he maintains that the criterion of consonance must be sought in conscious

³¹ Stumpf, *Differenz-Töne und Konsonanz*, *Zeitsch. f. Psych.* 39, 273.

³² *Op. cit.*, 273.

³³ *Op. cit.*, 274 ff.

³⁴ *Op. cit.*, 275 ff.

experience; as against Helmholtz and Krueger, that it must be a quality inherent in the tones themselves. After eliminating all other possible psychical conditions, he concludes that the ultimate psychological factor is to be found in the fusion of simultaneous tones; that is, in the relation of two sensory contents by which they form not a mere sum but a unit.^{85, 86} The general law is, briefly, the greater the fusion the more perfect the consonance.

Fusion, according to Stumpf, is something quite distinct from unanalysability. In fact, the fusion of a given interval is distinctly present as such to consciousness only after analysis has taken place. It is the qualitative unity that persists in a chord after its real unity, *i.e.*, its indistinguishability, has given way to an awareness of separate intervals.⁸⁷ This residual qualitative unity, says Stumpf, is an original relation, just as a sensation of color, only to be learned by examples, and not to be referred to anything more ultimate psychologically.

The validity of Stumpf's psychological position depends, as Krueger has shown,⁸⁸ in a large measure on his ability to maintain clearly the difficult distinction between the unity which disappears with analysis and the unity which remains after analysis. Here is a difficulty of which he has acquitted himself only imperfectly, and sometimes not at all. His experimental demonstration of the various fusion degrees is made according to difficulty of analysis.^{89, 40} Stumpf justifies this by saying that, other things being equal, a low degree of fusion will actually mean greater ease of analysis. His tests were made with unmusical persons whose powers of analysis would naturally be comparatively low. He found that the likelihood of their judging an interval to be a single note was greatest in the case of an octave; next in order came the fifth fourth thirds sixth and

⁸⁵ Tonpsychologie, II, 127 ff.

⁸⁶ Konsonanz und Dissonanz, Akustik und Musik, II, 1, 1, 1.

⁸⁷ Konsonanz und Dissonanz, 43 ff.

⁸⁸ Archiv für die Ges. Psych. I, 236 Differenzierung der Konsonanz.

⁸⁹ Tonpsychologie II, 142 f.

⁴⁰ Konsonanz und Dissonanz, II, 1, 1, 1.

sevenths.^{41, 42} He argues that the particular difficulty of analysis experienced in these cases cannot be due to beats or overtones, since the results are the same when pure tones are used; it cannot be due to absolute distance, since the largest of the intervals, the octave, presents the greatest difficulty; it cannot be due to feeling value, since the greatest feeling accompanies hearing the thirds and sixths which lie in the middle of the series; and it cannot be due to practice, since the persons are unpracticed in music. Therefore, it must be the qualitative unity, or fusion, that makes distinguishability less in the case of the octave and more in the case of the seventh.

Manifestly Stumpf does not get beyond unanalysability as the experimental fact. Fusion, which is assumed to be distinct from difficulty of analysis, is after all arrived at only by a process of eliminating other possible factors in a given case of difficult analysis. The Stumpfian dilemma, as his critics see it, is that whenever he sharply separates fusion from difficulty of analysis, what remains is not a clearly understood psychological principle, but only a new name for the different degrees of harmony.⁴³ Nevertheless, in spite of considerable disagreement among various authors as to the exact meaning of tonal fusion, it is quite generally agreed that Stumpf has pointed the way to proper treatment of the problem.

It is not the purpose of this discussion to argue specifically for or against any of the consonance theories just mentioned. They all have their spokesmen who are contributing abundantly to the controversial literature of psychology. My own contention will be that all of them have, by giving too slight attention to the genetic aspect of the problem, left an inadequate account of certain facts which sooner or later must be explained. A complete theoretical account of consonance will have to reconcile the sharp sensory distinction between a consonant and a dissonant quality with the fact that the dividing line between consonance and dissonance has shifted in the last thousand years.

⁴¹ *Tonpsychologie* II, 145-179.

⁴² *Konsonanz und Dissonanz*, 36, 37

⁴³ *Krueger* Op. cit. II. 225, 236.

If we examine Krueger's theory in this connection we see that he provides adequately for a fundamental breach between consonance and dissonance, but it is something given once for all. If all consonances are free from difference tone beats, while all dissonances contain them, we have indeed the basis for a sharp qualitative distinction, but it is one which would seem to preclude the possibility of change. Krueger does, to be sure, mention the fact that our more frequent hearing of an interval makes it more familiar,⁴⁴ but this is merely incidental to the fundamental criterion above mentioned.

Stumpf has had much to say about the genesis of music,^{45, 46} but concerning the genesis of consonance his position is not unambiguous. In his *Tonpsychologie*, II, 208 ff., he takes up the question whether frequent combination is the cause of fusion. This question he answers in the negative as far as the possibility of psychic combination is concerned. But what is denied as a psychic possibility is, a few pages later, affirmed as a physiological possibility.⁴⁷ The physiological basis of fusion, he says,⁴⁸ is the specific synergy of the cortex, that is, "a determinate mode of coöperation of two nervous structures, having its ground in the structure of the brain, of such a kind that whenever the two structures give rise to their corresponding sensations there arises at the same time a determinate degree of fusion of these sensations." And on page 215 he makes the suggestion that "the mechanism of synergy, connate with the individual, may have been acquired in the course of generations, and perhaps the influences under which it took shape may have been in part mental influences. Impressions that affect the sense organ, together with relative frequency, might gradually set up a disposition to conscious fusion." He even goes further and ventures a few "vague conjectures," to use his language, as to how some of the early intervals were established. At this time he

⁴⁴ Op. cit., II, 42.

⁴⁵ Stumpf, *Geschichte des Consonanzbegriffes*; *Abhandlungen der Bayerischen Akad. der Wissenschaften*, Vol. XXI, 1901.

⁴⁶ Stumpf, *Die Anfänge der Musik*.

⁴⁷ *Tonpsychologie*, II, 211, ff.

⁴⁸ *Ibid.*, 214.

evidently admitted the probability that the fusion grades had been partly developed since the advent of the human race. He has, however, since modified this position, and in *Die Anfänge der Musik*, page 84, he makes the following statement: "In der Tonpsychologie legte ich die Meinung zugrunde, dass in den Uranfängen des Menschengeschlechts die Verschmelzungsunterschiede doch noch nicht vollständig ausgebildet gewesen seien, was ich jetzt nicht mehr für wahrscheinlich halte. Die Untersuchung der Sinnesempfindungen bei den heutigen Naturvölkern hat immer mehr gezeigt, dass wesentliche Unterschiede gegenüber den unsrigen nicht vorhanden sind." And in the same book, page 27, he says of the original character of the fusion grades: "Diese Einheitlichkeit des Zusammenklangs ist . . . nicht eine Folge der musikalischen Entwicklung, sondern eine durch die Natur der Töne oder der ihnen zugrundeliegenden Gehirnprozesse notwendig bedingte Erscheinung." He proceeds here to suggest the probability that the octave is as consonant for animals as for us, the only difference being that they do not attend to its consonance relation as such. Two more statements will serve to make clear his present position as regards the evolution of consonance. In *Akustik und Musik*, Vol. I, p. 32, we read: "Es scheint eine Verschiebung des Lustgefühls von der vollkommensten Konsonanz gegen die unvollkommen hin stattzufinden, aber eine Verschiebung der Konsonanzverhältnisse selbst findet nicht statt." And, lastly, in his article entitled *Konsonanz und Konkordanz*,⁴⁹ "Konsonanz und Dissonanz sind nur graduell verschieden, und es kann der Grenzstrich an verschiedenen Punkten der Reihe gelegt werden, wie er denn tatsächlich von den alten schon nach der Quarte gezogen wurde, während wir ihn nach der Terzen und Sextengruppen setzen und spätere Musiksysteme ihn vielleicht nach den Siebern setzen werden." Taken together, the above statements indicate that the genetic problem of consonance that now exists for Stumpf is limited to the changing feeling attitudes toward the various intervals. The change of an interval from dissonance to consonance signifies for him nothing more than our growth in power to conceive an interval differently in a developing

⁴⁹ *Zeitschrift für Psychologie*, Vol. 28 p. 341

system of harmony. As to the actual degree of consonance or fusion of a given interval, that is something which has not changed since the beginning of the race. Such change as took place before then is to be accounted for in purely physiological terms; the influence of external stimuli is apparently of little account.

This negative attitude of Stumpf on the genesis of consonance seems to have been generally accepted without protest. The only article I have been able to find in which the opposite position is definitely stated is that of Professor R. M. Ogden.⁵⁰ He admits that Stumpf's arguments against the possibility of "inseparable associations"⁵¹ are irrefutable, provided we regard the phenomena of consonance to be the result of an individual's tonal experience during his own lifetime, but thinks that "we have every reason for supposing that congenital associations result from racial experience of a definite and oft recurring sort, to produce instinctive modes of adjustment and attendant complexes of experience which are fundamentally unified." The last statement is in full agreement with the position to be defended here, but I disagree as to the irrefutability of Stumpf's arguments against the formation by the individual of "inseparable associations."

In the first place, a consonance, conceived as an association, does not possess the degree of inseparability which Stumpf assumed to be necessary in his *Tonpsychologie*. The interval c-a^b is quite consonant so long as we imagine ourselves in the key of A flat. But imagine the same interval to be c-g[#] and it at once takes on a character of decided dissonance. Stumpf has given what seems to be the proper explanation of this fact;⁵² to be sure, not the same explanation which he had given previously in *Akustik und Musik*,⁵³ but this point does not concern us here. The important fact is that if we conceive the imperfectly consonant minor sixth as an association, it is an association easily

⁵⁰ R. M. Ogden, A Contribution to the Theory of Tonal Consonance, in Psych. Bull. VI, 297-303.

⁵¹ Ibid., 299.

⁵² Konsonanz und Konkordanz, 334, 335.

⁵³ Akustik und Musik, Vol. I, 104 ff.

altered according to the musical context. In fact, musical dis-orientation easily affects our whole system of tonal relations if we are introduced for a time into a realm of amusical tone-steps. The usual acoustical perspective can by such a process be so altered that even the octave may appear equal to the higher fifth.⁵⁴

But waiving the question as to how inseparable is the fusion of a consonant interval we may recall that certain observed facts, notably those of synaesthesia, favor strongly the view that relatively inseparable associations can, under certain conditions, be formed by an individual, especially during childhood. C. S. Myers⁵⁵ points out that in all his cases of colored audition it is the *individual* experience which seems to determine just what particular color will be associated with a given tone. A tone that is "seen" by one subject as green, appears to another as blue, and to another as yellow. "Two members of the same family, both endowed with synaesthesia, will hotly dispute the colors of certain words or vowels." Myers sees no possible explanation of these individual variations otherwise than as accidental personal associations. That this sort of association may be formed relatively late is indicated by the fact one subject reported on by Myers⁵⁶ acquired, by playing with figures at the age of twelve, the rather troublesome habit of seeing with every letter of the alphabet its individual number. This association, acquired at random, has persisted for eighteen years, although it is not invariably present. Myers thinks that the general setting in which such connections are found must be a strong tendency to a certain kind of association,—“a ‘sympathy,’ the psychological and physiological basis of which is unknown to us.” It seems to me that in any consonance theory some such ‘sympathy’ as this must be assumed as the basis of the greater affinity of the simpler related tones, but this does not prejudice the possibility that the actual realization of definite fusion developments may take place during the lifetime of an individual. Certainly the cases of

⁵⁴ L. E. Emerson, *The Feeling Value of Unmusical Intervals*, Harvard Psych. Studies, II, 269, 270.

⁵⁵ C. S. Myers, *A. Case of Synaesthesia*, Brit. Journ. Psych. 1911, IV, 228-238.

⁵⁶ *Ibid.*, 228

Myers leave little doubt that synaesthesia, at least, though in general a congenital endowment, may in its particular manifestations be the result of the individual's life experience. To be sure, the "associations" in these are sensory rather than ideational. But exactly the same is to be said of that peculiar association of two tones which causes them to be heard as relatively unified.

Stumpf argues⁵⁷ that if mere being in consciousness innumerable times necessarily involved fusion, the person who heard music frequently should be less capable of differentiating the two notes of an octave than one who heard music seldom. But "fusion," according to his own definition, is something distinct from analysability; it is a qualitative unity that persists unaltered even after the ear has discriminated the two elements involved. If Stumpf maintained his own definition with strict consistency he would have to admit that the very practice which improves the musician's ear in discriminating the two component tones of an interval may be at the same time fusing the same two tones into a more perfect qualitative unity. It is only his failure to make clear the psychological difference between the two kinds of unity—the numerical "Einsheit" which disappears with analysis, and the qualitative "Einheit" which remains after analysis—that enables him to argue, now as if fusion were identical with difficulty of analysis, now as if it were something altogether distinct.

The view to be maintained by the writer, that a tonal fusion means simply a highly permanent connection wrought under the direct influence of repeated sensory co-incidences, is rejected by Stumpf largely on "a priori" grounds; there is no record that this possibility was ever put by him to experimental test. The experiments to be reported in this paper were devised with the view to examining the characteristic changes effected by the frequent or prolonged hearing of certain intervals, it being my hope that an interpretation of these changes might throw light on the possibility of fusion changes denied by Stumpf. But before passing to the experimental results I shall state my own theoretical position more fully.

⁵⁷ *Tonpsychologie*, II, 2, 3.

CHAPTER III

A GENETIC THEORY

I shall begin with an hypothetical account of the genesis of consonance, the theoretical soundness of which will be tentatively assumed. After an examination of the historical and experimental facts to which it offers an explanation, I shall return to a discussion of theoretical points in detail. The tentative hypotheses with which we shall begin are:

(1) Consonance and dissonance have to do directly with the degree to which an individual is able to synthesize^A two tonal elements into a unified complex; this synthesis is unaffected by analysis, and the individual's ability to analyse is unaffected by synthesis.

(2) Tonal synthesis may be conceived as an undertaking, attended by success or failure on the part of the hearing organism. Success is experienced as the characteristic quality of consonance, failure as that of dissonance.

(3) The inherent feeling value of a particular interval is a function of two factors—success and degree of difficulty. It is the barely successful synthesis, in which the individual accomplishes with difficulty the unification of a manifold, that he finds the keenest pleasure. If the synthesis is effected without effort he has little awareness of his accomplishment, and therefore little pleasure. If, on the other hand, in spite of his synthesizing activity, he is baffled by the complexity of the manifold to be unified, he experiences acute displeasure.

(4) A certain initial disposition to synthesis is pre-determined for a given interval by the simplicity of its numerical ratio. That is to say, we must pre-suppose a "sympathy" such as that which Myers postulates in the case of synaesthesia,—a more or

A. I shall use the expression "synthesis" instead of "fusion" in order to avoid the ambiguity which has come to attach to the latter term. By "synthesis" I mean simply a kind of fusion that is wholly independent of analysability.

less strong tendency to association, the strength of the tendency being proportionate to the simplicity of the ratio.

(5) The actual degree to which any given interval realizes its possibilities of synthesis depends upon the number of times the interval has been attentively heard by an individual or by his ancestors.^A

(6) Between the distinct qualities of consonance and dissonance lies an intermediate zone of neutrality such as, for the present-day ear, 4:7 relation. In such cases neither success nor failure of synthesis is pronounced. It is characteristic of this zone that change due to frequency of hearing is more marked than for either the decided consonances or dissonances. Also we are likely to find the greatest individual differences in judgment of an interval when it has reached this stage.

Our task will be to show that the above assumptions, taken together, are the ones which best satisfy the facts. Now the facts with which we are concerned are of two kinds, those derived from the actual development of musical feeling, and those derived from experiments designed to test the conditions of consonance. We may examine the two kinds in order.

A. The addition of the phrase "or by his ancestors" may seem to imply an anti-Weissman point of view on the question of acquired characteristics. I should be unwilling to admit, in the present state of that controversy, that such an implication would necessarily invalidate my position, but I do think it possible to account for the transmission, here assumed, as a purely social one. If thirds are acquired as a consonance by one generation they will be heard so much the more by the next, and will therefore become more completely consonant. We may accordingly interpret the degree of synthesis of a given interval either in Lamarckian fashion, as being in part a congenital association resulting from race experience, or, in accordance with Weissmanian principles, as resulting entirely from an individual experience which has had the benefit of the accumulations of the race. I am indebted to Professor B. M. Anderson for the suggestion that such accumulation of increments of advance as is here involved may be plausibly assumed in any society where the musical profession tends to run in families. The son, on account of his father's tutoring, reaches at an earlier age the point attained by his father, the grandson surpasses the son, and so on.

CHAPTER IV

EVIDENCE FROM THE HISTORY OF MUSIC

Historical evidence does not go back far enough to either prove or disprove the statement that the octave was originally a dissonance. To be sure, we have what at first might be taken as evidence to the contrary in the findings of von Hornbostel and others⁵⁸ to the effect that our present-day "primitive" peoples of Africa and Asia make abundant use of parallel octaves, and in a large number of cases, even of fifths and fourths. But it hardly needs to be argued that the conditions here are far from original. Even Stumpf, who makes much of anthropological evidence as proof that the present-day fusion grades were an original possession of the race, makes the following admission:⁵⁹ "Wir müssen freilich auch schlieszen, dasz das meiste, was bei den Naturvölkern an Musik beobachtet wird, keineswegs die allerprimitivsten Zustände darstellt, sondern mindestens in Hinsicht . . . des ganzen Aufbaues doch schon vielfach eine lange Geschichte hinter sich hat." And C. S. Myers,⁶⁰ speaking of the contamination of primitive music, expresses the opinion that unpolluted aboriginal music can hardly be found nowadays owing to the almost universal influence of the trader and the missionary. He says: "We have evidence of these conditions in the influence of Arabic and Portuguese tunes introduced into Africa, and in the spread of favorite native airs throughout North America and throughout Australia."

If we examine still more rudimentary conditions we again find that such evidence as is obtainable is inconclusive. Experiments have been made⁶¹ to see whether a bird, by hearing its melody played after it in another key, could be induced to transpose. No variation on the part of the bird could be noticed. The

⁵⁸ E. M. von Hornbostel, *Zeitschrift f. Angew. Psych.* III, 465-487.

⁵⁹ *Die Anfänge der Musik*, 48, 49.

⁶⁰ *The Ethnological Study of Music*, *Anthropological Essays*.

⁶¹ *Anfänge der Musik*, I 2

nearest approach to transposition is a case reported by B. Hoffman.⁶² He tells of a woodpecker who began by singing his song between c^3 and a^3 , but with prolonged singing gradually flattened until he was singing the same song between a^2 and f^3 . However, as Stumpf remarks,⁶³ this seems to be due to a mere weakening of muscular contraction rather than to anything like a conscious transposition. There is so far not a shred of positive evidence that a bird has any conception of tone relation regardless of a particular pitch. Hence we may hardly suppose that an octave could be felt by it as in any relation to its fundamental. Such a lack, of course, does not mean that the two notes together would necessarily give a combinational quality of dissonance; we must rather suppose that at this stage everything except the particular note being sung is a meaningless confusion. But the later course of development will, I think, make plausible the assumption that the octave was the earliest dissonance. We may, at least, without prejudice to the facts that come later, accept this inferred fact as an historical starting point.

It is obvious that the octave enjoyed an early advantage over every other possible interval in its progress toward consonance. Its simplicity of ratio 1:2 predetermined for it a rank second only to that of the unison 1:1. And two circumstances led to a speedy development of its possibilities. For one thing, whenever any sound was heard that contained as many as sixteen overtones,⁶⁴ the interval of the octave would occur among the upper partials nine times, as compared with five fifths, four fourths, three major thirds, three major sixths, two minor thirds, two minor sixths, two natural sevenths, one major second, one minor second. For another, the natural difference of register for the voice of a man and a woman is such that in attempting to give the same tone they would naturally give two tones an octave apart.⁶⁵ In the primitive activity of calling signals, when every member of a company tried to hold fast to a certain tone this must have

⁶² *Anfänge der Musik*, 78.

⁶³ *Ibid.*, 78, 79.

⁶⁴ Stumpf, *Tonpsychologie*. II, 200

⁶⁵ *Ibid.* 215, 216

happened often. The natural frequency with which the octave was heard would, according to the theory here maintained, bring it about that its degree of synthesis gradually increased until it passed the threshold of consonance. It was then felt as more pleasant than the unison because this success was as yet attended by considerable difficulty.

Now we actually find that among the ancient Greeks the octave had reached this point of pleasantness. They derived an acute pleasure from the practice of *magadizing*, that is, of having the men and boys sing in melody together in octaves. Thus in the *Aristotelian Problems*⁶⁶ we find the following question asked: "Why is antiphonal singing (i.e., in octaves) more beautiful than singing in unison?" This characteristic experience of the octave was, as we shall see, set apart as essentially different from any other possible combination of two notes, but it is a question how far the Greek separation of the octave from other intervals corresponded to our division of consonance and dissonance. Indeed the question of what kind of musical consciousness to attribute to the Greeks is one that has undergone an extraordinary change of interpretation within the last fifty years. Gevaert⁶⁷ is authority for the following statement: "*Il fut un temps—qui n'est pas loin—où beaucoup de savants philologues et musiciens prétendaient dénier aux Grecs tout usage de l'harmonie simultanée. Quelques-uns allient plus loin, et voulaient même leur refuser la simple connaissance des accords de deux sons. LeMémoire de notre éminent helléniste Auguste Wagener sur la Symphonie des Anciens, publié en 1861, mit à néant ces opinions erronées.*" But if philologists and musical scholars before 1861 went too far in what they denied to the Greeks, those of to-day are surely swinging too far in the opposite direction. The commonly accepted view now is that the Greeks classed the octave, fifth and fourth together as consonances in exactly the same way that we do to-day. The only difference, it is maintained, between their consonance-dissonance division and ours is that for them the dissonance began with the thirds and sixths, whereas we include the latter among

⁶⁶ Gevaert et Vollgraff, *Les Problèmes Musicaux d'Aristote*, Problem 39a.

⁶⁷ *Op. cit.*, p. 136.

the consonances. Now if change has clearly taken place in the case of the thirds during the last 2000 years, and we shall see that there is no possibility of doubting this, there should be at least a certain presumption in favor of expecting development in the case of the fifths and fourths. Evidently the scholars since 1861 have not approached the Greek passages with this presumption in mind. It has lately been agreed to translate *συμφωνία* as "consonance"; and since the fifth and fourth are from very early times spoken of by the Greeks as *συμφωνίαι*, it is argued that they must have always been full-fledged consonances. But if one sets out by translating *συμφωνία*, not as consonance in a rigid sense, but as any interval easily available in music—including both perfect and imperfect consonances, and the milder dissonances as well—the whole literature takes on a new aspect. I think there is every warrant for this translation. Certainly the passages referred to by Stumpf in his *Geschichte des Consonanzbegriffes* bear readily the interpretation that the fifth was to the Greek approximately what the third was to the fourteenth century ear, that is to say, between the positions of the third and the minor seventh for the present-day ear. This intermediate value of the fifths and fourths would account for the comparatively late period at which a clear-cut distinction between consonance and dissonance occurs. Neither in Aristotle nor in the Problems attributed to him is there any particular expression for dissonance.⁶⁸ But we do have abundant evidence that the octave was in almost every respect set apart from all other intervals. Thus we read in the *Problems* 35a,⁶⁹ *Διὰ τί ἡ διὰ πασῶν καλλίστη συμφωνία*; "Why is the octave the most beautiful *συμφωνία*?" Again, in Problem 18, *Διὰ τί ἡ διὰ πασῶν συμφωνία ἄδεται μόνῃ; μαγαδίζουσι γὰρ ταύτην, ἄλληνδε οὐδεμίαν*. "Why is only the octave employed in singing? One magadisises (i.e., sings in parallels) the octave, something which is done with no other *συμφωνία*." And in Problem 17: *Διὰ τί διὰ πέντε καὶ διὰ τεττάρων οὐκ ἄδουσιν ἐντέθωνα*; "Why are fifths and fourths not sung in parallel after the fashion of the octave?"

⁶⁸ Stumpf, *Geschichte des Consonanzbegriffes*, pp. 10-11.

⁶⁹ Gevaert et Valerius, *op. cit.*, p. 10.

And in Problem 39b there is a longer passage to the effect that the octave is the only *συμφωνία* which can be used at the beginning or end of a musical piece, since it alone procures for the mind the impression of equilibrium and stability.

These four passages leave little doubt that at the time of Aristotle, or shortly afterward, the fifth and fourth were markedly less consonant than the fifths of the ninth century or the thirds of to-day. They could not be sung in parallels, and they could not be used at the end of a piece. Moreover, they were less beautiful than the octave, whereas for us the most imperfect of the consonances is the most beautiful of them all.

There is, to be sure, some indication that parallel fifths and fourths were occasionally played on instruments. Plato (*Laws*, VII, p. 812) speaks of the use by the zitherist of antiphonic successions other than that of the octave. If Gevaert's⁷⁰ interpretation of this passage is right, it can only refer to the doubling of the melody in fourths and fifths. But it also seems not unlikely that players of instruments, so far as they did this, were taking a step in advance of popular practice. The following passage from the *Republic*⁷¹ indicates a strong probability that at least some of the men who handled instruments were experiencing pleasure in effects which seemed absurd to the people at large. It reads: "'Tis as good as a play to hear them talking of their condensed notes, as they call them; they put their ears alongside the strings like persons catching a sound from their neighbor's wall—one set of them declaring that they distinguish an intermediate note, the others insisting that the two sounds have passed into the same—either party setting their ears before their understanding." "These gentlemen who tease and torture the strings, and rack them upon the pegs of the instrument" would, according to the theory here defended, quite certainly have developed the fifth from its status as an intermediate interval to one of consonance. Their position, we may imagine, was not unlike that of the singers in the fourteenth century who were forbidden by the conservative Pope John XXII to sing thirds, on the

⁷⁰ Op. cit., 149.

⁷¹ Gevaert's *Plato Republic VII*, 53c 1 f.

ground that this interval served only to intoxicate and not to soothe the ear.⁷²

Those authors who hold that the fifths and fourths were consonances for the Greeks⁷³ begin by calling attention to the Pythagorean harmonic proportion 12:8:6, or c-g-c. But the inferences that can be made from this leave much to be desired. In the first place, it is not clear that the Pythagoreans had reference to the consonance of simultaneous tones. The term *κρᾶσις* employed by them in this connection, was very frequently used to refer to the combination of successive impressions, as of consonants and vowels in speech.⁷⁴ And further, it is not clear that their "consonances" were not predetermined to a certain extent rather by calculation and measurement than by the actual report of the senses. At least, according to Boethius,⁷⁵ the late Pythagoreans Nikomachus, Ebulides, and Hippasus, obtained each a different classification of the consonances because they founded their calculations on different arithmetical speculations! How remote the Pythagorean "harmony" may have been from the actual harmony of musical combinations, as experienced, may be judged by their much reiterated conception of the harmony of the spheres, which, if it means the simultaneous sounding of all the heavenly bodies, would make out of the cosmos the most atrocious discord.

Plato can be relied on even less than the Pythagoreans to give us an exact notion of the Greek consonances. According to him⁷⁶ the intimations of the senses are so imperfect that it is absurd to take pains in investigating the exact truth about them. Just as the true study of astronomy lets the heavens alone so the true study of music limits itself to the natural harmonies of mathematical relations, and reflects not on the harmony of sounds, but on that of numbers.

We have already seen that the only Aristotelean evidence con-

⁷² Oxford History of Music, II, 91.

⁷³ Stumpf, *Geschichte des Consonanzbegriffes*, 6.

⁷⁴ Stumpf, *Ibid.*, 8

⁷⁵ Stumpf, *Ibid.*, 55.

⁷⁶ Jowett's Plato. Republic, VII, 530 ff.

cerning the consonance of the fourths and fifths is their inclusion among the *συμφωνίαι*. This word is defined by Aristotle as a numerical relation between a high and low tone. There is no mention of a specific difference between consonance and dissonance; in fact, no single word that we can translate by dissonance.⁷⁷ *συμφωνία* is usually referred to simply as *λόγος* or relation. Indeed, it is significant that Aristotle asserts, as against the Pythagoreans, *Met. N. 6. p. 1093 a. 20-25*, that there are more than three *συμφωνίαι*; Stumpf,⁷⁸ in order to translate the Greek by "consonance" in this passage, is obliged to assume that Aristotle refers here to the addition of such intervals as the double octave and twelfth. But if we accept the translation "musical interval," we may interpret this passage, more naturally, it seems to me, as the incipient recognition that smaller and more dissonant intervals than the fourth and fifth are beginning to impress themselves as musically useful.

By the time of Thrasyllus, first century B.C., the classification of intervals has become very definite,⁷⁹ but it is noteworthy that the fifth and fourth are put in a class distinct from the octave. Thrasyllus makes a threefold distinction, (1) antiphonies, including the octave and double octave; (2) paraphonies, including the fifth and fourth; (3) diaphonies, including the intervals mentioned in the other two classes. Five centuries after Thrasyllus, the same classification is used by Gaudentius, but a significant change has taken place. The fifth and fourth are ranked by him in the same class with the octave; the third is now a paraphony, and the diaphonies include the intervals more dissonant than the third and tritone.⁸⁰ If paraphony, as used by Thrasyllus, is at all comparable to the same expression as used by Gaudentius we cannot but conclude that as late as the first century, B.C., the fifths and fourths were still in the intermediate class, but that by the fourth century, A.D., they had developed into full-fledged consonances. And it is an interesting fact in this connection that

⁷⁷ Stumpf, *Geschichte des Consonanzbegriffes*, 22.

⁷⁸ *Ibid.*, p. 34.

⁷⁹ *Ibid.*, 48 ff.

⁸⁰ Stumpf, *Ibid.*, 69-71.

whereas the Aristotelian Problems speak only of the octave as the most beautiful interval, Bacchius in the fourth century A.D. mentions both the fifth and the octave, with the fifth first in order.⁸¹

By the ninth century the fifth is obviously a new kind of harmonic experience. The Christian church has found that a more beautiful concord than the octave is to be obtained by having concurrent voices sing in fifths.⁸² In its practice of "organizing," the "vox principalis" sang the melody and the "vox originalis" sang the accompaniment in the fifth below in parallel movement. The great pleasure which ninth century singers derived from this sort of music cannot be satisfactorily disposed of with the statement that they merely felt themselves to be diversifying the means by which a melody might be sung in unison. The writings of this period indicate clearly that the interval of the fifth made very much the same appeal to them that the third makes to us to-day. Thus Regino, in his *De harmonica Institutione*, written in 892,⁸³ "Consonantia est acuti soni gravisque mistura, suaviter uniformiterque auribus accidens. Et contra dissonantia est duorum sonorum sibimet permistorum ad aurem veniens aspera atque iniucunda percussio." Their distinction of consonance and dissonance was evidently quite as definite as ours; the only difference is that the third is as yet distinctly not included among the combinations described as "suaviter auribus accidens."

Taken as a whole, the evidence from the time of Aristotle to that of the ninth century points very strongly to a gradual advance of the fifth from being almost a consonance to being the most acceptable one. The reason why its development should follow next in order after the octave is obvious. Since it is the next simplest ratio 2:3, it requires further, according to our theory, only that it be impressed on the hearer sufficiently often. Now we have seen that as regards natural frequency of occurrence in overtones the fifth comes next in order after the octave, since it occurs five times to the first six overtones of any

⁸¹ Ibid., 66

⁸² H. E. Woodcock, *Music and the Middle Ages*, p. 100.

⁸³ Woodcock, op. cit., p. 100.

clang. Moreover, it was probably quite often produced in the earliest times by tenor and bass voices calling together naturally at medium pitch. And its development among primitive people would be peculiarly favored by their practice of singing simultaneous melodies. Of this characteristic primitive practice von Hornbostel observes:⁸⁴ "Alle diese Formen von Mehrstimmigkeit sind dadurch charakterisiert, dass mehrere Melodien zwar gleichzeitig erklingen, aber doch noch als Melodien. Auf die einzelnen Zusammenklänge als solche wird wenig oder gar nicht geachtet." Now whenever in such a chaos of sound there appeared a fifth, with its initial advantage of simple ratio and frequent natural occurrence, it is obvious that it would tend to become fixed rather than any other dissonance, and each time it was thus attentively observed its proximity to consonance would be increased. This is exactly what seems to have happened in every case. Those of the primitive and semi-civilized people of to-day who have advanced beyond the stage of parallel octaves, have invariably acquired, as the next step, parallel fifths and fourths. This is true not only of their singing, but of their playing on instruments. Stumpf⁸⁵ observes that in China, Japan and Sumatra it is quite usual that instruments make fifths and fourths with each other or with the orchestra.

The shifting division between consonance and dissonance seems to have included the fourth among the consonances almost simultaneously with the fifth. Its development among the Greeks exactly parallels that of the fifth; all the primitive races to-day who treat the fifth as a consonance treat the fourth likewise; and the ninth century "organum" which in its simplest form gave only parallel fifths, gave also parallel fourths when the "vox principalis" was doubled an octave above, and the "vox originalis" an octave below. But the simultaneous development of these two intervals may be readily understood according to the principle of the equivalence of octaves. That is to say, the resemblance between any note and its octave is now so strong that the presentation of

⁸⁴ E. von Hornbostel, Ueber vergleichende akustische und Musik-psychologische Untersuchungen, p. 484. Zeitschrift für Angew. Psych. III, 1909.

⁸⁵ Anfänge der Musik

any given fundamental is practically equivalent to the presentation of its octave. Hence, whenever a fourth was sounded, it could be heard, not as a new interval different from the fifth, but merely as the old interval inverted—and even perhaps a little more interesting, because of the slight increment of difficulty. The same principle explains the later simultaneous development of thirds and sixths; it is indeed the fundamental fact in all the triad inversions of modern harmony.⁸⁶ It is significant, however, that by the time of Guido of Arezzo, the first half of the eleventh century, the foundation of the Diaphony or Organum was no longer the fifth, but the fourth.⁸⁷ The perfect fifth has ceased to be the barely consonant and therefore intrinsically interesting interval it had been two centuries previously. As it becomes more consonant it becomes less available as a parallel interval.

The major third 4 : 5 is the simplest of the dissonances that now remain, and history merely repeats itself when the third by frequent hearing gains a place among the consonances. The possibility of its attracting especial attention was obtained when the strict form of Diaphony, which was based entirely upon the traditional concords, was relaxed in favor of free Diaphony, which admitted sounds that were discordant, provided they were not introduced consecutively or on an accented beat.⁸⁸ We find it recognized in the eleventh century *Micrologus* of Guido of Arezzo, who illustrates the pleasant effect of coming to the unison by way of the major third.⁸⁹ This is at a time when the minor third is still forbidden even in passing. By the twelfth century there is evidence of the use of both major and minor thirds and sixths provided they are treated lightly. A twelfth century manuscript of the British Museum, published by Coussemaker in his *Histoire de l'Harmonie au Moyen Age*,⁹⁰ gives the following very explicit regulations with respect to the progression after thirds and sixths: "The minor third requires the unison after it; the major third, the fifth; the minor sixth, the fifth; the major

⁸⁶ Stumpf, *Konsonanz und Konkordanz*, *Zeitsch. für Psych.* 58,334.

⁸⁷ Woolredge, *op. cit.*, I, 63.

⁸⁸ Woolredge, *op. cit.*, I, 66.

⁸⁹ *Ibid.*, 66.

⁹⁰ See Woolredge, *op. cit.*, Chap. VI.

sixth, the octave." These intervals at this time evidently had their "regular" resolutions in much the same sense that the dominant seventh has its regular resolution in the harmony of to-day. The interesting transformation which has taken place in the third in the last seven hundred years is described summarily by Parry,⁹¹ when he says, "whereas in modern times a man feels that he wants something more when he is without it, in medieval times he wanted something more because he had it." A twelfth century writer, comparing his own sense of consonance with that of the Greek, might have made exactly the same statement in regard to the fifth; for this interval left the Greek with a sense of disturbed equilibrium,⁹² whereas in the twelfth century it was the regular resolution of the major third and minor sixth. The exact steps by which the almost consonant third became generally recognized as a consonance cannot be traced with certainty. Riemann⁹³ thinks the practice of singing in parallel thirds and sixths appeared earlier in England than on the continent, and that it spread from England to France. This, however, is thought very doubtful by Woolredge.⁹⁴ What interests us especially here is the fact that it seems to have had for a considerable time the characteristic attribute of an intermediate interval; that is, it had become a consonance to some ears, while it was yet a dissonance to others.¹ Pope John XXII, in a decree—1322—forbidding the use of discant, said: "Yet for all this, it is not our intention to forbid occasionally, and especially upon feast days or in the solemn celebration of the mass . . . the use of . . . the octave, fifth and fourth which heighten the beauty of the melody. . . . Used in such sort, the consonances would both soothe the hearer and arouse his devotion."⁹⁵ This decree, which excluded thirds, was set aside just fifty-five years

⁹¹ *Evolution of the Art of Music*, 88.

⁹² *Aristotelian Problems*, 39 b.

⁹³ *Geschichte der Musiktheorie im IX-XIX Jahrhundert*.

⁹⁴ *Op. cit.*, I, 161 ff.

I. We have already compared this state of affairs with the one that probably existed in Greece at one time regarding the fifth. We shall see later that the natural seventh 4:7 to-day manifests the same characteristic

⁹⁵ *Op. cit.*, I, 161.

later, when official recognition was given to the *Faulx Bourdon*, a form in which consecutive thirds and sixths were the rule. Obviously the ultra-moderns in the musical taste of that time had continued to exert such a pressure that the conservatives finally came to acknowledge their point of view as the better one.

The decline of the fourth and fifth seem just about to have kept pace with the advance of the third. Thus Simon Tunstede, an English theorist of the year 1400, in his *Quattuor Principalia*, writes:⁹⁶ "Let him who is to discant avoid as much as possible the perfect concords (i.e., fifths and fourths), keeping his part in the imperfect intervals, that is to say, in the thirds, sixths and tenths above the tenor, and with these let him discourse, ascending and descending according as it may seem to him expedient and most agreeable to the hearer." Thus the fifth and fourth, as they grew more consonant, lost the attribute which invariably belongs to the least consonant of the consonances, namely, the pleasing effect as a parallel interval.

Two possible arguments suggest themselves for maintaining the essentially consonant character of the third despite the slowness with which its recognition developed. The first is the fact that the earliest third was not the natural major third 4:5 but the Pythagorean 64:81. And indeed Helmholtz⁹⁷ suggests this as a possible reason for its treatment as a dissonance. But he himself admits later on⁹⁸ that "even when the natural major third was included in the so-called syntono-diatonic mode of Didymus and Ptolemaeus, it was not recognized as a consonance." And besides, recent experiments have shown that an interval can be considerably mistuned while continuing to give its characteristic fusion effect. It would seem then that very little weight can be attached to the slight discrepancy of the Pythagorean ratio.

A second argument is that the medieval man was so bound by what had been established before him that he refused to admit anything for which he could find no precedent in tradition. Such

⁹⁶ Woolredge, *op. cit.*, 1

⁹⁷ *Sensations of Sound*, 106

⁹⁸ *Op. cit.*, 225

an argument, if it did not concern the immediate facts of sense, would deserve careful consideration. Certainly the medievals had an extraordinary respect for established rules, but that they could have maintained in theory what their ears were constantly upsetting in practice, that they could have heard thirds as we hear them, and yet have described their impressions as they described them,—this passes understanding.

The primitive people of to-day among whom has been found the practice of singing or playing in passages of thirds are very rare, and even then it is always a question how far European influence has found its way among them. But at least it is certain that the third has invariably been the interval acquired next after the fifths and fourths. Thus G. Knosp⁹⁹ reports that the Annamites found the major triad beautiful, but the minor triad and all seventh chords were horrible and meaningless. Stumpf¹⁰⁰ reports that a member of the Siamese court orchestra, tested by him, thought our major triad pleasing, but disliked the minor triad and dominant seventh.

It might be argued that since the Siamese scale is totally different from ours, the liking of this subject for our major triad is an argument against gradually developing fusion grades. But such a conclusion does not follow from the facts. In the first place, it is to be noted that this Siamese, whom his fellows named as the best musician among them, had already gained some familiarity with European music both in Vienna and at home. He was accordingly so far spoiled in respect to being a typical example of Siamese musical evolution. And furthermore, it is certainly possible to find an interval acceptable without feeling it as a consonance. The twelfth century musician expressed satisfaction with the third—for him a dissonance—provided it was preceded and followed by a consonance, and we ourselves do as much for the dominant seventh which we still class as a dissonance. The distinction between degree of pleasure and degree of consonance is one on which every writer now insists.¹⁰¹ But

⁹⁹ E. von Hornbostel, *op. cit.*, 486.

¹⁰⁰ *Tonsystem und Musik der Siamesen*, *Akustik und Musik*. III.

¹⁰¹ *Stumpf Akustik und Musik* I 30-34.

for this distinction we should have to consider the third at present more consonant than the fifth. And lastly, if the Siamese felt the various fusion grades in the same way as a European, it might be asked why he should have objected to passages of thirds,¹⁰² and why he should have disapproved of the minor triad and of the seventh chord. So far as the facts presented warrant any judgment as to the Siamese's distinction of consonance and dissonance, we should have to say that for him the minor third is more dissonant than is the minor seventh for us, while the major third may or may not be.

The difficulty of getting definite results from the questionnaire method with primitive people, or any people differing widely from ourselves, is illustrated by the attempts of von Hornbostel to get judgments from a group of North American Indians concerning various consonant and dissonant combinations.¹⁰³ Every possible answer was obtained with regard to every chord! Myers¹⁰⁴ in the article previously mentioned, refers with considerable dissatisfaction to the attempts so far made to investigate the affective value of various pairs and triads of tones presented to savages. He says: "It is clear that both native and European intervals should be presented, that the intervals should be sounded on native and not merely on European instruments, and that repeated judgments must be obtained before reliance can be placed on such comparisons. Investigators have hoped to arrive at an answer by playing primitive melodies on the piano, harmonizing them now in one way, now in the other. But the likes, dislikes, or indifferences of the natives, ascertained by such a rough method of experiment, cannot be accepted as trustworthy." The only test of this sort which I was able to make in connection with the present investigation was to try the comparative effect of parallel fifths and thirds on one Japanese and two Chinese students at Harvard. The answer from one of them was uniformly that they were "just the same." Both of the other men usually gave a similar answer, but so far as any

¹⁰² Stumpf, *Tonsystem und Musik der Siamesen*, 106.

¹⁰³ E. von Hornbostel, *op. cit.*, 486.

¹⁰⁴ C. S. Myers, *op. cit.*, 2.

preference was expressed it was for the fifths, despite the fact that they had lived in this country for a year and a half. The inconclusiveness of these results is typical of what seems likely to be obtained by any method short of a prolonged and systematic investigation. Surely, until an exact understanding can be guaranteed between the experimenter and his subject, the safest guide in the matter of consonance and dissonance is the actual musical practice of a people, and this we find in such cases is limited almost entirely to the intervals of the octave, fifth and fourth.

If the test for the complete establishment of an interval as a consonance is its use in the final chord of a cadence,¹⁰⁵ the first historical example of the complete establishment of the third is in the Nesciens Mater, 1450,¹⁰⁶ which offers a cadence in which the middle voice passes to the third instead of the fifth. The minor chord was avoided in closes down to the time of Bach, and even Handel and Mozart occasionally indicate their desire to give greater finality to a minor piece by ending it with a major chord.¹⁰⁷ Once admitted as a consonance, the minor third has not relegated the major to a position of lower importance, but rather the constant interchanges of the two have tended to enhance the feeling value of both. It is practically impossible in our present diatonic system to get a succession of thirds which are entirely major or minor. What we get in any passage is a frequent alternation between the two; that is, between the slightly less and the slightly more consonant. This situation has been of immense advantage in maintaining the third as the most interesting interval. It has been thus doubly fortified against gradual reduction to the status of an "empty" interval as the octave and fifth before it.

Of the sixths little more need be said than that their development as consonances parallels that of the thirds. Although slightly more complex in ratio than the latter, they give, owing to the equivalence of octaves before mentioned, more the effect of inverted thirds than of intervals in their own right. Their

¹⁰⁵ Parry, *Evolution of the Art of Music*. 98.

¹⁰⁶ Oxford History of Music. II, 130-142.

¹⁰⁷ Helmholtz, *op. cit.*, 2.

simultaneous development with the thirds is accordingly to be explained on exactly the same grounds as that of the fourths with the fifths,

The next simplest ratio is the natural seventh 4:7. Its effect is peculiarly interesting on account of the fact that its development has been arrested, owing to its exclusion from the diatonic scale. It remains to-day an intermediate interval, and we find, as is to be expected, wide individual differences of opinion as to its status. Stumpf¹⁰⁸ considers it intermediate between consonance and dissonance; Hohenemser¹⁰⁹ and the Lippsians in general consider it a pronounced dissonance; Helmholtz¹¹⁰ thought it nearer to the consonances than the dissonances; and Krueger¹¹¹ and many others think it belongs distinctly among the consonances. These differences recall the state of affairs, already mentioned, in regard to the third in 1322, and make even more plausible the conjecture that has been made with regard to the fifth among the Greeks. They suggest further that if the 4:7 were included among the intervals in regular use, its frequent hearing would presently make it unquestionably consonant for all ears. This consonance would be less complete than its predecessors, but therefore all the more interesting. Unfortunately there is no instrument which makes possible with the 4:7 the kind of experiments which I am here reporting on the thirds, fifths, minor and major sevenths, but there is every reason to believe that when similar tests can be devised the results will be of the same kind.

By the time of Palestrina, 1524-1594, the division was definitely marked between the thirds on the one hand and the minor seventh 5:9 on the other. The change of attitude that has since taken place toward the latter interval is in every way indicative of a general progress in the direction of consonance. Palestrina treated the minor seventh just as the twelfth century musicians treated the third in free Diaphony. He allowed it, but only as

¹⁰⁸ Differenzttöne und Konsonanz, *Zeitschrift für Psych.* 59, 164.

¹⁰⁹ Krueger, *op. cit.*, II, 219.

¹¹⁰ *Sensations of Tone*, 195.

¹¹¹ Krueger, *op. cit.*, 219.

an "unessential" discord; that is to say, as a suspension or passing note. It thus appeared in a transient juxtaposition of voices while the main interest was directed to the movement of the parts themselves. But, shortly afterwards, interest began to attach to the interval on its own account, and Monteverde, 1567-1643, upset all traditions by attacking the dominant seventh and leading tone seventh directly without preparation.¹¹² The more recent developments are familiar to every student of harmony. Not only was the dominant seventh later allowed to enter unprepared, but feeling gradually developed in favor of extending the same license to the secondary sevenths, until now any chord of the seventh may be directly introduced without giving offense. At the same time, the feeling that the seventh required a "regular" resolution has undergone a marked alteration. In many passages from *Parsifal*, for example, one can see that Wagner, by the same device of passing notes which Palestrina used to introduce a single interval of a seventh, contrives to produce transient artificial chords with sevenths which are succeeded by new chords with other sevenths. These passing note combinations mark the incipency of parallel sevenths just as surely as the sevenths occurring frequently in half closes in *Tristan and Isolde* point to the ultimate possibility of a complete close containing a seventh. But the precaution which Wagner takes in introducing his sevenths only when attention is concentrated on the melodic progression of the parts, is brushed aside by his more modern successors. Strauss is to Wagner in this respect as Monteverde is to Palestrina. The consecutive sevenths which Wagner suggests more or less indirectly, are attacked by Strauss without any relieving contrapuntal device. The climactic parts of *Heldenleben*, and the succession of chords which comes at the end of the song *Morgen*, are but two of the numerous instances of consecutive dominant sevenths which before Strauss would have been considered absolutely impossible. More decided still is an example from Florent Schmitt's *Tragédie de Salome* in which we have a passage of ten successive sevenths.

¹¹² Helmholtz, op. cit.

It is needless to multiply instances of what contemporary orchestral compositions are coming more and more to abound in.

It might be argued that the composer is merely trying to add a novel interest to his work by daring to use progressions that would have been considered too bold by his predecessors. But a composer's choice of means is never entirely irrelevant to the potential capabilities of his hearers. His startling effects, if true products of art, must end by introducing them to a new and more ultimate pleasure. We must suppose that the great composer builds somewhat better than he knows, in that his radical harmony is not a mere bit of wanton freakishness, but a more or less unconscious finding his way to the next stage in the musical progress of the race. If this is true, the rapidly developing freedom of treatment of the minor seventh in present-day music urges strongly the experimental investigation of the effect of repeatedly hearing this dissonance which bids fair to become a consonance.

But, before proceeding to an examination of experimental results, it will be well to summarize the main points of historical development. The qualitative distinction between consonance and dissonance has at all times had most immediate reference to the self-sufficiency which always exists for the one and never for the other. The practical test of self-sufficiency, and therefore of consonance, we have taken to be the immediate effect of the interval in a passage of parallels. On the basis of this distinction we may say that the history of each of our present consonances has apparently been a gradual development as dissonance, followed by a relatively short transition period, and later a gradual development as consonance. The typical case, in which we may observe all the gradations, is that of the third. As a dissonance it was at first too harsh for any use at all, but afterwards was considered a useful interval if properly restricted. Then follows a transition during which some seem to have felt it as a consonance while others still considered it dissonant. Later comes its general admission as a parallel interval, and still later its use in the final chord of a cadence. Each stage, we have seen, had its characteristic pleasure value. As a decided dissonance

it was essentially unpleasant; as a mild dissonance it was pleasant according to the context; as a bare consonance it was essentially pleasant, as for example to Monteverde, who confessed his inordinate fondness for thirds; when it becomes a more perfect consonance it begins to take on a more empty character, as is instanced by our present growing dissatisfaction with such music as that of Donizzetti. The history of the fifth exemplifies the end of this same general process; that of the minor seventh its beginning. The fifth, recognized by the Greeks as more like the octave than any of the other intervals, though most probably not at that time a consonance in our sense, appears in the ninth century as the sweetest of concord. A few centuries later it has lost in content until consecutive fifths are become taboo; and the present-day effect of the fifth reminds one of the answer of Rossini to the question, "What is more *langweilig* than a flute?" His answer was, "Two flutes." The fifth to-day is a most essential element in the structure of harmony, but considered *per se* it has an unpleasant barrenness for most ears, so that it is not used consecutively except for particular effects. The seventh, altogether too harsh for the twelfth century ear, comes later to give much pleasure in the mildly dissonant chord of the dominant seventh, and the boldness of contemporary radicals augurs for it an eventual transition to the recent status of the third, that of the most pleasant of consonances.

CHAPTER V

EXPERIMENTAL EVIDENCE

The experiments which I shall report were performed on nine subjects during the winter of 1913-1914 under the direction of Professor Münsterberg and Doctor Langfeld in the Harvard Laboratory. The intention was to examine the characteristic effects of prolonging and repeating two consonances, the fifth and third, and two dissonances, the major and minor seventh, with a view to ascertaining whether the facts thus obtained would lend support to such an interpretation of the genesis of consonance as has been given above.

The requirements to be fulfilled by the instruments to be used were,

(1) That it be possible to sustain an interval for any length of time and at fairly uniform pressure.

(2) That two successive chords or intervals could be compared, each having been played for the same length of time and with the same amount of pressure.

(3) That passages of any desired number of parallel intervals could be played at any speed desired.

The Tonmesser and harmonium are obviously not adapted for this last requirement; it was therefore necessary to use a cabinet organ with tempered scale. The one used was an instrument made by the Estey Organ Company, Boston. The stop exclusively used was a melodia with very prominent second, third, and fourth overtones.

PRELIMINARY EXPERIMENT

Since the final conclusions were to rest on an interpretation of the subjects' comparative judgments of intervals under different conditions, it was necessary first of all to compute for every subject a co-efficient of musical consistency obtained from a series of judgments under constant conditions. And, further, since the final results were to have to do with the question of

consonance and dissonance, it was desirable to find out in advance how far the subject naturally inclined in his preferences toward the one or the other. In short, the intention of the preliminary experiment was twofold, to find out in the case of each subject his degree of consistency and his degree of consonance.

As stimuli for paired comparison in the preliminary experiment, I used nineteen resolutions of the dominant seventh chord. The expression "resolution" in these experiments is to be understood as meaning a pair of chords; the dominant seventh plus the chord into which it resolves. A comparison of resolutions hence means a comparison between two pairs of chords. The actual chords used, and their corresponding numbers as they appear in the tables follows. It will be seen that eight of the



RESOLUTIONS OF DOMINANT SEVENTH USED IN PRELIMINARY EXPERIMENT.

resolutions, 9, 10, 11, 12, 15, 16, 17, 18, are dissonant; the rest consonant.

Each of the nineteen resolutions was compared with each of the others in an irregular series, and then the same comparisons were made with the pairs presented in the reverse order. This made in all three hundred and forty-two judgments, given under conditions such that memory could not possibly have had any influence in determining the choices. The time during which each pair was sounded was kept constant by allowing two seconds for the dominant seventh and three for the following chord, mak-

TABLE II

[illegible]

TABLE III

[illegible]

TABLE IV

		G																	
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	X	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	1
2	2	X	2	2	2	2	7	2	9	10	11	12	13	14	15	16	17	18	2
3	3	3	X	4	3	=	7	3	9	10	11	=	13	14	15	16	17	18	3
4	4	4	4	X	5	=	7	4	9	10	11	=	13	14	15	16	17	18	4
5	5	5	5	5	X	5	7	5	9	10	11	12	13	14	15	16	17	18	5
6	6	2	3	6	5	X	7	8	9	10	11	12	13	14	15	16	17	18	6
7	7	7	7	7	7	7	X	7	7	10	11	12	13	14	15	16	17	18	7
8	8	2	3	8	5	8	7	X	9	10	11	12	13	14	15	16	17	18	8
9	9	9	3	9	9	9	9	9	X	10	11	12	13	14	15	16	17	18	9
10	10	10	10	10	10	10	10	10	10	X	10	10	10	14	15	16	17	18	10
11	11	11	11	11	11	11	7	11	11	10	X	12	13	14	15	16	17	18	11
12	12	12	12	12	12	12	12	12	12	10	12	X	=	14	=	16	17	18	12
13	13	13	13	13	13	13	7	13	9	10	11	12	X	=	14	=	16	17	18
14	14	14	14	14	14	14	14	14	14	10	14	14	=	X	14	16	14	18	14
15	15	15	15	15	15	15	15	15	15	10	15	15	15	15	X	16	17	18	15
16	16	16	16	16	16	16	16	16	16	10	16	16	16	16	=	X	16	18	16
17	17	17	17	17	17	17	17	17	17	17	17	=	17	17	17	17	X	=	17
18	18	18	18	18	18	18	18	18	18	10	18	18	18	18	18	18	=	X	18
19	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	X

TABLE V

		K																	
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	X	2	3	4	1	1	1	8	1	1	11	1	13	14	1	1	1	1	19
2	2	X	=	4	2	2	2	=	2	2	11	12	13	14	2	2	2	2	=
3	1	2	X	3	3	3	3	8	3	3	11	3	13	3	3	3	3	3	19
4	4	2	3	X	5	4	=	8	4	4	=	12	13	14	4	4	4	4	19
5	5	2	3	5	X	6	5	8	9	5	5	5	13	14	5	5	5	5	19
6	6	2	3	6	5	X	6	8	6	6	11	12	13	14	6	6	6	6	19
7	=	2	3	7	5	7	X	8	7	7	11	7	13	14	7	16	7	7	19
8	8	8	3	8	=	8	8	X	8	8	8	8	13	=	15	8	8	8	8
9	9	2	3	=	5	9	9	8	X	9	11	9	13	14	9	9	9	9	19
10	1	2	3	4	5	10	7	8	9	X	11	10	13	14	15	16	10	10	19
11	11	2	3	4	11	11	7	8	9	11	X	11	13	14	11	11	11	11	19
12	1	2	3	4	5	6	7	8	9	12	11	X	13	12	15	12	12	12	19
13	13	13	=	13	13	13	13	8	=	13	13	13	X	=	13	13	13	13	=
14	14	2	3	14	14	14	14	8	14	14	14	14	13	X	14	14	14	14	14
15	1	2	3	4	15	6	7	8	9	10	15	15	13	14	X	15	17	15	19
16	1	2	3	4	5	6	7	8	9	16	11	16	13	14	16	X	16	16	19
17	1	2	3	4	5	6	7	8	9	10	11	17	13	14	15	16	X	17	19
18	1	2	3	4	5	6	7	8	9	10	11	12	13	14	18	16	18	X	19
19	19	19	=	19	19	6	19	19	19	19	19	19	=	14	10	19	19	19	X

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	X	1	1	1	5	6	1	1	1	10	11	1	13	1	1	1	1	1	1
2	1	X	2	2	5	6	7	2	9	10	12	12	13	2	2	16	2	18	2
3	1	3	X	4	3	3	7	3	9	3	3	3	13	3	3	3	3	3	3
4	4	4	3	X	4	4	4	4	4	4	4	4	13	4	15	4	4	18	4
5	1	5	5	5	X	5	5	5	9	5	5	12	13	5	5	5	5	18	5
6	1	6	6	4	6	X	7	6	6	10	11	12	6	14	15	6	17	18	6
7	7	7	7	7	5	7	X	7	7	10	11	7	7	7	15	7	7	18	7
8	8	8	3	4	5	6	7	X	9	10	11	12	13	8	15	16	17	18	8
9	9	9	9	9	9	9	7	9	X	10	11	12	9	9	9	9	9	9	9
10	1	10	3	10	10	10	7	10	10	X	11	10	13	10	15	16	10	18	10
11	1	2	3	4	11	11	7	11	11	10	X	11	13	11	15	16	17	18	11
12	1	12	12	4	5	12	12	12	9	10	12	X	12	12	12	16	12	18	12
13	1	13	13	13	13	13	13	13	13	13	13	13	X	13	13	16	13	18	13
14	1	2	3	4	5	6	7	14	14	10	11	12	14	X	15	14	17	18	14
15	1	15	15	15	15	15	15	15	15	15	15	15	15	15	X	15	15	18	15
16	1	16	3	4	5	6	7	16	9	16	11	12	13	14	15	X	17	18	16
17	1	17	17	4	17	17	17	17	17	10	11	17	13	17	15	17	X	18	17
18	18	18	3	18	18	18	18	18	18	18	18	18	18	18	18	18	18	X	18
19	19	2	3	4	5	6	7	8	9	10	11	12	13	14	15	19	19	18	X

[illegible]

TABLE VIII

	M																		
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	X	2	3	1	1	1	1	1	1	1	1	1	13	1	1	1	1	1	19
2	2	X	3	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	19
3	1	2	X	3	3	3	3	3	3	3	3	3	13	3	3	3	3	3	19
4	1	2	4	X	4	6	4	4	4	4	4	4	13	4	4	4	4	4	19
5	1	2	3	4	X	5	7	5	5	10	11	5	13	5	5	16	5	8	19
6	1	2	6	4	6	X	6	6	6	6	6	6	13	6	6	6	6	6	19
7	1	2	7	4	7	6	X	8	7	7	7	12	13	7	15	7	7	7	19
8	1	2	3	8	8	8	8	X	8	8	8	8	13	8	8	8	8	8	19
9	1	2	9	9	9	6	7	9	X	10	9	9	13	14	9	9	9	18	19
10	1	2	10	4	10	6	10	10	10	X	11	10	13	10	10	10	10	10	19
11	1	2	3	11	11	6	11	11	11	10	X	11	13	11	11	11	11	18	11
12	1	2	3	4	5	12	12	8	12	10	12	X	13	14	15	12	17	12	19
13	1	2	13	13	13	13	13	13	13	10	13	13	X	13	13	13	13	13	19
14	14	2	14	4	14	6	7	8	9	14	14	14	14	X	14	14	14	14	19
15	1	2	3	4	15	6	15	8	15	10	11	15	13	15	X	15	15	15	19
16	1	2	3	4	16	6	7	8	9	10	11	16	13	16	15	X	17	18	19
17	1	2	3	4	17	6	7	8	9	10	11	17	13	17	17	17	X	18	19
18	1	2	18	4	18	6	7	8	18	10	18	18	13	14	18	18	17	X	19
19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	19	X

TABLE IX

	R																		
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	X	2	3	1	5	6	7	1	9	10	11	12	13	14	15	1	1	18	19
2	1	X	3	4	5	6	7	2	2	10	11	12	13	14	15	16	17	18	19
3	3	2	X	3	5	6	7	8	3	3	3	3	13	14	3	16	3	18	3
4	1	4	3	X	5	6	7	8	4	10	4	12	13	4	15	16	4	18	4
5	5	5	3	5	X	6	5	8	9	5	5	5	13	14	5	16	5	18	19
6	6	6	3	6	6	X	6	8	6	6	6	6	6	6	6	6	6	18	19
7	1	7	3	4	7	7	X	7	7	10	7	7	13	14	15	7	7	18	19
8	1	8	3	8	5	6	8	X	8	10	8	12	13	8	15	8	8	18	19
9	9	9	3	9	6	7	9	X	10	11	12	13	14	9	9	17	18	19	19
10	10	10	3	10	10	6	7	10	10	X	11	12	13	10	10	10	17	18	10
11	1	11	3	4	5	6	11	8	11	10	X	12	13	14	15	11	17	18	19
12	12	12	3	4	5	6	7	8	9	10	11	X	13	12	12	16	17	18	19
13	1	13	13	13	5	6	13	8	13	13	11	13	X	14	13	16	17	13	19
14	14	14	14	4	5	6	14	8	14	10	11	14	13	X	16	16	17	18	19
15	15	15	3	15	5	6	7	8	9	10	11	15	13	14	X	16	17	18	15
16	1	16	3	4	5	6	16	16	16	10	11	16	13	14	15	X	16	18	19
17	1	17	3	4	5	6	7	8	17	17	17	17	13	14	15	16	X	18	17
18	18	18	3	4	18	6	18	18	9	18	11	18	13	18	15	18	18	X	18
19	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	19	X

By representing the judgment "better than" by the symbol $>$ we can express briefly each subject's order of preference according to the total number of votes for each resolution, as follows:

Bridges, $1 > 19 > 2 > 13 > 6 > 3 > 8 > 11 = 7 > 4 = 5 > 9 > 12 >$
 $10 > 14 > 16 > 15 > 18 > 17$
 Burt, $19 > 14 > 5 > 8 > 9 = 2 > 1 > 3 > 13 > 7 > 4 > 11 > 16 =$
 $15 > 12 > 17 > 6 = 10 > 18$
 Cutting, $19 > 13 > 2 > 15 > 1 > 3 > 9 > 5 > 4 = 7 > 6 > 12 > 17 >$
 $11 > 14 > 18 > 16 > 10 > 8$
 Givler, $18 > 17 > 16 > 10 > 14 > 15 > 12 > 13 > 11 > 7 > 9 > 5 > 2$
 $= 3 > 4 > 8 > 6 > 1 > 19$
 Kellogg, $13 > 8 > 19 > 14 > 3 > 2 > 11 > 5 > 4 > 9 > 1 > 6 = 7 >$
 $10 > 12 = 15 = 16 > 17 > 18$
 Dr. Langfeld, $18 > 13 > 15 > 1 > 7 = 4 > 10 = 9 = 5 = 3 > 12 >$
 $11 > 17 > 6 > 16 = 2 > 14 > 8 > 19$
 Marston, $19 > 2 > 13 > 1 > 8 > 3 > 7 > 5 > 6 > 14 > 4 > 9 > 15 > 10$
 $> 11 > 16 > 17 > 12 > 18$
 Miss Martin, $19 > 2 > 1 = 13 > 3 = 4 = 6 > 8 = 10 > 7 = 11 >$
 $14 > 9 > 18 > 15 > 5 = 17 > 12 > 16$
 Roback, $6 > 18 > 13 > 3 > 5 = 10 > 7 = 8 > 14 > 15 = 16 = 4 >$
 $17 = 11 > 19 > 1 > 12 > 9 > 2$

Now, if a subject is perfectly consistent in his judgments, we may expect that the preference of one resolution over another, as indicated by the total number of votes for each, will not be contradicted when the two resolutions are directly compared. Thus perfect consistency in the case of the first subject, Bridges, would mean that he will always prefer number 1 to any other resolution; 19 to any other resolution but 1; and 2 to any other but 1 and 19; also that 11, when directly compared with 7, ought to be judged equal to it, and so on. The method of computing the amount of deviation from perfect consistency may be seen by the calculation of Bridges' consistency for resolution number 4. This, according to the order of preferences by total votes, ought to have been judged $< 1, 19, 2, 13, 6, 3, 8, 11, 7;$ equal to 5; and $> 9, 12, 10, 14, 16, 15, 18, 7.$ It is compared

The above average represents the subject's consistency only for the number 4. This, when averaged with eighteen other averages, gives his general coefficient of consistency. The final coefficients, obtained in this manner, were as follows:

Givler92
Burt88
Marston88
Bridges87
Kellogg86
Miss Martin84
Dr. Langfeld78
Cutting76
Roback71

The classification of preferences according to their consonant and dissonant character shows an extreme preference for the dissonances on the part of Givler; Dr. Langfeld also shows a fairly distinct tendency of the same sort. The rest of the subjects prefer the consonances in varying degrees. The extremely "consonant" subjects are Marston, Miss Martin, Bridges, Kellogg; the other three—Burt, Cutting and Roback—have the same general preference, but less marked. Ranking the subjects from the most "dissonant" to the most "consonant," we obtain the following order:

Givler
 Dr. Langfeld
 Roback
 Cutting
 Burt
 Kellogg
 Bridges
 Miss Martin
 Marston.

FINAL EXPERIMENTS

The next problem was to find a method of recording any change in the degree of consonance of a given interval for the subject without suggesting to him the purpose of the experiment. Obviously little can be accomplished by merely playing a passage of intervals to him for five, ten or fifteen minutes, and then asking him to make a judgment whether the consonance of the interval has changed during that time. Not only is it impossible to eliminate a certain amount of suggestion in such a method, but it is hopeless to expect anything that approaches reliability in figures based on comparing pairs of impressions, one of which is pretty well obliterated at the time the judgment is made. We may find a clue to a better method by turning again for a moment to a consideration of the historical development of consonance. Musicians have always agreed¹¹⁸ that the practical criterion of fully established consonance is the suitability of an interval in the final chord of a cadence. Now all the intervals that have so far met this requirement have fulfilled it only after having become the most acceptable intervals in parallel passages—parallel octaves among the Greeks, parallel fifths and fourths in the middle ages, parallel thirds in more modern times. The increase in acceptability as a parallel interval has been such an invariable feature of developing consonance that we may safely assume for our present purposes that the amount of decrease in dissonance may be measured in terms of this increase. But by the same token, further increase in fusion, once the interval has become consonant, will be marked by the decrease in its acceptability in consecutive parallels. That is to say, the interval which is just consonant is always at the height of its effectiveness for consecutive use. In becoming more consonant it becomes less available as a parallel interval.

We shall assume then as the measure of decreased dissonance the increase in the parallel value of an interval; and, as a measure of the increased fusion or synthesis of an interval already consonant, the decrease in parallel value. This is by no means an identification of consonance with the pleasantness of an

¹¹⁸ *Parson*

interval, for we have to do here not with isolated intervals, but with consecutive parallels.

As before stated, the experiment is concerned directly with four intervals,—two consonances, the fifth and third; and two dissonances, the major and minor seventh. The problem is to find out whether the prolongation and repetition of these intervals produces anything which may be interpreted as a change in their degree of consonance; such a change, we have seen, can best be indicated by the amount of change in the acceptability of this interval as a parallel. How to determine the latter amount is our next question. The general method was that of paired comparison, with each judgment graded by the subject according to the degree of his preference. I began each hour of experiment by making out a table of graded comparisons of the four parallel intervals in question,—to which was added another, the minor ninth, in order to give a wider range of comparison. This table, which served as the standard for the day, was made up as follows: each interval, played consecutively in a passage of seven parallels, with $c' d' e' f' e' d' c'$ as the fundamentals, was compared with each other one played similarly. The subject was asked, in each case, not only to state his preference between the two passages, but also to grade the strength of his preference according to a scale A, B, C, D, E. A was to mean extremely strong preference, E extremely weak, while B, C, and D were intermediate grades. An example of the standard table follows:

		3	5	7	VII	9	Miss Martin, Dec. 1, 1913
Standard	3	×	3C	3B	3B	3A	
	5	3C	×	5B	5B	5A	
	7	3B	5A	×	7C	7D	
	VII	3B	5B	7D	×	VIIE	
	9	3A	5A	7C	VIIE	×	

In all these tables, 3 means a passage of thirds; 5, one of fifths; 7, one of minor sevenths; VII, one of major sevenths; 9, one of minor ninths. The numbers to the left of the horizontal

columns indicate the interval given first in presenting a pair for comparison; those at the top of the vertical columns indicate the second interval in the pair. Thus parallel thirds, coming first, were in this table preferred to fifths by C, to minor sevenths by B, to major sevenths and to minor ninths by A. Parallel fifths, coming first, were inferior to thirds by C, better than minor sevenths by B, better than major sevenths by B, and better than minor ninths by A.

The interval under consideration for this particular day was the minor seventh; the method was that of prolongation. After having recorded the standard table, I proceeded to play a passage of parallel sevenths, sustaining each interval exactly one minute. I did this for five minutes, playing thus five intervals of a minor seventh. Immediately after this I asked for new judgments of preference, in which the comparison was made only between the minor sevenths and the other parallels. The results of these judgments, at the end of five minutes, appear in the "first five minute" table below.

	3	5	7	VII	9
Standard 3	×	3C	3B	3B	3A
5	3C	×	5B	5B	5A
7	3B	5A	×	7C	7D
VII	3B	5B	7D	×	VIIE
9	3A	5A	7C	VIIE	×

TABLE TWO
(after five minutes)

	3	5	7	VII	9
3	×		3B		
5		×	5B		
7	3A	5C	×	7C	7B
VII			7D	×	×
			7B		×

TABLE THREE
(after second five minutes)

	3	5	7	VII	9
3	×		3D		
5		×	7E		
7	3B	5C	×	7B	7A
VII			7B	×	×
9			7B		

When we compare the judgments of the sevenths in the standard table with those after five minutes of prolonging the interval, we find that it has lost one point to the thirds, has gained two from the fifth and three from the minor ninth. This gives it an increase in value of $+4$ as compared with the standing of the seventh before prolongation. Repeating the same process for another five minutes, I obtained the results in Table Three. Here the minor seventh has gained two points on the third, six points on the fifth, three on the major seventh, and four on the minor ninth. Its value in this table is accordingly $+15$ as compared with that of the standard table.

This process was repeated for a third and a fourth period of five minutes, sometimes for a fifth, but by this time the subjects expressed such general discomfort that it seemed hardly likely that the results could continue to have the same significance, especially as the hour was always nearly up at the end of the fourth trial. The use of a fifth period was therefore abandoned. The only other change in method which took place during the course of the experiments was in regard to the length of the prolongations. During the first few weeks of the experiments I used ten-minute periods, made up of five two-minute prolongations, and obtained the same type of results as later with the five-minute period, but the subjects indicated a far greater satisfaction in their attitude toward the five-minute length, and it was accordingly adhered to regularly after the first few weeks.

The method of repetition differed from that of the experiment

in that each period, three minutes in this case, was occupied not with sustaining intervals, but with playing repeatedly an entire melody in parallel thirds, fifths, minor or major sevenths, as the case might be. The melody used was, with the exception of one note, within the compass of a sixth, so that the same notes recurred with great frequency. Three minutes sufficed for four repetitions of the entire melody. The first phrase of the melody, eight notes, was used in making the comparison for the tables. The amount of positive or negative change was determined, as before, by counting the difference between A and B one point, between A and C two points, and so on. The difference between an E preference in favor of an interval and one against it was also counted one point.¹

The general tables which follow give for each of the four intervals the average amount of change from the standard at the end of each successive period. In the few cases where the periods were longer than five minutes, the result was recorded in the "second five-minute" table. Thus in the case of the first subject, Bridges, the only prolongations of minor sevenths were in ten-minute periods. The figures in each of the five-minute spaces represent an average of from one to four experiments.

I. The subjects were instructed to limit their introspection to the graded expressions of likes and dislikes, unless something of unusual interest occurred. The danger of accompanying paired comparisons by abundant general introspection is shown very clearly in the experiments of T. H. Pear on the major and minor triad.²⁴ Of his five subjects, the three who introspected least were by far the most self-consistent. His most introspective subject had a coefficient of consistency of 27.5 on a scale where zero was perfect consistency, whereas the coefficient of the least introspective subject was 3.64. It seems almost inevitable that the subject, in attempting to give an account of how he arrives at his judgment, leads himself to judgments that would normally not be made.

²⁴British Journal of Psych., 1911, p. 57.

TABLE X
Method of Prolongation

Subject	Interval	First 5 Minutes	Second 5 Minutes	Third 5 Minutes	Fourth 5 Minutes
B Consistency .87 Dissonance Rank 7	Thirds	- 7	- 2	- 4	-10
	Fifths	- 4	+ 4	+ 9	+ 6
	Minor sevenths		+14		+ 8
	Major sevenths	- 1	+ 3	+ 2	+ 3
T Consistency .88 Dissonance Rank 5	Thirds	+ 1	+ .5	+ .5	- 2
	Fifths	- .5	- 1.3	+ 1	0
	Minor sevenths	+ 5	+ 5.7	+ 3	+ 5
	Major sevenths	+ 3	+ 6.7	+ 5.5	+ 7.5
C Consistency .76 Dissonance Rank 4	Thirds	+ 3	+ 2	+ 2	+ 3
	Fifths	+ 5	+ 3	- 1	0
	Minor sevenths		+ 3		+ 5
	Major sevenths	+ 5	+ 1	+ 4	+ 6
G Consistency .92 Dissonance Rank 1	Thirds	-11	-19.7	-15	-15
	Fifths	+ 2	+ 6	+ 4	+ 2.5
	Minor sevenths	+ 4	+ 9.3	+ 7	+22.2
	Major sevenths	+ 8	+15.5	+ 8	+19.8
K Consistency .86 Dissonance Rank 6	Thirds	- 4	- 4.9	- 3.2	- 3.8
	Fifths	- 2	- 3.1	- 1.3	- .5
	Minor sevenths	- .8	- 2.3	+ 1.5	+ 3
	Major sevenths	- 3.2	- 1.5	- 1.8	- 3.2
L Consistency .78 Dissonance Rank 2	Thirds	- 4	- 5	- 6	- 6
	Fifths	- 4	- 2	- 4	- 8
	Minor sevenths		+ 4		+ 4
	Major sevenths	+ 2	+ 3	0	0
M Consistency .84 Dissonance Rank 8	Thirds	- 5	- 9	- 9	-12
	Fifths	- 1	-12	- 5	+ 4
	Minor sevenths	+ 4	+15	+10	+ 3
	Major sevenths	+ 2	+ 4	+ 5	+ 3
S Consistency .88 Dissonance Rank 9	Thirds	+ .5	+ .3	- 1.5	- 2.5
	Fifths	+ 1	+ .5	+ 1.5	+ 1.5
	Minor sevenths	+ 6	+ 2.3	+10.5	+ 7.5
	Major sevenths	+ 7	+ 4.5	- 1	+ 3.5
R Consistency .71 Dissonance Rank 3	Thirds	- 6	- 9.3	- 7	- 5
	Fifths	+ 2	+ 3	- 1	+ 2
	Minor sevenths	+ 2.5	- 1.5	0	+ .5
	Major sevenths	+ 4	+ 4	+ 7	+ 7

TABLE XI

Method of Repetition

Subject	Interval	First 3 Minutes	Second 3 Minutes	Third 3 Minutes	Fourth 3 Minutes	Fifth 3 Minutes
B Consistency .87 Dissonance Rank 7	Thirds	- 3	-13	- 3	- 6	- 6
	Fifths	- 6	- 9	-12.5	- 9.5	- 7
	Minor sevenths	+12	+10	+ 9	+10	+12
	Major sevenths	+ 4	+ 5	+ 5	+ 7	+ 4.5
T Consistency .88 Dissonance Rank 5	Thirds	0	+ 1	+ 2	+ 3	+ 4
	Fifths	+ 1	+ 1	0	0	- 4
	Minor sevenths	+ 4	+ 4	+10	+10	+10
	Major sevenths	0	+ 5	+ 9	+10	+ 8
C Consistency .76 Dissonance Rank 4	Thirds	0	- 3	- 2	- 2	- 1
	Fifths	- 6	- 8	- 5	- 5	- 3
	Minor sevenths	0	+ 3	+ 1	+ 7	+ 6
	Major sevenths	+ 2	+ 2	+ 1	+ 3	+ 7
G Consistency .92 Dissonance Rank 1	Thirds	- 7	- 9	-14	-14	-17
	Fifths	+ 1	+ 1	- 5	0	0
	Minor sevenths	+12	+ 6	+ 2	0	0
	Major sevenths	- 2	0	0	+ 2	+ 5
K Consistency .86 Dissonance Rank 6	Thirds	+ 5	0	+ 1	- 2	- 1.5
	Fifths	+1.5	+ 1	+ 2	+ .5	+ 1
	Minor sevenths	+ 2	- 6	+ 5	+ 4	- 5
	Major sevenths	- 4.5	- 5	- 6	- 6	- 5
L Consistency .78 Dissonance Rank 2	Thirds	0	- 1	- 2	- 2	- 1
	Fifths	- 2	+ 6	+ 1	+ 1	- 1
	Minor sevenths	+10	+ 7	+ 5	+ 2	+ 5
	Major sevenths	+ 5	+ 6	+ 5	+ 3	+ 6
M Consistency .86 Dissonance Rank 8	Thirds	-16	-17	-18	- 4	-14
	Fifths	- 3	- 2	- 2	- 1	- 1
	Minor sevenths	+ 7	+13	+15	+12	+ 9
	Major sevenths	+ 5	+ 1	+ 1	+ 4	+13

TABLE XII
SUMMARY OF AVERAGES
Method of Prolongation

Interval	First 5 Min	M.V.	Second 5 Min.	M.V.	Third 5 Min.	M.V.	Fourth 5 Min.	M.V.
Thirds	— 2.9	3.6	— 6.2	5.3	— 4.0	3.9	— 6.6	4.4
Fifths	— .3	2.4	— .5	3.9	+ .4	4.2	+ .9	2.8
Minor sevenths	+ 3.0	1.9	+ 4.0	4.7	+ 5.0	3.8	+ 8.5	5.3
Major sevenths	+ 2.1	2.8	+ 4.3	2.9	+ 2.5	3.1	+ 7.5	5.0

TABLE XIII

Method of Repetition

Interval	First 3 Min.	M.V.	Second 3 Min.	M.V.	Third 3 Min.	M.V.	Fourth 3 Min.	M.V.	Fifth 3 Min.	M.V.
Thirds	— 3.0	5.7	— 6.0	6.0	— 5.1	6.2	— 3.9	3.7	— 5.2	6.1
Fifths	— 1.9	2.6	— 1.4	4.2	— 3.0	4.1	— 2.0	3.0	— 2.5	2.2
Minor sevenths	+ 6.7	4.0	+ 5.3	4.2	+ 6.7	3.9	+ 6.4	3.8	+ 6.8	3.4
Major sevenths	+ 1.4	3.2	+ 2.0	2.7	+ 2.1	3.6	+ 3.3	3.2	+ 5.3	3.5

The above results are represented graphically in the tables which follow. The last of these tables, number XXII, is made from the statistics of C. W. Valentine¹¹⁵ concerning the ontogenetic development of interval preferences. Reference is made to it later in the discussion.

INTERPRETATION OF RESULTS

It will be seen at a glance that whether we use the method of prolongation or of repetition, certain very characteristic tendencies appear for each of the four intervals. The third loses rapidly, the minor seventh gains equally rapidly, the fifth maintains a fairly constant level, the major seventh rises in value, but less rapidly than the minor seventh. It cannot be said that these changes are merely cases of a general adaptation which levels the distinction of pleasant and unpleasant. If this were true we should expect the greatest positive values in the case of the major seventh parallels, which are distinctly more disagreeable than those of the minor seventh. But the minor

¹¹⁵ C. W. Valentine, *Internal Preferences in Elementary School Children*, *Brit. Jour. of Psych.*, Oct. 1912, 100 ff.

seventh actually has an average rise of 5.8 points as against 3.4 for the major seventh. Moreover, the lowest individual average value of the major sevenths,—3.1, was obtained in the case of a very “consonant” subject, Kellogg, who expressed the strongest distaste for them at the outset. And, further, the highest average positive value for the same interval, + 10.0, was obtained from the most “dissonant” subject, Givler, whose persistent bias in that direction we saw in the preliminary experiments on resolutions. Any argument to the effect that the processes which we have been describing merely tend to reduce affective differences is set at naught by the fact that there were actually 132 cases in which the minor seventh, after being judged inferior to the fifths or thirds in the standard table, was judged better in the prolongation or repetition tables. Thus Givler, after four five-minute prolongations of the minor seventh, preferred such parallels to any others; and remarked that to him they then seemed less like dissonances than like very full interesting consonances. His is, of course, an extreme case, but extreme only in exhibiting more of the same tendencies which others manifest to a less degree.

Clearly the particular form of adaptation manifested here is most readily accounted for by the interpretation that a dissonance, when prolonged, becomes more suited for consecutive use; that is to say, more perfectly synthesized, in short, more nearly consonant. And since the region of highest affective value for an interval is the barely consonant region, we may expect the most rapid increase of value in that dissonance which is nearest this region; namely, the minor seventh. We actually find this to be the case almost without exception. We further expect to find the most rapid decline in the case of prolonged thirds, since they have reached their maximum value on becoming just consonant. This also is obviously indicated in the results. The fifth, being a more established consonance than the third, and therefore nearer its limit of synthesis, we should expect to show a lower initial value than the third, but a correspondingly greater stability in resisting the effects of repeated hearing. This is borne out by the results. When prolonged, the fifth changes

almost not at all; when repeated, it declines, but much more slowly than the third. If this experiment had been made on a ninth century subject, we might have expected a very rapid decline of the fifth and rise of the third.

The fifths give in one or two cases certain curious results which call for especial consideration. The highest values regularly assigned to them in the standard tables were given by Givler and Kellogg, both violinists. Kellogg ranked them very nearly equal to the thirds at the outset, and gave a slight positive change after repetitions, a slight negative change after prolongations. There was nothing unusual in this except in the high initial value; but Givler always preferred the fifths to the thirds at the outset, and liked them still better after prolongation! His average prolongation value for fifths was $+ 3.4$. When asked to introspect his attitude toward them, he stated that to him they seemed to have more masculine quality than the thirds; also that he felt less jar of movement in a passage of fifths than in one of thirds. Further, the fifth seemed so unified and self-sufficient that he did not think of it as empty.

This suggests very strongly the existence of a second stage in the development of an interval after its consonance has been well established, a stage in which it is apprehended by the hearer less as an interval and more as a reinforced unison. In this new capacity it is capable of taking on a new lease of interest. Further experiments will be necessary in order to determine the existence of such a secondary transition from empty interval to full unison. For the present the only evidence at hand is the comparison of the fifths with the octave. Statistics by C. W. Valentine^{115*} on 62 men and 84 women indicate that in immediate pleasantness the octave is ranked by the men second among all musical intervals, preceded only by the major third; women rank it fourth, preceded only by the thirds and major sixth. In both cases it has a distinctly better value than the fifth, although it is distinctly the more consonant of the two. The stage of consonance which lies between the fifth and the octave thus appears to be marked at some point by an increase in affective

^{115a} Op. cit.

value. A fairly adequate idea of the changes which take place in the affective value of the octave and fifth between childhood and maturity may be obtained by examining in Table XXVII the graphic representation of some of Valentine's statistics. He obtained two widely different groups of results, one from elementary school children, and the other from girls in a preparatory school. His object was to grade the intensity of likes and dislikes for all the musical intervals at the different ages between six and fourteen. The judgments given by his elementary school children are too chaotic to offer any sort of conclusive evidence. These children of the ages of six and seven "liked" all the intervals, "liked" them all very much the same, but the major seventh was preferred to the major sixth! Either the real facts were obscured by the inability of the subjects to conform to the simple requirements made of them, or else their likings were for mere noise as such. The second group, the girls in the preparatory school, were apparently in every way more alert than the first. Valentine says of them,¹¹⁶ "These children took a very keen interest in the tests, and most of them, even the very youngest, seemed very decided in their judgments." It seems therefore probable that the results given by these girls between the ages of nine and fourteen will represent to a certain degree the affective changes that take place during that period. His results for these years, combined with the statistics on adults, are represented graphically in the table. They show a general rise of the minor and major seventh, and a general decline of the major third from the age of nine to maturity. The fifths, with the exception of a marked fluctuation at the age of twelve, shows a slow and continuous increase up to maturity. The octave shows a marked rise between the ages of nine and thirteen, followed by a decline from then until maturity. These results offer in general a significant parallel to my prolongation tables, and in addition point to the existence of the particular kind of development in the fifth and octave which we have found suggested by my results in one subject. That is, the increase of the value of the octave in children, and of that of the fifth in my violinist

subject, both alike indicate that when the unity of the developed consonance has reached a certain completeness, its effect may become enhanced by its increased dynamic value. The octave, which doubles the melody, and the fifths, used in organ mixtures, have acquired a new kind of interest in their capacity of reinforcing the fundamentals. But let the octave or fifth be apprehended as an adhesion of two separate tones, as in the consecutive octaves and fifths of four part harmony, and the result is a most disagreeable barrenness.

CHAPTER VI

CONCLUSION

The facts obtained in the foregoing experiment, as well as those of musical history, favor the assumption that the degree of consonance increases as a result of the frequency with which an interval is heard. Moreover, each interval, in its development from dissonance to a high degree of consonance, undergoes a characteristic affective development corresponding to the different stages in its course. There is a continuous rise in affective value as we pass from the extreme to the slight dissonance; the bare consonance possesses the highest pleasantness of all intervals, but increasing consonance means for it a marked affective decline, until we reach the point where the interval gives the effect of a true unison, intensified dynamically; at this point follows a second affective rise, but by no means so great as that which marks the transition from dissonance to consonance. It goes without saying that the inherent affective quality of an interval may be greatly changed by the presence of such contributory factors as enter into any system of harmonic relations but this fact does not alter the fundamental fact that each consonance degree has its characteristic affective value.

The phenomenon of consonance, if the theory here advanced is right, is a special case of the adjustment of the inner to the outer relations. The nervous system, by a form of activity that tends with each repetition to become more simple and economical, gradually affects the synthesis of more complex physical relations. The affective aspect of the various stages of this process becomes more explicable when we consider that consciousness is most vividly present where new adjustments are being made. Where a synthesis has been accomplished so often as to be affected with ease, as in the case of the fifth, the output of consciousness is low as compared with that involved in synthesizing the third, where the accomplishment is relatively difficult. Now according

to Professor Fite,¹¹⁷ whose view I accept, the whole question of pleasure-pain is to be referred to the same conflicts which occasion consciousness. To quote from him: "Not only is conflict a condition of consciousness, but it is especially a condition of pleasure-pain. . . . Pleasure is succeeding, pain is failing in the process of resolving a conflict."¹¹⁸ Using as an illustration a game of tennis, or of chess, he says: "All the feeling lies between two points—between that at which your opponent becomes strong enough to offer appreciable resistance and that at which he becomes so strong that any effort on your part is futile. If your opponent is too weak, struggle is unnecessary; if he is too strong, struggle is useless."¹¹⁹ In another passage he says to the same effect,—“There is certainly a more intense pleasure in removing a stubborn obstacle than a pliant one. Pleasure depends not merely upon the prosperity of our undertakings, but upon the size of them, as estimated by the difficulties presented.”¹²⁰ This last sentence presents, I think, exactly the point of view which we must carry to the consideration of the varying pleasurability of various intervals. It is thus that we can understand why different periods of history, different peoples of the same period of history, and the same individual at different periods of his own development can give such different accounts of the same interval. We may say that the fifth was the most pleasurable of all intervals for the ninth century hearer because, in the conflict which his synthesizing mind waged against the complexity of physical relations, he was just able to succeed with the 2:3 ratio. The third, 4:5, was slightly too difficult, but not so much as to discourage further hearing, especially if it could be immediately followed by a fifth. With a further advance, due to repeated hearing, the fifth becomes a relatively uninteresting affair, whereas the third, which can just be unified, acquires a tremendous vogue. Hence the amazing fondness for thirds at the time of Monteverde, hence the high value of the third for the young

¹¹⁷ W. Fite, *The Place of Pleasure and Pain in the Functional Psychology*. Psych. Rev. X, 1903, 633-644.

¹¹⁸ Op. cit., 638.

¹¹⁹ Op. cit., 637.

¹²⁰ Op. cit., 638.

child of to-day, hence the rapid decline of the third when heard with great frequency by the adult to-day. The minor seventh, we may suppose, was almost discouragingly complex for the hearer in the tenth century,—as for Valentine's nine-year-old girls who gave it a value of -44 as compared with $+52$ for the major third. But with more and more frequent hearing it has been attacked with nearer success. Its rapid gain in my experiments is most clearly understood if we consider the $5:9$ ratio as a tonal adjustment not quite completely successful. That it will later rank among the consonances we can scarcely doubt, if past successes are any index to the future. When it does become so, it will be the most pleasurable of all intervals, since its unification will make more demands on the mind than that of any of the other consonances. Exactly the same is to be said for the major seventh. At first it is disheartening in the opposition it offers to synthesizing activity, but the change that takes place with repeated hearing is, as a rule, definitely positive. Certainly it will take thousands, perhaps hundreds of thousands, of hearings to bring it about that its impression will be distinctly less dissonant than at present, but this is no reason for assuming that it will mark any exception to the law of consonance evolution.

What limits may we expect to the type of development here set forth? This is in effect the question asked in the introduction of the discussion. An answer is suggested by the following passage from Professor Fite: "Where an equilibrium has been reached it is certain to be disturbed by the coming to consciousness of further capacities which call for further realization. . . . The conflict is brought about by the increasing demands of the life purpose as opposed to the conditions that stand in the way of its realization, and the resolution of the conflict means that the obstructions are either removed or discovered to be definitely insurmountable."¹²¹ Applying the same principle to that form of equilibrium which is experienced as consonance, we may say that there is no reason to expect that any one consonance-dissonance division will ever be ultimate. As soon as our tonal consciousness succeeds in the mastery of certain ratios, it tends

necessarily to pass on to those that are a little more complex. The reduction of old consonances to emptiness, the establishment of new dissonances as consonances, this seems to be the typical form of a development to which it is impossible to set any limit. To be sure, there are certain tonal combinations which, to our present consciousness, mean only insurmountable difficulty. For example, if all the notes on a piano be struck at once, the effect is not even one of unpleasantness; it is musically insignificant. The case is comparable to that described by Professor Fite in which the resistance to be overcome is so strong that any effort is futile. This is an altogether different kind of experience from that of the harsh dissonances, such as the major seventh, for in experiencing the latter the hearer pits himself against them; his mind tries to synthesize them; he fails, and experiences excruciating displeasure. But as these extremely harsh intervals become gradually more acceptable, we must suppose that more and more of what now passes for chaotic jumble will become significant,—always painfully significant at first; and the direction of this progress will be from the more simple to the more complex mathematical ratios.

There remains the question whether the practical limits set by a particular system of harmony may not cut short an otherwise limitless progress. Stumpf touches on this question, in his article entitled "Konsonanz und Konkordanz."¹²² He maintains here, as elsewhere, that the difference between a consonant and dissonant interval is a difference only of degree, and he admits, on different grounds from those which I have set forth, the possibility of an indefinite increase in the number of intervals that will be included among the consonances.¹²³ He accordingly attempts to explain the specific opposition of consonant and dissonant combinations, not as belonging to the common fundamentals of music, but only to the developed products of the harmonic epoch. The distinction of consonance and dissonance, which he limits in application to intervals, is gradual and susceptible to continuous change; but the distinction of concord and dis-

¹²² *Zeitschrift für Psychologie*, Vol. 58.

¹²³ *Op. cit.*, 341

cord, which he applies to chords formed according to a particular structure principle, is one that will continue unaffected by the gradually enlarged scope of consonant intervals. Thus,—“Konsonanz und Dissonanz sind nur graduell verschieden, und es kann der Grenzstrich an verschiedenen Punkten der Riehe gelegt werden. Dagegen Konkordanz und Diskordanz sind spezifisch verschieden, und es giebt keinen Ubergang und keine Zwischenstufe, sondern nur ein Entweder-Oder.”¹²⁴

The structure principle according to which our present system of concord has developed is, says Stumpf, that of selecting as a fundamental chord basis the greatest possible number of consonant intervals within the octave, taking them progressively according to their grades of consonance, from the stronger to the weaker degrees. Thus in the octave *c-c'* we proceed from *c* first to the most consonant interval, the fifth, *g*; then to the third, *e* or *e^b*. We thus obtain as fundamental elements the triads *c-e-g*, and *c-e^b-g*. By repeating the same process with the two notes most consonant with *c*, namely *g* and *f*, we obtain the additional triads, *g-b-d*, and *f-a-c*. With these we have the material for the whole of our present scale. Secondary triads are now formed on every note on the scale, and thus furnish the chromatic elements with which the concordant triads can be rationally altered. A concord is a pure triad form; that is, a tonal construction according to the principle of the maximal number of tones within an octave that can be consonant with the fundamental and with each other, when the consonances are arranged in order from the stronger to the weaker. A discord is a rationally altered concord; that is, a tonal construction in which a triad, by the substitution or addition of chromatic elements, thus progresses more necessarily and fluently to another related triad.

The above scheme, which is worked out with great consistency by its author, offers a solution to many theoretical difficulties, especially to the question why a consonant interval may sound either concordant or discordant according to its musical context. It is, in fact, the only attempt up to the present which adequately explains the extent to which our pleasure and displeasure can

¹²⁴ Op. cit., p. 14.

emancipate themselves from the merely sensory factors in musical combinations. But is not clear that even this well grounded opposition of concord and discord is one whose foundation elements will not undergo a further evolution. It is arbitrary to assume that the maximal number of consonances available for the construction of concords will always be the particular number at present in use. Stumpf is at considerable pains to justify his exclusion of the natural seventh, 4:7, from his original interval elements. He gives two arguments for building his system on the triad 4:5:6, rather than on a chord of four notes, 4:5:6:7. First, the chord 4:5:6:7, when played with the third a minor, would give a noticeable dissonance, 20:24:30:35, since the fifth 24:35 varies too widely from the pure fifth 24:36. Second, the modern scale, formed from triads of the more perfect consonances, has already established as its seventh the dissonant 5:9. The intrusion of the 4:7 would only cause disagreeable confusion of the 5:9 and 4:7; and, since the 5:9 already has the right of way, the 4:7 is rejected altogether.

But what, we may ask, is to be expected when the 5:9 also becomes a consonance? The argument from the necessary dualism of major and minor will no longer hold good, since with the 5:9 as seventh the minor chord $c-e^b-g-b^b$ offers even less difficulty than the major. In fact, the same chord, $a-c-e-g$, is treated almost as a concord by Leoncavallo in "Pagliacci." Likewise the argument from the confusion of sevenths will cease to obtain, since the new consonance in question will be an element already contained in the scale. If our system of harmony is to continue to be as rational as Stumpf makes it out to be at present, then the advent of the 5:9 as a universally accepted consonance will necessitate the reconstruction of the whole system of harmony on the basis of a chord of four instead of the triad 4:5:6.

We must conclude, then, that the present opposition of concord and discord is alterable for the same reasons as those that obtain for the opposition of consonance and dissonance. The only difference is that the latter rests on isolated sensory experience whereas the former is an antagonism which has been organized into a complete system. It will, therefore, offer a tremendous

resistance to anything that makes for upheaval. But, if the point of view that has been maintained here is a sound one, we must expect that our whole system of harmony will gradually come to reflect whatever changes occur in the consonance degrees of its constituent intervals; and this latter type of change, we have seen, is one to which no limit can be set.

TABLE XIV
 SUBJECT B
 CONSISTENCY .87
 DISCORDANCE RANK 7

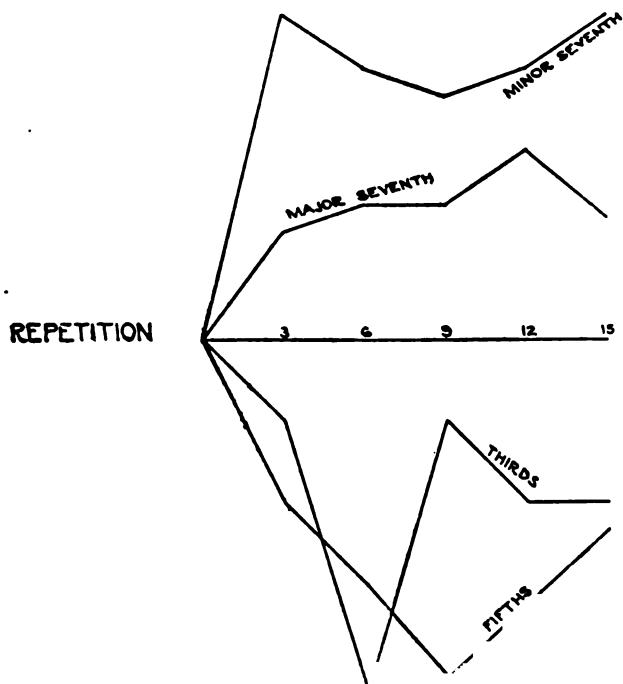
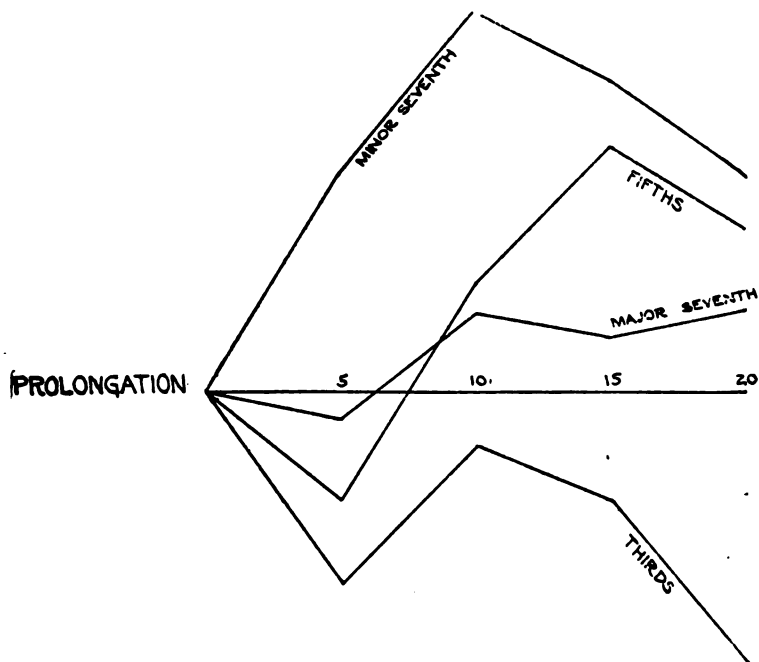


TABLE XV
 SUBJECT T
 CONSISTENCY .88
 DISSONANCE RANK 5

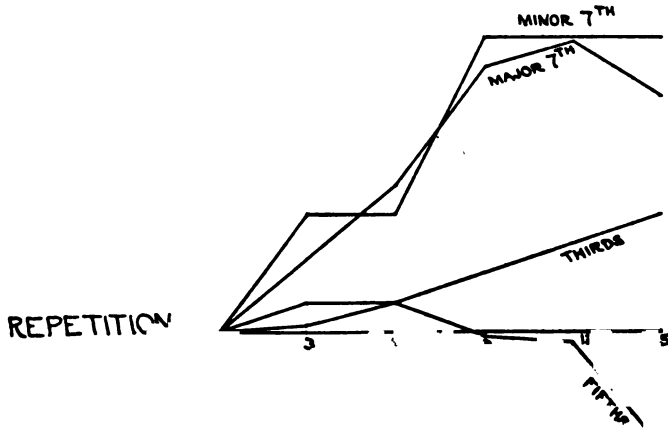
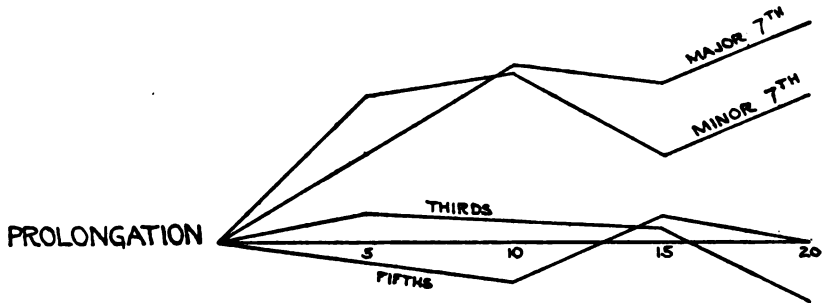


TABLE XVI
 SUBJECT C
 CONSISTENCY .76
 DISSONANCE RANK 4

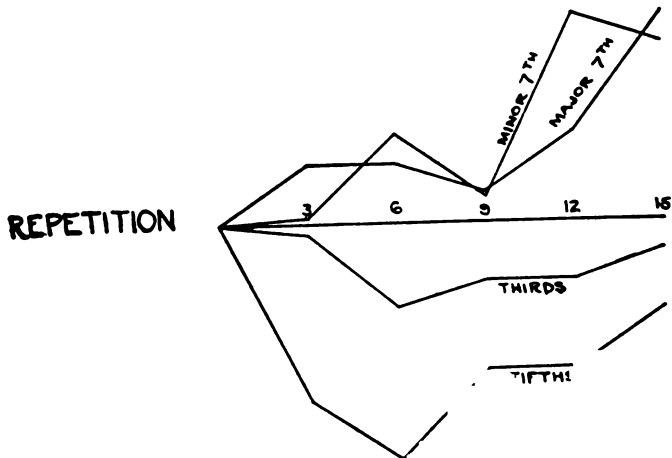
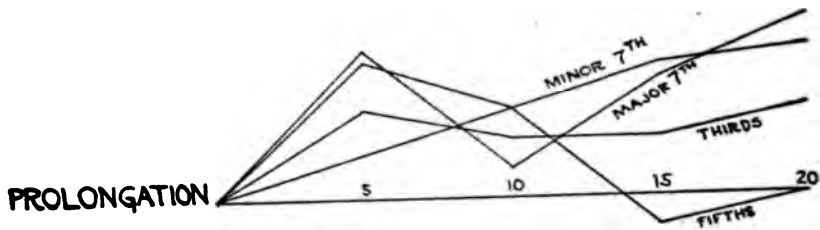


TABLE XV
 SUBJECT T
 CONSISTENCY .88
 DISSONANCE RANK 5

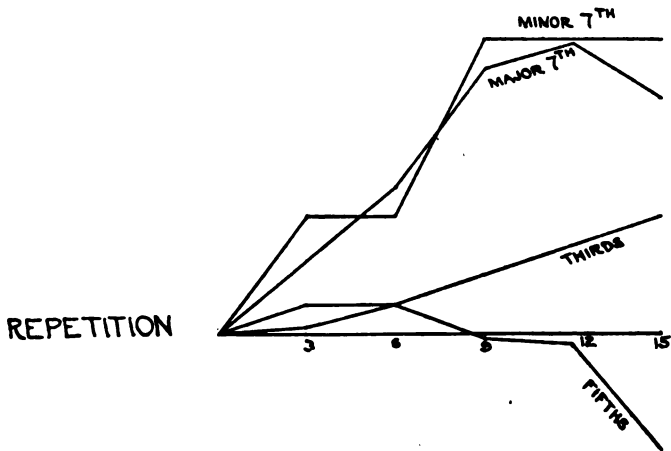
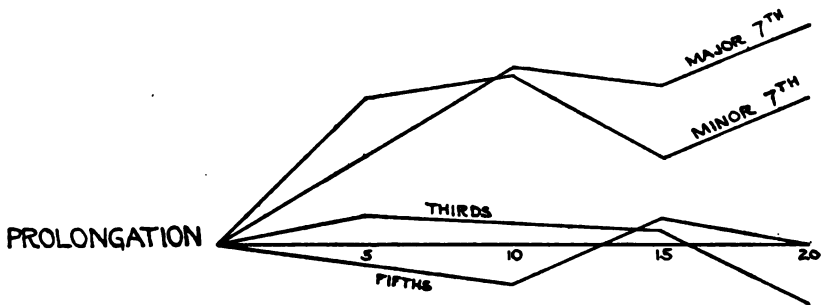


TABLE XVI
 SUBJECT C
 CONSISTENCY .76
 DISSONANCE RANK 4

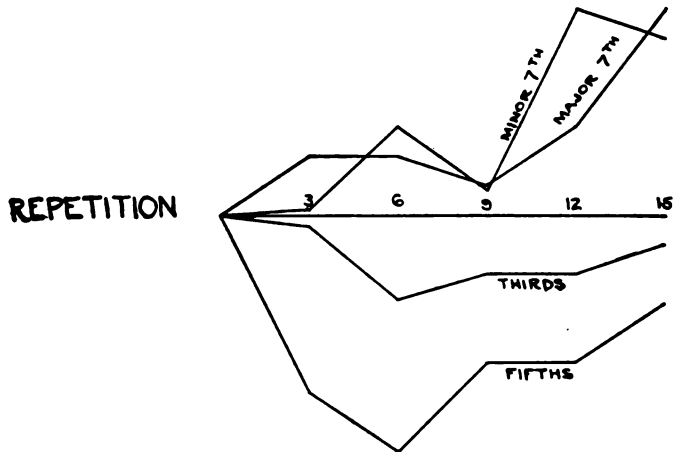
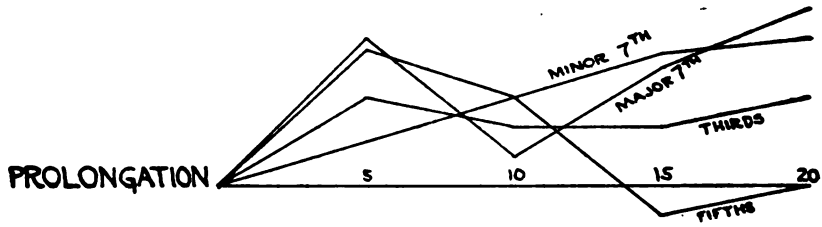


TABLE XVII
SUBJECT G
CONSISTENCY .92
DISSONANCE RANK 1

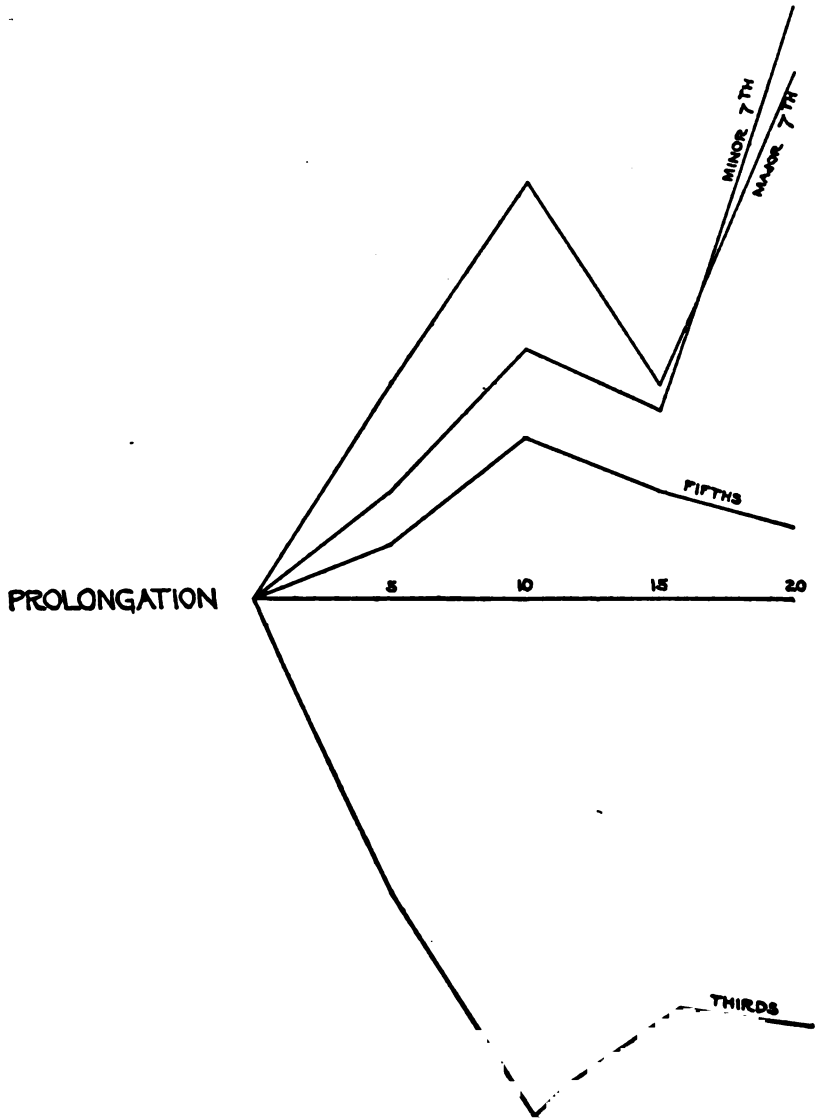


TABLE XVIII
 SUBJECT G
 CONSISTENCY .92
 DISSONANCE RANK 1

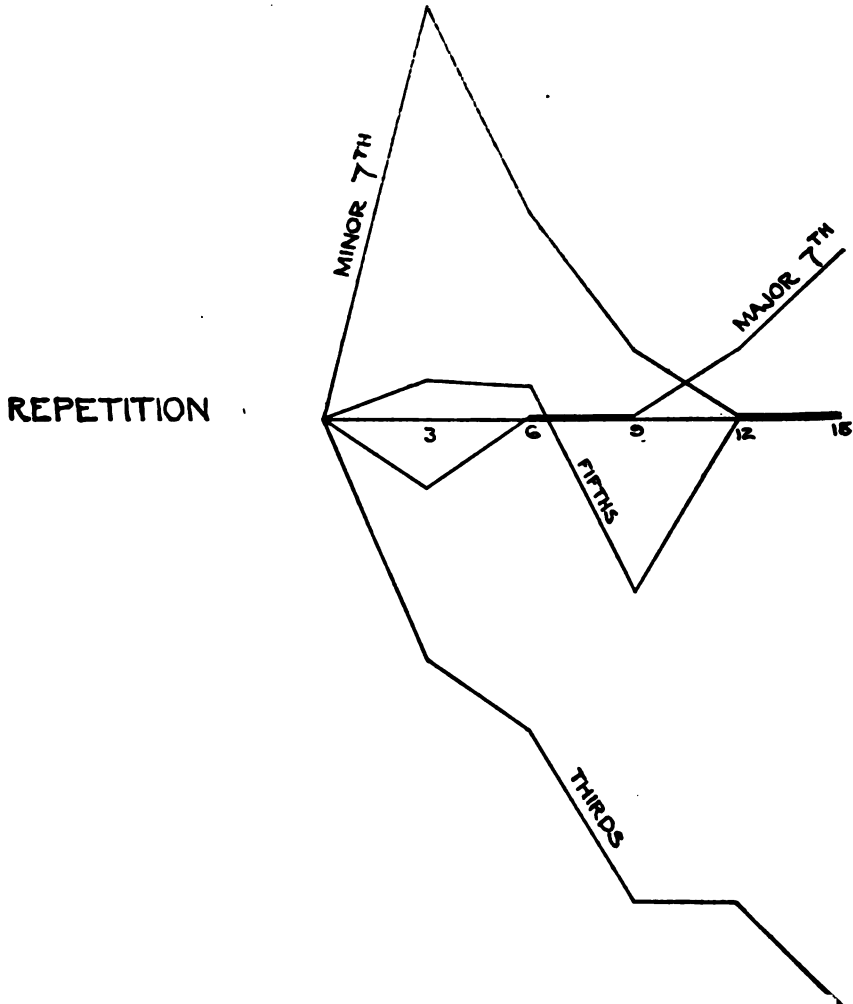


TABLE XIX
 SUBJECT K
 CONSISTENCY .86
 DISSONANCE RANK 6

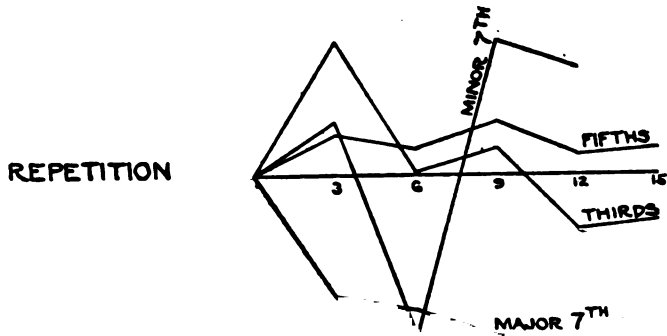
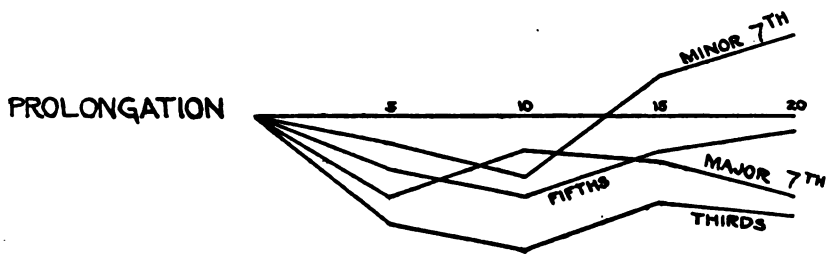
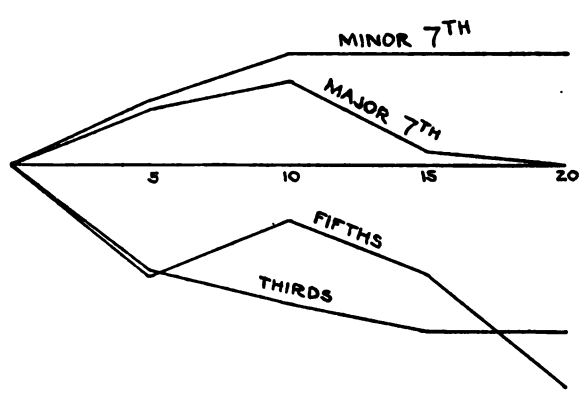


TABLE XX
 SUBJECT L
 CONSISTENCY .78
 DISSONANCE RANK 2

PROLONGATION



REPETITION

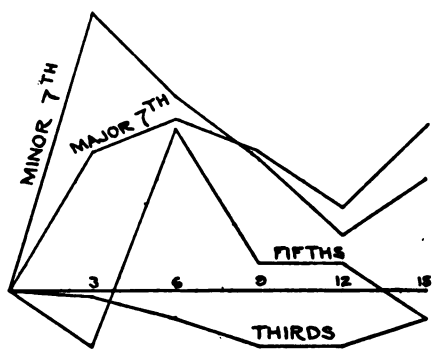


TABLE XXI
SUBJECT M
CONSISTENCY .84
DISSONANCE RANK 8

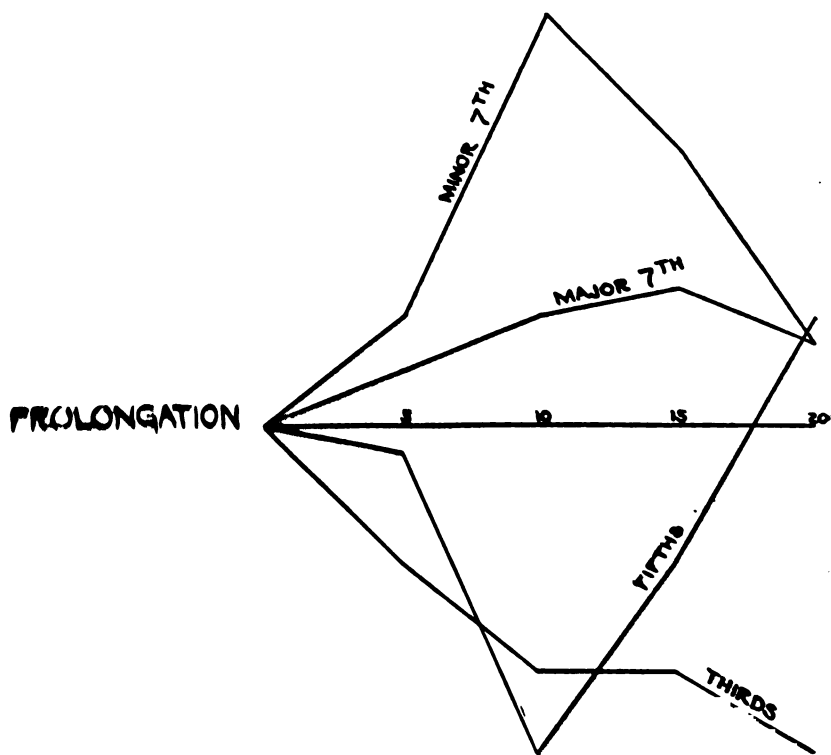


TABLE XXII
 SUBJECT M
 CONSISTENCY .84
 DISSONANCE RANK 8

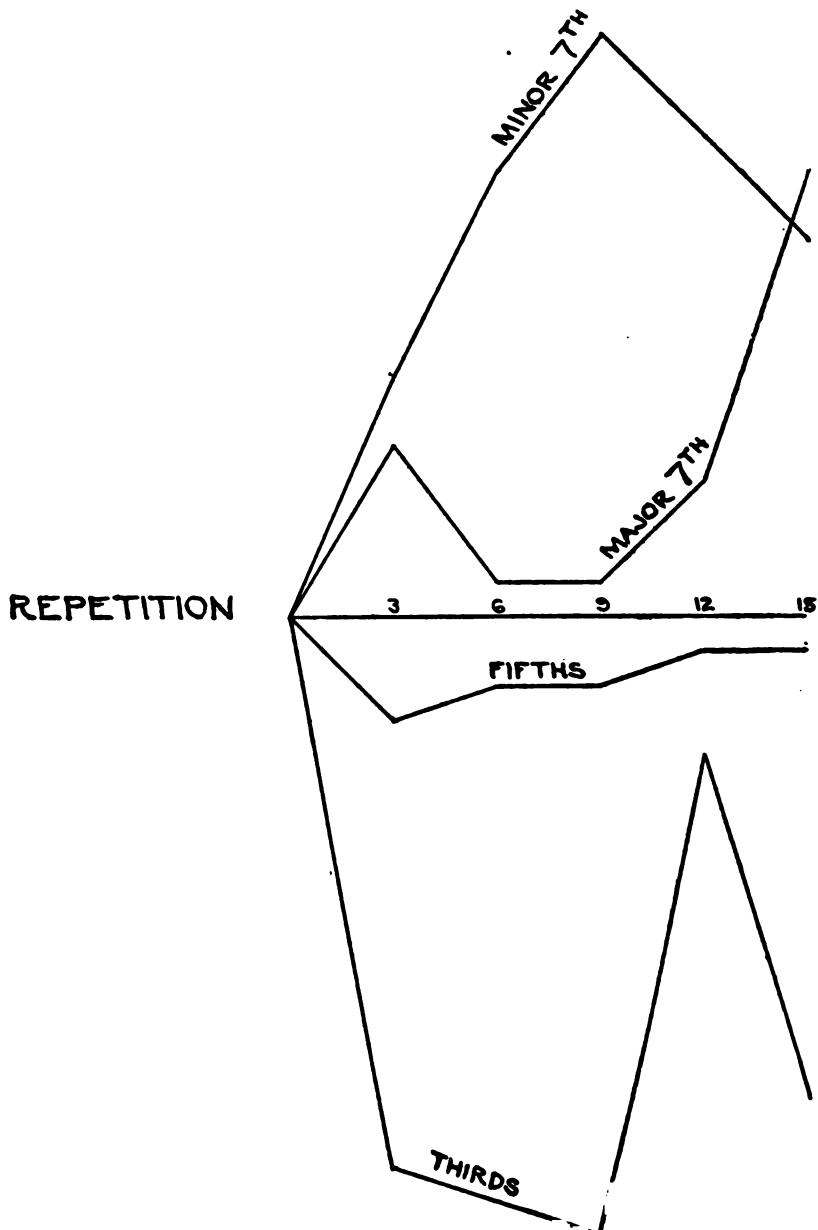


TABLE XXI
SUBJECT M
CONSISTENCY .84
DISSONANCE RANK 8

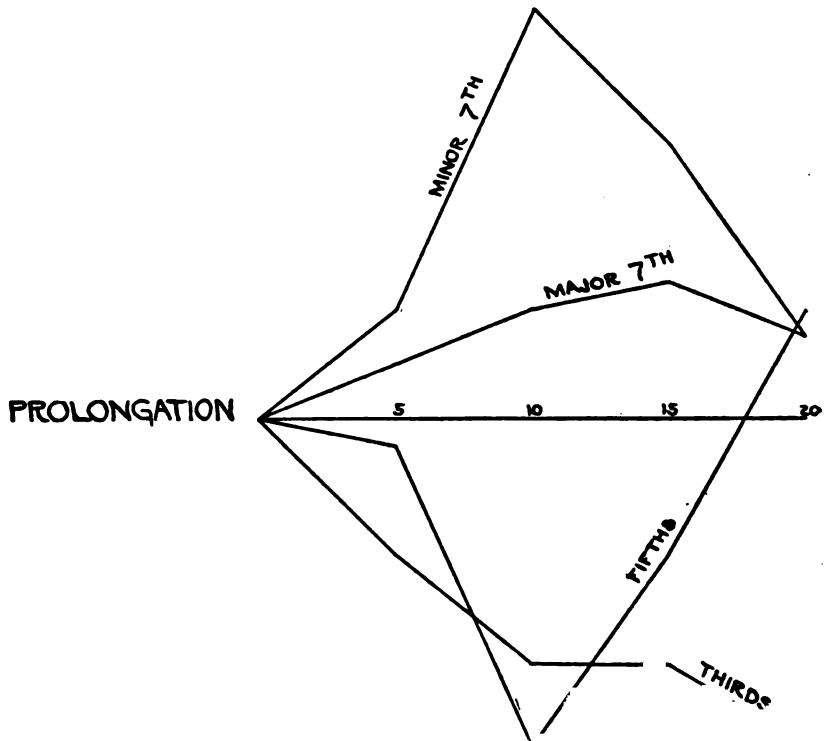


TABLE XXII
SUBJECT M
CONSISTENCY .84
DISSONANCE RANK 8

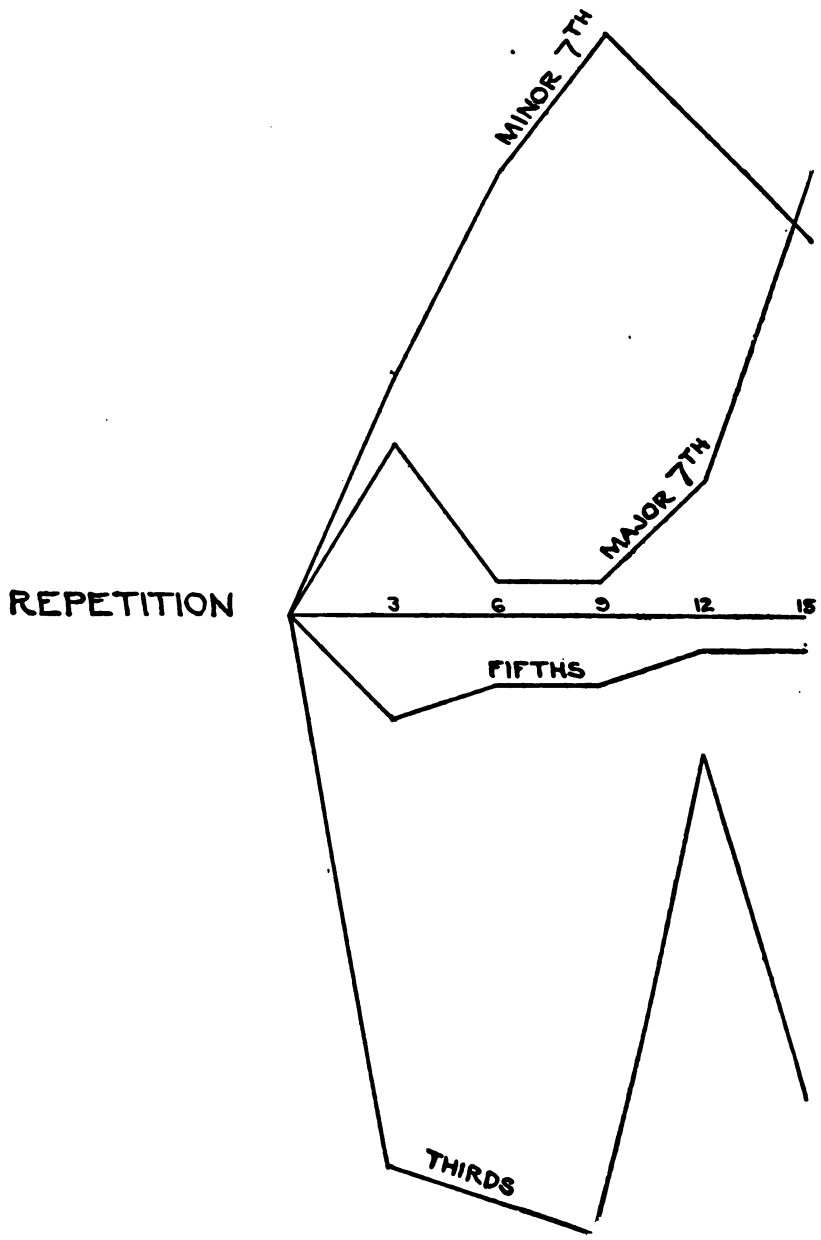


TABLE XXIII
 SUBJECT S
 CONSISTENCY .88
 DISSONANCE RANK 9

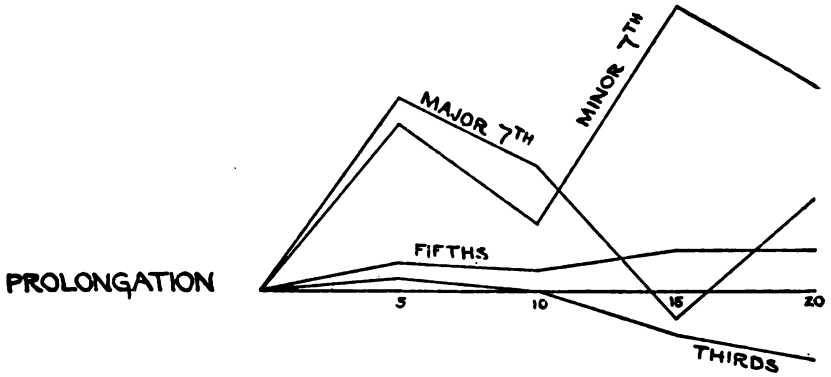


TABLE XXIV
 SUBJECT R
 CONSISTENCY .71
 DISSONANCE RANK 3

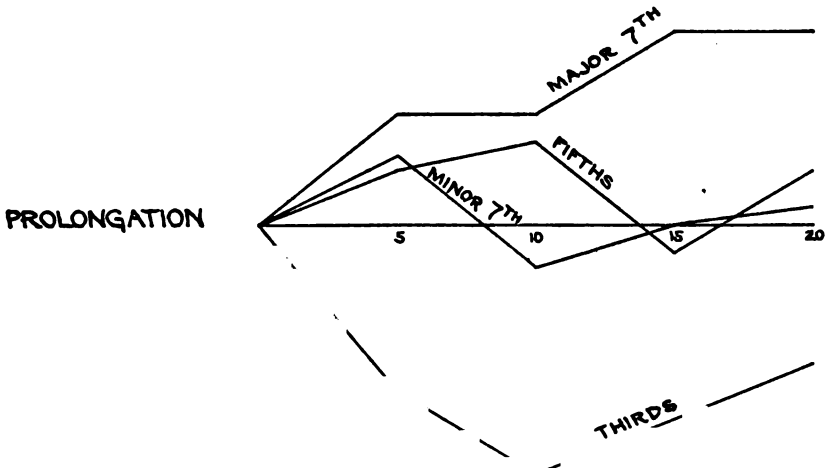
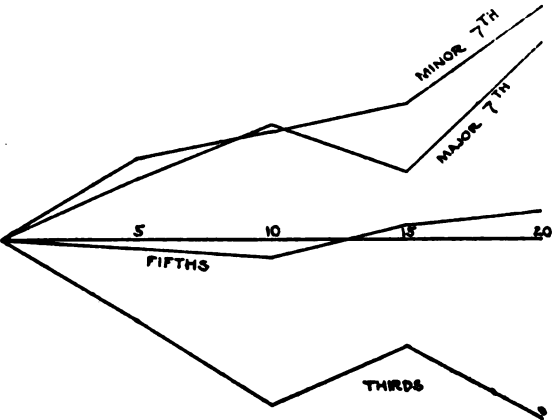


TABLE XXV
AVERAGE OF ALL SUBJECTS

PROLONGATION



REPETITION

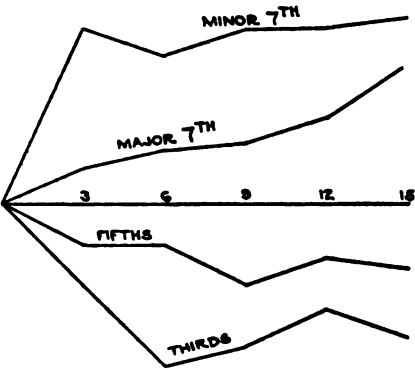


TABLE XXIII
 SUBJECT S
 CONSISTENCY .88
 DISSONANCE RANK 9

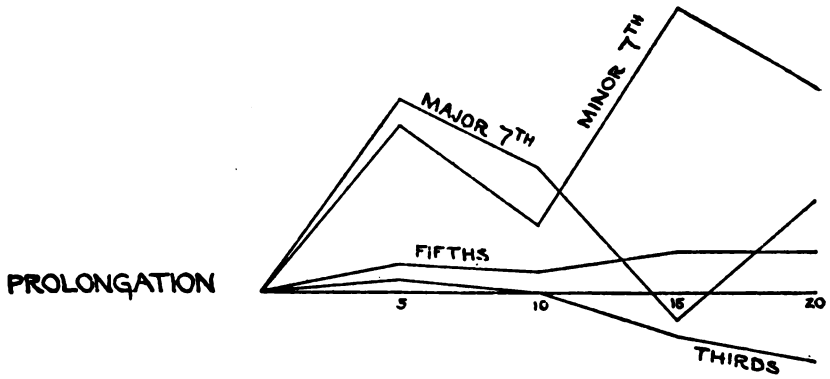


TABLE XXIV
 SUBJECT R
 CONSISTENCY .71
 DISSONANCE RANK 3

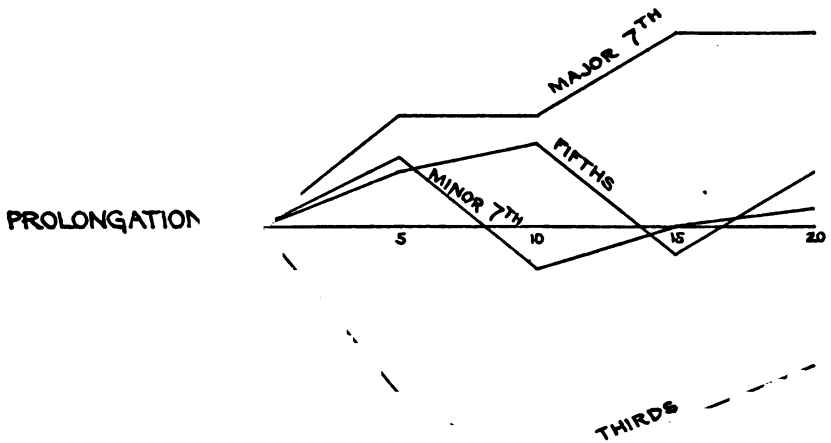
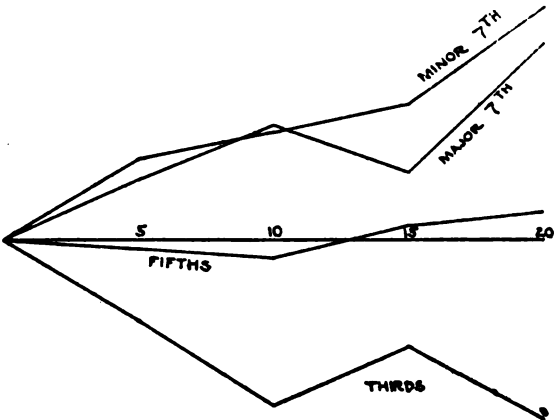


TABLE XXV
AVERAGE OF ALL SUBJECTS

PROLONGATION



REPETITION

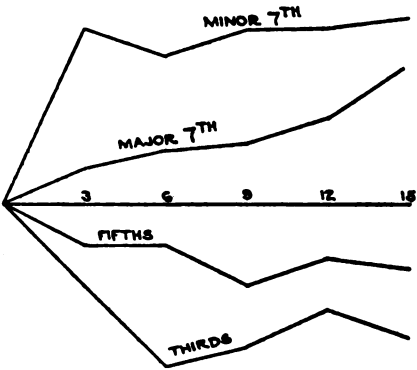


TABLE XXVI
AVERAGE OF SEVEN MOST CONSISTENT
SUBJECTS

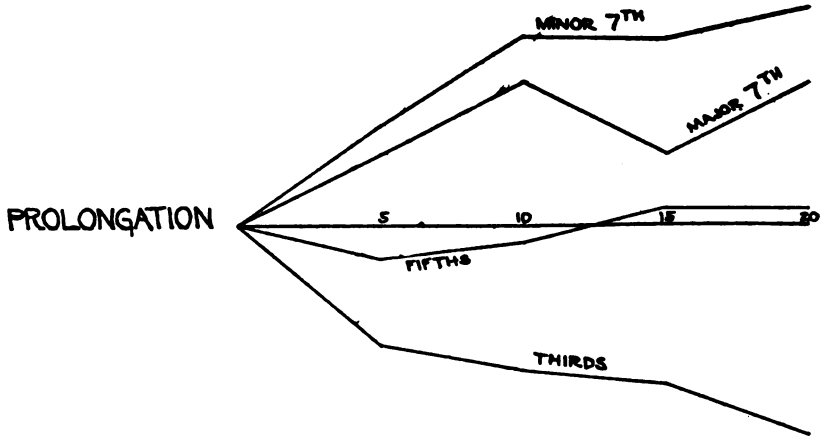
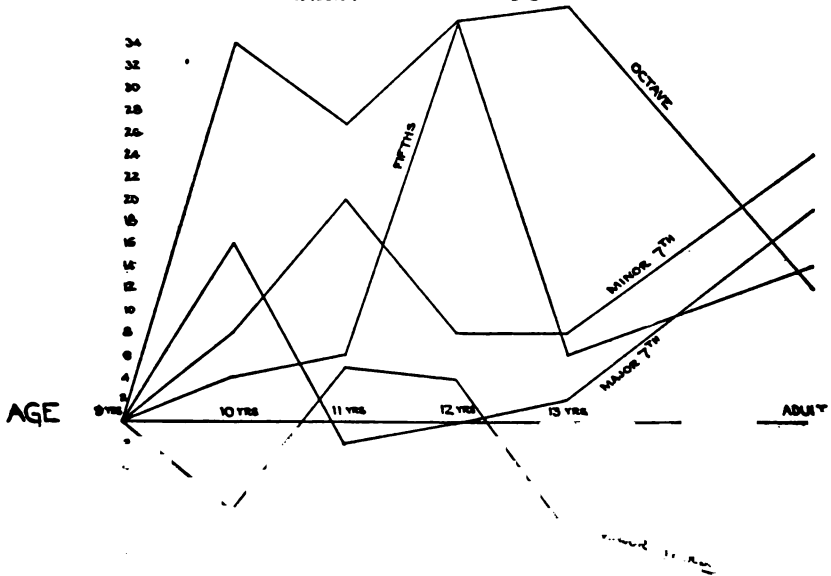


TABLE XXVII
ONTOGENETIC DEVELOPMENT OF INTERVAL PREFERENCES
FROM RESULTS OF C.W. VALENTINE, GIRLS 9-14 YRS. AND WOMEN

112

INITIAL VALUE OF OCTAVE AT AGE OF 9 = +14
 . . . FIFTH . . . = +2
 . . . THIRD . . . = +32
 . . . MINOR 7TH . . . = -44
 . . . MAJOR 7TH . . . = -58



THE EFFECT OF PSYCHO-PHYSICAL ATTITUDES ON MEMORY

BY WILLIAM D. TAIT

I. INTRODUCTION

THE investigation to be described was conducted in the Harvard Psychological Laboratory during a period of two years. Nine subjects took part in the experiments during the greater part of the first year and nine during the greater part of the second year. Five were common to both periods, and all were members of the laboratory.

This work is not quite the same as the orthodox memory experiment, but was suggested by the following facts well known to all psychologists. It is found that, due to some sudden shock, whether emotional or physical, one or both of the following may happen.

1. The most recent events just preceding the shock may be lost, *or*
2. Those events immediately *after* the shock may be lost.

Both of these may occur in any instance or only one. The latter is usually the more common and even where both occur this loss is more extensive.

Time was when psychologists considered the abnormal individual as something totally different from the normal type, but to-day matters have changed, and there is a growing belief and conviction that there are no strong demarcation lines, and that, in general, the abnormal case only shows very exaggerated conditions of what is found in the normal individual. On account of the absence or comparative absence of this exaggeration we say that the normal person is better balanced. Sanity and insanity are then relative terms. The whole question is one of proportion, which is decided by fitness to meet environment.

More particularly, then, an attempt will here be made to see how far the memory of the normal individual is influenced by shocks; to determine the influence of pleasant

2 *The Effect of Psycho-physical Attitudes on Memory*

and unpleasant feelings on memory; to see how different kinds of ideas crowd one another out; to find out some of the factors which have an influence on memory after impressions have been received, and to discover if possible the significance of this after period. Further, if it be found that to all appearances some impressions are forgotten on account of some particular filling of the after-period, can some or all of them be recovered?

This outlines the general field. Needless to remark, no complete answer is given, but it may open up new ground in a somewhat neglected field.

Some years ago the aim of the experimental psychologist was to find out general laws, and little stress was given to individual differences, but now there is a growing tendency to see in these seemingly small differences powerful factors in deciding the make-up of the individual. In what follows there are some things which are common to all, and some strictly individual, and of these individual differences some are and some are not explained.

II. METHOD AND MATERIAL

For the most part words were the material used. They were arranged in lists of twenty. In some cases the words forming a list were associated with one another and in other cases they were detached or isolated. Some lists were made up of pleasant or unpleasant words respectively, and in some experiments colors were used. The method consisted of a combination of reproduction and recognition, except in the case of colors, where recognition alone was employed.

These lists of twenty words were read to the subject in an even tone and with great regularity. (Timed to the tick of a stop-watch.) This required practice and hence for a considerable period, at first, no results were counted. The subject was instructed to reproduce the words remembered at a given signal, and the time was taken with a stop-watch from the giving of the signal until the subject uttered the first word. As mentioned above, the filling of this period after the reading of the words and the giving of the signal

...

was varied as will be described under the head of each experiment. The receptive attitude of the subject was also varied in the different experiments — sometimes it was passive, and at other times he was told to remember in an active way. The conditions of the experiment also varied the receptive attitude in an objective way.

III. EXPERIMENTAL

The first study to be taken up is the influence of the feelings of pleasantness and unpleasantness on memory. This is not altogether new, but it has an important bearing on other experiments, and the subject was approached in a somewhat different way, not altogether agreeing with the work of some other experimenters. This problem was investigated in three different ways.

1. Lists of words used to test memory were either pleasant or unpleasant.

2. Neutral lists with the after-period filled in by something pleasant or unpleasant.

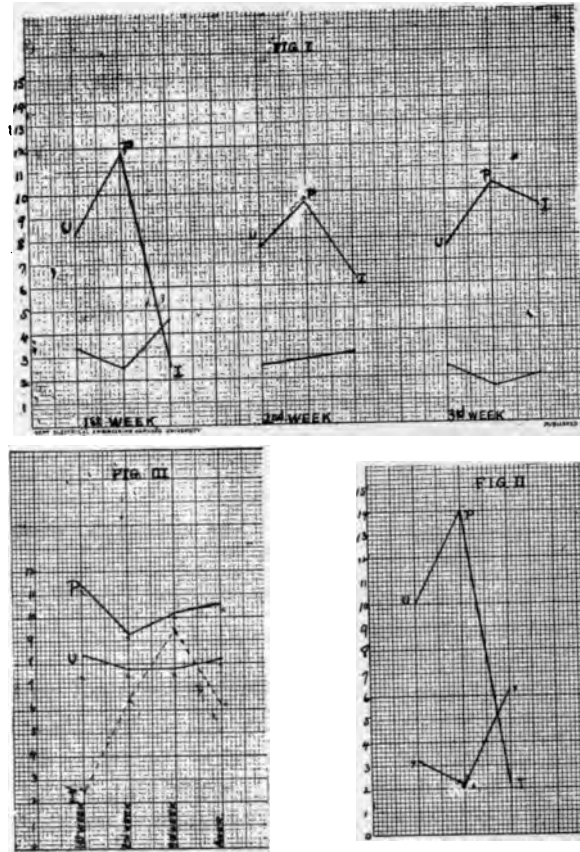
3. Recognition of colors.

- (1) Lists of twenty words each were read to the subjects, and three kinds or classes of lists were used. In one, the words composing the list were pleasant, optimistic, cheerful; in a second, unpleasant, pessimistic, cruel; while in the third the words were as devoid of feeling tone as possible. In none of the lists were the words purposely associated. The experiment in this connection covered a period of three weeks, and eleven subjects took part; they were instructed not to use any artificial means of remembering the words or to form any associations, but simply to take the words as they came, let them drop in and see how many remained.

Fig. I shows the results in graphic form. The ordinate shows the number of words remembered out of twenty. U=unpleasant list, P=pleasant list, I=indifferent list.

The lower lines represent the time from the giving of the signal until the subject uttered the first word. Each point—U, P, I—represents the average number of words remembered by eleven subjects. Curves for each of the three weeks are given. Fig. II is an individual record

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chosen more or less at random. Fig. III gives the average results grouped.

The obvious conclusion is that the pleasant words are remembered better than the unpleasant or indifferent. The last week is an apparent exception because the supposed indifferent list was not indifferent for over half the subjects. It is also worthy of note that the unpleasant words are remembered better than the indifferent. It will also be seen that the time is shorter in cases where there are more words remembered, that is, the time is shortest in the case of the pleasant words and longest in the case of the indifferent. This is in agreement with the results of Bigham, *viz.*, that

the quicker the memory, the better it is, or the better it is, the quicker it is. One limitation must be made to that statement. We must always distinguish between immediate and retentive memory. Immediate memory is better, the quicker it is, but this is by no means necessarily true of retentive memory.

In only one subject were the unpleasant words remembered better than the pleasant ones, and as this is not regarded as an exception it will be discussed later.

The introspection is in substantial agreement with the objective results.

Introspection on pleasant lists. "Words isolated and disconnected and yet they were easy to remember."

"List seemed on the bright side of life and easy to remember."

"List seemed optimistic and easy to remember."

"Considerable emotion was an aid in the case of four or five words. No negative influence of emotion. Words did not inhibit one another as in the case of the unpleasant list."

"Words had an emotional tinge and easy to remember."

"No emotion."

"Some emotion connected with this list, especially the word 'hero,' which hauled other words along with it. Easier to remember than the last list (unpleasant)."

"More emotion connected with this list than with the other, and remembered more easily."

"Ill-defined feeling of satisfaction. This was a decided aid."

Introspection on unpleasant lists. "The words which are remembered stand out prominently and inhibit others."

"Decided emotional coloring."

"Vivid emotion, words seemed to crowd one another out."

"Not conscious of any emotion."

"No emotion. Words remembered stood out in relief and inhibited others. This was very noticeable."

"No emotion. Words remembered stood out clearer than the other words, so clear that they seemed to inhibit other words."

"Some emotion."

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"Words remembered stood out plainly and seemed to inhibit others. The word poverty is an example."

It will be noted that many of the subjects, and those the most observant, speak of the inhibitory effect of the unpleasant words. There may be three reasons for this. First, it may be due to a strife between the expressions aroused by the unpleasant words and a more or less pleasurable attitude in the subjects themselves previous to hearing the list. Secondly, it may be due to a strife between the expressions aroused by the words individually. Expressions of unpleasantness are not so well unified as pleasant ones; the emotions connected with them and the accompanying expressions or instincts are more diverse than in the case of pleasant emotions. The latter is the more likely. Thirdly, unconscious suppression of the unpleasant.

(2) In the experiment described the lists were intrinsically pleasant, unpleasant or indifferent. In the following, the lists are all as indifferent as possible, and the words forming such lists were not associated in any obvious way. The receiving mood of the subjects was as far as possible the same. The influence of pleasantness or unpleasantness was introduced after the lists were read to the subjects. After one list something pleasant, optimistic and cheerful was read to the subject; after another something unpleasant and depressing was read.

The results here are not so evident, due to the fact that the reading itself was a distraction and that the associations thus involuntarily introduced would be as various as the different subjects. The work on this aspect of the question covered a period of three weeks with eleven subjects. The results may be summed up as follows:

	AVERAGE % OF WORDS REMEMBERED
When list was followed by pleasant ideas	21
When list was followed by unpleasant ideas	15

There is a difference of six per cent in favor of the pleasant words, which is in accordance with the results of the preceding experiment.

An individual example of how one unpleasant idea may

inhibit others and yet remain itself is well evidenced in the case of one subject specially well trained and very acute in the way of introspection. A list which was apparently neutral for all the other subjects contained the word "fagot." This immediately brought up vivid recollections of tortures contained in Lecky's *History of European Rationalism*. In this instance every word with the exception of "fagot" was obliterated. Evidently, the feeling and emotional tone connected with the word in question was very strong, and possessing a strong attitude, inhibited all others with different expressions or attitudes. Put in another way it would be that the complex — for no idea is alone — to which "fagot" belonged was stronger than the other complexes, strength being defined in terms of emotion and feeling, which accompany certain motor expressions. However, this is anticipating.

Introspection. "Had list excellently in mind before hearing about the torture. Its influence is destructive."

"Tortures seemed to scare one from the words."

"Reading about tortures is far more destructive than other reading."

"Felt that words remained better by the pleasant reading."

"Mind an absolute blank after the unpleasant reading, and many words never came back."

"Mind a blank after reading the unpleasant story."

The evidence so far seems to show that unpleasant ideas have a tendency to be suppressed and have a suppressing influence on other ideas with which they are connected intimately or even more remotely.

(3) The third method was the recognition of colors. Here a series of fifteen colors was shown to the subject on a uniform gray background, and he was asked to pass judgment according to the following scale:

1. Very pleasant.
2. Moderately pleasant.
3. Just pleasant.
4. Indifferent.
5. Just unpleasant.

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- 6. Moderately unpleasant.
- 7. Very unpleasant.

After the judgment of the series was completed, two or three alterations were made in it by changing as many of the colors. The series thus changed was again shown to the subject in a different order and judgments again asked for. The subject was also asked after the exposure of each color if that color was in the preceding series.

In all, two hundred and forty-two tests were made, and the results can be summed up as follows:

Averaged number of pleasant colors remembered	63.4%
Average number of unpleasant colors remembered	47.2%
Average number of indifferent colors remembered	27.3%

The results would be much more marked but for the fact that one subject had the heartiest detestation for the whole work with colors, and reported every color unpleasant with but few exceptions. To some extent the same holds true in the case of two other subjects who came in the afternoon and were very often fatigued by their work of the morning. These exceptions could be explained on the theory that the tone of the existing complex is stronger than that of the incoming impressions and the same mantle is thrown over them.

It may seem strange that the unpleasant impressions are remembered better than the indifferent. A reason might be given that both the pleasant and unpleasant impressions arouse a definite attitude on the part of the organism, and are biologically important, whereas the indifferent impressions can be ignored. There are two sides to this attitude, so that it can be called a psychophysical attitude. On the physical side there is the arousal of certain instincts or impulses, or the combinations of these instincts or impulses into complexes. On the psychical side there are the emotions and complexes of emotions corresponding to the instincts and impulses. Added to this again are the feelings of pleasantness or unpleasantness. It must not be understood that the instinct and emotion are different or even running parallel, but they are one and the same fact looked at from a different point of view. It may also be said that

if any instinct is stopped or arrested in its expression the feeling of unpleasantness is apt to be aroused. Motor activity is curtailed. This may come about by the conflict of different instincts. Physiologically the matter can be resolved into the compounding of reflexes.

It might be well to say what is meant by attitude. As used here it means instinctive actions or impulses or the combination of such with their accompanying emotions. To all this is added feeling tone of pleasantness or unpleasantness in various degrees, and also the whole list of the so-called organic attitudes, which by their varied combination form a background and coloring for the whole mental life. To cover all this the term "psycho-physical attitude" is used.

The pleasant impressions are remembered better because the reactions which they arouse are in harmony with the welfare of the organism; that is, the attitude which is aroused has a tendency to be continued, and in many cases this may be only an incipient response. Unpleasant impressions are rejected for the opposite reason; that is, they arouse attitudes which do not have a tendency to be continued, except in abnormal cases. Looked at from the point of instinct, one can say that instincts which have a pleasant feeling tone have a tendency to propagate themselves, and the opposite is true of instincts which are accompanied by unpleasant feelings. Indifferent impressions are not remembered well because there is practically no attitude aroused either towards or away from.

However, there is another side to all this. In some cases unpleasant impressions may be remembered better. This may be due to the fact that the existing complex has an unpleasant feeling tone which is so strong that it decides which impressions shall be selected from those incoming. This is what happens in abnormal cases, and one can conceive that there is a pleasure in so doing. In short, the tone of the existing attitude or complex (for those who like that word better) is a deciding factor in what shall be remembered. Now this tone is to a large extent influenced by the various compounds of instincts which are aroused, that is, by compounds of reflexes, so that in the final analysis it is a matter of reaction whether we speak of attitude or instinct or complexes, or of emotions or of reflexes.

IV

As already stated, the period after the impressions are made is an important one. Some of the experiments to follow were performed with reference to this problem. Words were used as material and read to the subject as in the preceding experiments. After the list was read to the subject, one of several distractions was introduced before the signal was given for the reproduction of the words remembered.

The following table is submitted:

TABLE A

	a	b	c	d	e	f	g	h	i	j	MEAN	M. V.
8.	20	25	20	35	25	35	25	30	45	35	29.5	6.5
9.	20	25	15	20	15	30	10	35	60	30	26.0	9.8
10.	15	10	..	0	25	25	15	20	25	35	18.8	7.9
11.	20	15	..	20	30	25	25	..	15	40	23.7	5.7
12.	15	20	26	15	25	25	10	0	17.0	4.3
13.	0	15	30	15	25	..	30	19.1	5.1

A word of explanation concerning the table. The letters at the head represent the different subjects, and the numbers under each, the percentage of words remembered out of a list of twenty. The numerals to the extreme left refer to the various tests in order to be able to identify them throughout the investigation. The mean and the M. V. are self-explanatory. Except in the case of the shot, the interval after each list was one minute, filled as follows:

8. Passivity.
9. Attention directed towards recalling the words.
10. Disagreeable odor.
11. Sudden ringing of hidden bell.
12. Pistol shot.
13. Dizziness.

Ten subjects took part in this test. During the reading of the lists they were instructed to be passive and just sufficiently awake to take each individual word. Words making up the lists were not associated with one another.

It is at first noticeable that the ordinary sense stimuli have little effect, due to the fact that they do not arouse any strong attitude. The factor which seems to have played

the greatest part in this subsequent interval was some kind of concentrated mental activity, such as arithmetical calculation, etc. In nearly every case, the list of words was entirely forgotten. *That they disappear for good is another question to be considered later.* In many cases the pistol shot had an almost equal effect with the mental work, and for the same reason. It required an adjustment out of the ordinary, and hence the attitude was strong at first. Associations, etc., also play an important part in the feeling tone. However, it is not so uniform in its effects, and in many cases the effect grew less and less. Odors seemed to have little influence on most subjects, and those who were affected attributed it to some association or more commonly to the fact that there was something held near the face. Without this remark some of the numbers in the table would be misleading.

The most favorable condition seems to have been passivity, where the subject was instructed to be passive and let himself drift along. In plain figures attention does not seem to have been so good as passivity, but the difference is too small to be of great importance. It may be that the very fact of trying to recall the list during the one minute has something of the same effect as other mental work, and thus a new psycho-physical attitude is introduced. The fact that the passive state is a little better than attention seems to lend color to the view of some that a certain period after impressions are received is necessary for organization and assimilation. It may be that memory is better in one case than another because there are no conflicting attitudes. The longer the attitude aroused by the impression continues, the better the chance of memory for that impression, and of course the opposite holds true. If the attitude aroused at first is curtailed by the intervention of another or of others, then the first impression loses its hold, so to speak. This may also apply to abnormal cases.

The introspection is as follows:

"Effort of attention did not seem much of an aid."

"Odor seemed to interfere."

"Bell did not interfere."

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- “Not much effect from pistol shot.”
 “Dizziness seemed to drive some words away.”
 “Shot erased some words, nervous tonus over the whole body.”
 “Effort of attention seemed to retard memory.”
 “Bell did not bother much.”
 “Bell a little startling at first but soon lost its effect.”
 “Do not feel that attention is any aid.”
 “Odor attracted attention.”
 “Odor interfered with memory.”
 “I cannot see that attention was successful.”
 “Shot was complete interruption — felt as if my mental powers were out of commission.”
 “Bell was not much of a disturbance.”
 “Effect of attention was beneficial.”
 “Do not think that attention was beneficial.”

In the experiment tabulated in Table A the distraction or shock was applied after the list was read, but in the following it was given about the middle of the list.

TABLE B

	a	b	c	d	e	f	g	h	MEAN	M. V.
15.	20	25	20	30	30	20	25	35	25.6	4.5
16.	10	20	10	25	40	20	25	10	20.0	7.5
17.	5	5	0	20	15	5	15	5	8.7	5.9
18.	5	20	10	..	20	..	5	..	12.0	6.4
19.	15	25	25	25	25	25	23.3	2.8
20.	15	20	20	25	15	25	20.0	3.3

This table is arranged on the same principle as Table A, so no further explanations are necessary. The conditions in each case were as follows:

- List 15, Hidden bell rung in the middle of the list.
 List 16, Shot in the middle of the list.
 List 17, Puzzle *after* the list.
 List 18, Exercise *after* the list.
 List 19, Odor in the middle of the list.
 List 20, Rotation during reading of the list.

In this test as in the preceding one the subjects were

passive. A few facts in connection with the above table and Table A are to be noted.

1. The shot, although not so destructive coming in the middle of the list as when coming after it, has more effect than the other distraction. Objectively from the table it is on a par with rotation, but in this latter instance there was in some cases a difficulty in hearing the words.

Coming in the middle of impressions the shot seemed to have different effects on different individuals, which are not shown in tables. With some, the words after the shot are lost, that is, in the latter part of the list, while with others it is the words in the earlier part of the list, or before the shot, that disappear. On the whole, however, it seems that not so many are lost as when the shot comes at the end; that is, it has a retrograde effect. This can be seen by comparing Table A with Table B. To some extent, it is true, in cases where the words after the shot have disappeared, that those before it are all the more firmly fixed, or, as one subject put it, they are "frozen stiff." In other instances, when the words before the shot were lost those coming after it were reinforced.

2. It will also be noticed that the solving of a puzzle at the end of a list is more destructive in its effects than the shot was in Table A. An average of 10 words was remembered after the puzzle and 16.6 after the shot.

3. It may be added that the sudden disturbances seem in many cases to drive the subject to the list, and in the majority the words which are retained are those which come after the distraction. On the whole it seems pretty evident that distractions coming in the middle of a list are not so destructive as if coming at the end. The greatest effect, then, is retroactive.

The introspection is as follows:

"Lost the words before the bell."

"Shot brings me back to the words."

"Bell disturbed what came later."

"Shot brought me back to the list."

"Shot was a great disturbance. Seemed to be an exhilaration."

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"Bell disturbed what came after it."

"Lost some words immediately after the bell. Felt as if I came back to the list with a feeling that I had been away from it."

"Bell cut away the first part of the list."

"Effect seems to be on words preceding the shot."

"Bell broke up the first few words."

"No words remembered after the pistol shot."

V

So far the lists used were made up of detached or isolated words, that is, they were not associated with one another in any evident way. In the following experiment the list in each instance was made up of associated words, in some cases concrete, and in some cases abstract. The following is the table of results:

TABLE C

	a	b	c	f	h	i	MEAN	M. V.
21.	30	25	25	45	15	55	32.5	11.6
22.	35	25	35	30	25	55	34.1	7.5
23.	30	35	30	30	30	25	30.0	1.6
24.	20	20	25	30	25	30	25.0	3.3
25.	20	15	35	35	5	50	26.6	11.6
26.	10	20	15	30	0	15	18.0	7.6

The table below will show how the interval after the list was filled in each case, and also the character of the list.

21. Concrete associated list followed by one minute passivity.

22. Abstract associated list followed by one minute passivity.

23. Concrete associated list followed by one minute attention directed to recalling the words and holding them till the signal was given.

24. Abstract associated list with the same conditions as 23.

25. Concrete associated list followed by pistol shot.

26. Abstract associated list followed by pistol shot.

The most noticeable thing here is the effect of the shot on the concrete and abstract lists. Only in the case of one

subject are more abstract words remembered after the shot, and the subject was an inveterate Hegelian. In the instances of lists 25 and 26, where the list was followed by the pistol shot, an attempt was made to see if any words which had been lost by the effect of the shot could be recovered. This was successful on four subjects and the means employed as follows:

A metronome was set going, and the subject was instructed to give a word for each beat of the metronome and not to choose words, but give any that came up irrespective of whether they belonged to the list or not. This process was successful in the case of concrete words, and only very partially successful in the case of abstract words; in fact, no success at all. The facts may be tabulated as below. The numbers represent the per cent of the lost words which were recovered.

SUBJECT	ASSOCIATED	ASSOCIATED
	CONCRETE	ABSTRACT
a	18.7	16.6
b	17.6	0.0
c	15.3	0.0
f	<u>23.0</u>	<u>0.0</u>
Mean	14.9	3.3

This experiment of trying to recover lost words was tried on five subjects only, and the average computed on that basis. It is readily seen that the concrete associated words have the advantage, not only in ease of recovery, but also the fact that the shot was not so destructive on them in the first instance. A tentative explanation in keeping with the rest of the experiment might be, that the concrete words have more marked attitudes, that more instinctive and impulsive reactions are connected with them, and more feeling tone. This also agrees with Ribot's Law of Regression.

In Table A we saw the result of distractions coming after lists of words made up of detached words. In the experiment now to be described the effect of those distractions will be studied when coming *after* lists of associated words.

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TABLE D

	a	b	c	d	f	g	h	i	MEAN	M.V.
27.	50	15	25	35	45	35	20	30	31.8	9.2
28.	25	30	30	45	45	30	35	35	34.3	5.6
29.	60	30	45	40	50	50	..	40	45.0	7.1
30.	20	10	..	15	50	20	5	30	21.4	10.4
31.	15	35	45	45	40	30	25	55	36.2	10.0
32.	..	40	20	..	45	40	..	25	34.0	9.2
33.	0	25	25	25	20	0	10	45	18.7	11.5
34.	0	5	0	5	10	0	0	10	3.7	3.7

The following schedule will show how the periods after the various lists were filled.

List. Filling of after period.

27. Immediate reproduction. No after period.
28. Passivity for one minute.
29. Attention for one minute.
30. Shot.
31. Bell.
32. Odor.
33. Followed by one minute arithmetical work.
34. Followed by one minute arithmetical work.

This series was made up of eight lists, the first seven of which were composed of associated words, and the last one of detached words. The same conditions, as to subject and experimenter, hold as in the previous experiments. The purpose of the first six lists was to test the influence of distractions on associated material, and in the case of the last two, to see if the words which were lost could be recovered, and to ascertain the difference in this respect between associated and detached words. Eight subjects took part in the experiment. The following results may be noted.

(1) Of the sensory disturbances the shot is as before the greatest, but not so great as the mental work represented by the arithmetical calculation, as is shown by comparing list 33 with list 30.

(2) Spending the minute after the list in giving some attention towards getting back as many words as possible is more beneficial than passivity. This is directly opposed

to the results in the case of detached words as recorded in Table B. Reproduction of associated words is benefited by the direction of attention on their recall.

(3) The bell and the odor made very little difference, being practically on a par with the passive period.

(4) One of the most important things is the number of words recovered in lists 33 and 34. The former was made up of associated words, and the latter of detached words. These results can be tabulated as below.

SUBJECT	PER CENT OF WORDS RECOVERED	
	ASSOC. LIST	DETACHED LIST
a	35	0
b	6.6	0
d	13.3	0
f	12.3	0
g	35.0	0
h	5.5	0
i	9.0	0
Mean	15.2	0

This was tried on seven subjects, and a metronome was used as before, but the detail of the method was somewhat different. In the former experiment a stenographer took down the words as spoken by the subject in rhythm to the metronome, but in order to do this the metronome had to run at a comparatively slow speed, and it was found by trial that a slow speed was not so effective in bringing back lost words. Then, too, the articulation of the subjects speaking rapidly made it hard for any one to get all the words, and hence many were inevitably lost. In place of this the subject was instructed to write down words to the beat of the metronome, and not to make any selection in so doing. This was found to be a better method than that of selection. In all cases the subject first gave all the words remembered, before beginning the metronome test.

The fact that no words were recovered from the detached list is quite conclusive as the figures 15.2 to 0 show. Not even a chance word was recovered, neither in this experiment nor in the ones to follow, so that the recall of the associated words cannot be attributed to mere chance, and, in any case, the word "chance" should not be in the psychologist's

vocabulary any more than in the physicist's. One might explain it on the grounds that the associated material is grouped around one central attitude, or to look at it from another point of view it forms part of one complex so that when some members of the complex come up there is a tendency for the others to do likewise. They are already part of the mental equipment while the detached material is not, in fact organization might be defined as the grouping of material into complexes or becoming attached to some common or predominate attitude. The detached words are not remembered so well, not because they do not arouse attitudes, but because the attitudes which they do arouse are in many cases opposed, so that there is sort of a mutual inhibition. In a word, the associated material forms a complex bound together by a psychophysical attitude. This same principle applies to the question of pleasantness and unpleasantness.

The experiment next to be considered follows naturally from the preceding ones. In Table E (1) will be given the percentage of words reproduced immediately after the distraction and in Table E (2) the number of words recovered by the metronome method.

TABLE E (1)

	a	b	c	d	e	f	g	h	i	MEAN	M. V.
39.	15	25	20	10	15	25	5	0	50	18.3	10.3
40.	30	25	25	35	60	50	20	60	85	43.3	18.1
41.	0	5	15	15	0	5	15	0	15	7.7	6.4
42.	30	15	55	30	50	60	35	35	30	37.7	11.4

TABLE E (2)

	a	b	c	d	e	f	g	h	i	MEAN	M. V.
39.	0	0	0	0	0	6.6	0	0	0	0.73	1.3
40.	14.2	13.3	0	7.4	0	0	6.2	0	0	4.6	5.0
41.	0	0	0	0	0	0	0	0	0	0.0	0.0
42.	28.5	35.2	9.9	..	0	62.5	61.5	15.3	7.1	27.5	17.2

It will be seen from the tables that four lists were read. The first three of these were made up of detached words, and the last one of associated words. In all cases the attitude of the subjects was the same as in the former experiments, and the tables are on the same plan.

After list 39 the subject was told to remain passive for two minutes and then asked to translate some German. This lasted for one minute, and then the subject was asked for as many words as were remembered. The next thing was the metronome test, which gave a result of .73 or practically nil.

In list 40 the subject was told to remember the words, and the list was read four times. Then followed the German translating, and next the reproduction of words remembered, and finally the metronome test. Only a comparatively few words were recovered by the metronome method.

In list 41 there was no passive period, and the subject was given the German to translate as soon as the list was read. Here fewer words were remembered, and none were recovered by the metronome. It is interesting to compare this list with list 39, where there was a passive period after the reading of the list. In the former cases more words were remembered, that is, the distraction had less effect. It may be explained on the theory that the passive period after the list allows time for the arousal of certain attitudes, and thus helps to fix the words. The case of the learned list and the associated one can be explained on the same basis.

The last list in this series was made up of associated words, and was immediately followed by German translation, then the subject was asked for words remembered, and then the metronome test, which was quite successful, as in previous cases.

In addition to the above observation a few more general facts may be mentioned.

(1) In the case of detached impressions memory is better when there is a passive period after the reception of the impressions. This is in agreement with the previous experiments.

(2) In the case of the learned list more words are immediately remembered, but fewer are recovered.

(3) The number of words recovered by the metronome test is considerable, and bears out previous results in connection with associated material.

The influence of the passive period after impressions

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was to some extent noticed in the last experiment and in some preceding. In the following test it is the aim to find out the influence of such a period on both detached and associated material so that they can be more exactly compared in that respect. In addition, the filling of the period after the reading of the list will be looked at from the point of view of attention. To do this the subject was to spend one minute trying to recall as many words as possible and to hold those already in mind.

The following is the table.

TABLE F

	a	b	c	d	e	f	g	h	j	k	MEAN	M. V.
47.	17.5	25	42.5	25	27.5	30	35	25	25	27.528.0	4.7
48.	20	7.5	32.5	22.5	25	30	25	30	22.5	37.525.2	5.7
49.	37.5	40.0	35	40	35	45	37.5	30	27.5	42.537.0	4.1
50.	47.5	40	32.5	45	57.5	55	47.5	32.5	32.5	5044.0	7.7

The percentage of words credited to each subject is the average of two lists under each condition. The following facts may be noted.

(1) That in agreement with preceding work a passive period after detached impressions is superior to one of effort in trying to recall those impressions.

(2) That the opposite is true in the case of associated material.

(3) That the two foregoing facts are significant in relation to the recovery of words with the metronome, namely, that no words (one solitary instance excepted) were recovered in the detached lists, but only from lists which were made up of associated material.

In view of this, it may be said that a certain period is necessary following a series of impressions in order that they be the better fixed. This period gives time for initial attitudes to take place, and thus group the impressions into a complex. In the cases of the associated material this is unnecessary, for they are already grouped, and for this same reason less affected by distractions. We know also from pathological cases that the material last acquired is the first to be affected by disease, etc.

The experiment next to be considered has to do with the

effect of motor accompaniments as one of the conditions of memory. The method of procedure was as follows: The list of twenty words was read to the subject, who was instructed to be passive, and then as he recalled the words to make some movement for each word thought of. Sometimes it would be the lip and tongue movement or some movement of the arm for each word. After that the subject was given some mental arithmetic for the period of one minute. In this way, it was thought to find out the different fixating power of the various movements. The work was carried on for a period of two weeks, and the percentages for each week represent the mean of two lists under each condition, making four for two weeks under each condition. Further, during the second week the various conditions were placed in different positions in the series from what they were the first week. For example, during the first week the writing of words in the air came first in order of trial, but in the second week it came last. Ten subjects took part for the first week and eight the second.

TABLE G (1). FIRST WEEK

LIST CONDITION	MEAN	M. V.
77. Writing words in the air	20.7	5.1
80. Passive after reading of list	13.4	5.3
82. Movements of lips and tongue	12.7	4.7
84. Arm movements for each word	14.0	6.2

TABLE G (2). SECOND WEEK

LIST CONDITION	MEAN	M. V.
86. Arm movements	19.0	5.0
88. Movements of lips and tongue	16.5	8.5
90. Passivity	14.5	7.8
92. Writing words in the air	21.2	0.0

Combined results for the two weeks are as follows:

CONDITION	MEAN
Passivity after list, then arithmetic	13.9
Movements of lips and tongue, then arithmetic	14.6
Arm movements, then arithmetic	16.5
Writing words in air, then arithmetic	20.9

It will be seen that the condition conducive to the best fixation is that of writing in the air, second best is

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arm movements, then articulatory movements and passivity, in descending order. There are some individual differences which are due to some unavoidable associations by one or two subjects. As throwing some more light on the results, the introspective judgments of the subjects as to the best method are interesting. These are tabulated as follows:

CONDITION	JUDGMENTS IN FAVOR OF
Writing in the air	10
Articulation	4
Arm movements	2
Passivity	2

Practically this bears out the objective results.

In general the results of this experiment are as follows:

The writing of the words in the air is better because such is an organized attitude while the others are not so customary, and hence detrimental to some extent. Articulation was too marked, that is, more expressive than is usual, hence attracted attention to the movement itself. In addition to this the incipient articulatory movements are combined with writing in the air, so that has a double advantage.

SUMMARY OF EXPERIMENTAL CONCLUSIONS

I. Pleasant impressions are remembered better than unpleasant, and both are remembered better than indifferent ones.

II. Not only are such impressions themselves remembered better, but they seem to exert the same influence on other material. Unpleasant impressions have the opposite effect, that is, they exert a repressing influence on other impressions.

III. Ideas are more affected by other similar mental material than by sensory disturbances, unless these latter arouse strong psycho-physical attitudes, e.g., the pistol shot.

IV. That such distractions, whether sensory or otherwise, have less effect on associated material than on detached. This is proved not only by the fact that more words

are remembered from an associated list after a distraction, but also by the important fact that practically no words are recovered from those lost in the case of associated material, whereas many of those lost from an associated list are recovered, seeming to show that the associated material is better organized. By this is meant the grouping of impressions around attitude.

V. This is further proved by the fact that if there is a passive period after a detached list before a distraction intervenes, the effect of the distraction is lessened.

VI. Even where there is an apparently complete loss of associated words they can be recovered to a great extent by the metronome method already described.

VII. This recovery method failed in the case of abstract associated words which are in this respect on a par with the concrete detached words.

VIII. The effect of attention in the case of associated material was an aid, but not so in the case of detached words.

IX. That motor accompaniments are an aid, provided they are not new or unusual, as then they act the same as another attitude intervening. If the words are grouped around some existing set of attitudes, better still if several motor activities are combined, as in the case of writing in the air, they are remembered better. This combination of motor activities is really a motor definition of attitudes which in turn is a compounding of reflexes.

X. Disturbances coming in the middle of the list are not so effective on memory as when coming at the end. In some individuals a distraction may even reinforce what went before, but in the majority of cases it effaced what went before and reinforced what came afterwards. The greatest effect of shocks on memory is retroactive.

REMARKS

It is not our purpose to go into the physiology of the brain processes which are at the basis of memory. That has been done elsewhere as well as the knowledge of such will allow. Leaving that aside, a few stray notes on some points may not be out of place.

From the experiments as they stand it would seem to be an evident conclusion that pleasant impressions are remembered better than unpleasant. Such a conclusion is to be taken with care. It may be that the unpleasant things are remembered, potentially just as well the pleasant, but their expression in the normal individual may be suppressed. They arouse attitudes or reactions which are just as intense as the pleasant and that is the reason why both classes are remembered better than the indifferent, which in all probability are completely dropped. The unpleasant impressions may have made connections with old attitudes, old states of consciousness in the larger sense. These old ones may be completely suppressed, and the new ones share the same fate. Abnormal cases show us that these old experiences, possibly long since absent from self-consciousness still play a part on incoming impressions. They still play it as psychophysical attitudes. This then is in all likelihood the explanation of the experiments dealing with pleasantness and unpleasantness. One subject who always made a good record with unpleasant words took this means, without knowing it, of course, of expressing in a dislocated way some of his personal feelings, which were usually well masked. Another subject always remembered unpleasant colors for the reason that some brown colors aroused an intense antipathy to the whole experiment, notwithstanding the fact that he was something of an artist. The reason has lately been found, by experimental methods as a continuance of this investigation, to have been a long forgotten experience of childhood¹—one that was vividly emotional. In a general way this holds true of the whole investigation.

It may be worth while to make a note in regard to the memory of concrete and abstract words, and the effect of distractions on them. Impressions which take a large part in the actual contact of the organism with its environment will arouse more attitudes, and they will be more habitual and ingrained than those impressions which do not perform such a function. They will have greater emotional value, greater feeling value, and will represent a greater number of instinctive or reflex reactions in one direction. Such are

¹Cf. JOURNAL OF ABNORMAL PSYCHOLOGY, April-May, 1912.

concrete words, hence they are better remembered, less disturbed by distractions, and even when apparently banished by some cause they are less liable to complete loss. To a large extent the opposite is true of abstract words. They serve the purpose of adjustment to environment, but more remotely.

It may also be stated that if a subject is in an unpleasant "frame of mind," as we say, that is, if the subjective psychophysical attitudes are of a certain kind, and if a certain incoming impression does not arouse a very strong attitude of its own, then it will take on the tone of the complex existing at that time. The converse is true of pleasant complexes. Impressions which harmonize with the existing attitude stand a good chance of being well remembered. Psychophysical attitudes then select from impressions those that are remembered by way of conscious reproduction. Inhibition or suppression, of course, may take place as already explained. The cause of any existing attitude may be said to lie in the response to some previous impression or impressions with a high emotional and feeling tone, and which were not allowed at the time adequate expression in the normal way. Distractions are also most effectual when they arouse intense psychophysical attitudes. When they are very intense, as in the case of accidents, then much is forgotten that happened before and after the accident. Our work shows the effect, on the whole, is to influence what preceded the accident.

When we come to associated and dissociated words the matter is not different. We have seen that if impressions arouse a strong psychophysical attitude they stand a good chance of being remembered. Some limitation must be put on that statement. If several impressions come in close sequence to one another, and, as is probable, they arouse quite different psycho-physical attitudes, then the various combinations of reflexes which give expression to these attitudes will be mutually inhibitory. In this way inhibition takes place, and thus many words in a dissociated list are lost. One psycho-physical attitude will cancel or partly cancel another. To this can also be added the effect of ideas already suppressed. In associated words only this

last feature is operative to banish them. The impressions are all centered around one central attitude and hence this mutual inhibition is absent. Not only are they centered around one central attitude, but around one which in most instances is well established.

Much is now written about mental complexes or constellated ideas. This constellating or complexing seems to be brought about by the actual or potential reactions of the organism. Physiologically, it may be said that ideas are grouped and unified by the compounding of reflexes, that back of emotions and feelings are well-defined and well-established reflex expressions. To include this and also to keep in sight the part which appears in consciousness, the term psychophysical attitude is convenient. These much ignored initial responses are the background and foundation of our whole mental life. To include all responses, then, to include consciousness and that small part called self-consciousness (by some this latter is wrongly called consciousness and the word is denied further extension), to include organic feelings (a poor term), also many reactions which never come to self-consciousness but which are undoubtedly the most important, and on which the stress is here laid — for all this we use the term psycho-physical attitude. Consciousness would be a much better word for the organism's total reactions, and then self-consciousness, attitude, reflex, etc., could be used to mark off the various ways of functioning.

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APPENDIX

Example of Pleasant List:

Warm, income, perfect, amiable, delightful, hopeful, spring-time, kind, elegant.

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divine, lovely, home, best, pleasant, gorgeous, truthful, good, rapture, healthy, golden.

Rich, dream, clever, joyful, rose, skill, laugh, lily, tidy, daisy, strong, flower, merry, gentle, honest, clumsy, cottage, sweet, sincere, polite.

Example of Unpleasant List:

Defeat, drear, dirty, shriek, drown, false, skull, cringing, scold, sigh, groan, whine, crape, woe, knife, vulgar, sullen, rude, choke.

Example of Indifferent List:

Request, ability, order, need, made, great, about, relish, suppose, cousin, answer, morning, child, earth, view, start, number, enough, bush, again.

Example of Associated List:

Field, trees, orchard, breeze, reaper, stream, river, meadow, clouds, mower, brook, forest, garden, road, cottage, woods, swamp, plain, hay, cut.

PREFACE

The investigation which forms the basis of this treatise was originally undertaken for the purpose of supplying a scientific guide to courts of law whereby they could settle disputes arising from Trade Mark Infringement more equitably. There came letters to Professor Münsterberg from members of the legal profession,¹ declaring that under the present system of settling such disputes there always exists an element of uncertainty as to whether the imitating trade-mark, label, or whatever it may be, is really similar enough to the copied article to deceive the "unwary purchaser" or not.

To furnish such a guide it appeared, after much deliberation, that the proper thing to do would be to construct a scale of graded similarity among meaningful objects of the same category, and then to determine the point at which the similarity becomes so great that the objects are no longer distinguishable.

ERRATA

p. 27, l. 27, "depend," *read* "defend"

p. 64, l. 16, ranged 10x, *read* ranged between 10x.

The foundation for our practical structure. Hence the work was divided into two parts. The first attempts to answer the question: What is the relation between recognitive ability and different degrees of meaningful similarity? The second, How can the results obtained be applied to industry and jurisprudence? The former, of course, is by far the longer, inasmuch as it comprises the experimental data and all theoretical discussion. The latter is brief, as it should be, because it is written with the assumption that the person who turns to the section on the Application of the Laws of Recognition to

¹E. S. Rogers: The Unwary Purchaser: A Study in the Psychology of Trade Mark Infringement. *Michigan Law Rev.*, Vol. 8 (1910).

²Münsterberg, Psychology and Industrial Efficiency.

PREFACE

Jurisprudence, will have made himself acquainted with the bases of those laws by first having read the theoretical part.

The investigation was undertaken at Harvard University in the fall of 1911, and was carried on uninterruptedly to the beginning of 1914. Over 5000 individual tests were made and the same number of judgments recorded.

I wish to take this opportunity to express my hearty thanks to Professor Hugo Münsterberg, who not only suggested the problem to me, but who, by his constant advice and encouragement, enabled me to carry it to completion. My gratitude is also due to Dr. H. S. Langfeld for the interest he has taken in my work, for his many suggestions, and for the time he has devoted to it in the capacity of an observer.

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PART I

INTRODUCTION

Recognition has well been defined as that psychical process which makes a present state of consciousness appear to us as having already formed part of our previous experience.¹ Recognition, therefore, is midway between perception and recollection. It is related to the former in that it depends upon an external stimulus for its arousal; it is akin to the latter in that it is always accompanied by a consciousness more or less clear of a past relation between the ego and the object of present apprehension.

There is no fundamental difference between recognition and discrimination. Each of these terms refers to the same mental process looked at from different points of view. If recognition means re-cognize, to know again what has been known before, discrimination means to separate, to distinguish two contents of consciousness, one of which has been appropriated by the self, and is employed as a check or a foil upon the other. We recognize every time we discriminate, but we do not necessarily discriminate every time we recognize.² If the mind is suddenly and unexpectedly flooded by the feeling of likeness, we have recognition proper. If we know what to expect in perception and experience something that is different but generically related, we have discrimination proper.

The twofold nature of the mental state we are describing is neatly illustrated in the experience we have while waiting for a friend, or some one we know, at a railway station. As the scores of people come pouring out of the train, we scrutinize their faces and repeatedly form the judgment: This is not he, this is not he. The distinctness with which we form it is directly proportionate to the resemblance between the person

¹ D. Katzaroff, *La Recognition*; *Archives de Psychologie*, Vol. XI, p. 2.

² James, *Principles of Psychology*, Vol. I, p. 528.

we see and the person we are looking for. In this act we are noticing differences. We are separating that which is from that which we know ought to be. Suddenly our attention is arrested by a familiar form, the gait is familiar, the facial expression, the dress, in short, the total impression is one of general affirmation. We owe it to the fact that our lives are cast in a world which has uniformity and complexity, likeness and unlikeness combined in one that we are constantly called upon to use both recognition and discrimination in our daily life.

Because recognition depends upon an external stimulus for its arousal and because it is a consciousness of having experienced that stimulus before, recognition may be considered as both objective and subjective. But it is more of the latter than the former, since in the act of recognition the stream of consciousness seems to fold back on itself.

Again, recognition is either direct or indirect, immediate or mediate. When the former, it is a spontaneously induced mental state, completed without the conscious aid of any other psychic factors. When the latter, it is a slowly dawning state, depending upon mediating ideas, images and associations for its completion. Thus as I walk along the street of a strange city I suddenly hear my name called out. I look about, and recognize one of my intimate friends. The process is instantaneous, it is almost like direct apprehension, yet it is not that because there exists the consciousness of "I-know-you," or I have felt-you." In the words of Bergson, it is recognition without thought.⁸ But again, I am sitting in a hotel lobby when a man comes up to me and greets me by name. I return his greeting, but I cannot "recollect" who he is. Rapidly I cast over in my mind the possible places where I could have met him. I call up the images of some persons whose acquaintance I recently made. But no, he is none of them. He probably sees my confusion, and asks me if I remember Blank Beach. My mind instantly flies thither. I call up a memory image of the beach named. I try to put myself into situations where I possibly

⁸ *Matter and Memory*, p. 113.

could have been in his company. I now recall a boating party where I made several new acquaintances. Perhaps he is one of them. Why, yes; his face looks familiar when he smiles. His name begins with a vowel—with an A, I think. At this point he introduces himself to relieve me of my perplexity. The moment he mentions his name I grasp his hand. I feel a sense of relief. Of course I know him. He is the man who told me of his strange experience in Africa; and there is the scar on his forehead, showing where he had been wounded in a battle with some natives. Here we have recognition through inter-mediating facts—ideas, images, associations, and efforts of will.

We have sketched two extreme cases of direct and indirect recognition. But that does not exhaust the process yet. Recognition may further be distinguished with respect to mental complication. If it consists merely of an already experienced impression it is termed simple recognition. Most direct recognition is of this sort. If, on the other hand, it concerns the nature, importance and circumstance of the object, it is said to be complex, intellectual, or conceptual recognition. Most indirect recognition is of this sort.⁴ Complex recognition may be either ideational or affective. As a rule it is a combination of the two. But if it is the former alone, it is accompanied and supported by visual, verbal or auditory images, by associations, reflections and inferences. If the latter alone, it is the embodiment of the feelings of familiarity accompanied by such other feelings as pleasantness, satisfaction, relief, fitness, appropriateness, or the reverse of these if recognition is uncertain.

The feeling of familiarity (Wundt's *Wiedererkennungsgefühl*, of Höffding's *Bekanntheitsqualität*), is the most fundamental factor in all forms of recognition. And it is because this feeling cannot be further analysed that recognition is called a primary mental quality. None of the attempts made to describe the feeling of familiarity have been especially enlightening. Wundt says it comes about from the assimilation of the sensory im-

⁴ Katzaroff, *op. cit.*, p. 5.

pression with the corresponding image. It is independent, however, of the assimilative process itself, and is only stirred up by it.⁵ For Höffding the feeling consists in the process of assimilation.⁶

Wundt further maintains that each separate act of recognition "possesses its own peculiar quality." Thus he says, "The feeling differs when we recognize our friend, X, and when we meet Mr. Y. whom we did not care to see again."⁷ In each case the peculiar quality of the feeling of familiarity is a function of the object of recognition. Like all feelings, it is determined by "the influence of the ideational content of consciousness upon the apperception."

In reply to this, however, it might be said: (1) The different feelings of familiarity aroused by different objects may not be distinct feelings, but fusions of various feelings—the feelings of pleasantness or disgust, ease or strain, fulfillment or disappointment—which blend with each other and thus swamp the original and unique feeling of familiarity. (2) The recognition of geometrical forms, nonsense syllables and other objects that are devoid of ideational content, is invariably accompanied by the same and uniform feeling of familiarity.⁸

The theory advanced by Höffding in his famous controversy with Lehmann says that the feeling of familiarity is due to the fusion of two similar mental processes: one, 'A,' which is the perception of the object, and the other, 'a,' the image that the object had left behind when it was first perceived. According

⁵ Introduction to Psychology, pp. 105-7.

⁶ Vierteljsch. f. wiss. Philos., Vol. XIII, p. 432.

⁷ *Op. cit.*

⁸ Katzaroff, *op. cit.* See also Meumann, *Bekanntheits- und Unbekanntheitsqualität*; Arch f. d. Ges. Psychol. Vol. XX, p. 36. In this short, but well written article, Meumann makes an excellent distinction between the feelings of familiarity and strangeness. The former is characterized by a certain easiness with which the psychic processes—both organic and mental—flow; by a mild feeling of relief, satisfaction and pleasure; by little voluntary attention; and by the presence of reproduced memory images. The latter is characterized by a sort of inhibition or feeling of arrest which is both motor and mental; by the presence of a peculiar consciousness of "inner emptiness" by a feeling of distress and by the presence of images and associations.

to Höfding the image, 'a,' fuses with the percept, 'A,' and it is the blending of the two that gives rise to the peculiar feeling of familiarity. In some instances the coalition is rapid and complete, and so we have direct recognition with full certainty. In other cases the overlapping of 'a' with 'A' is only partial, or the superposition takes an appreciable length of time, and so we have indirect recognition accompanied with uncertainty or doubt.⁹

In reply to this view it may be said: (1) It has been demonstrated time and again that recognition can take place without the aid of mental imagery.¹⁰ (2) Experiments have been made showing that even when the subject has a *distorted* image of the object perceived, he can recognize it accurately nevertheless.¹¹

Claparède firmly believes that recognition does not depend on memory images. Very often a person, in describing an object he had formerly perceived, shows himself to be absolutely mistaken as regards its qualities—such as color, size and form; yet when confronted with that object he will recognize it immediately as the one he had seen before. "If recognition depended on mental imagery, then the distortion of the memory image ought to prevent correct recognition from taking place," concludes Claparède.¹²

Besides being simple or complex, recognition is also either implicit or explicit. Implicit recognition takes place when an object appears familiar to us, but we cannot locate it in our past experience, we do not know its setting. Such expressions as, "Your face looks familiar, but I don't remember when I met you"; or "That air sounds familiar, but I can't recall the opera from which it comes nor where and when I heard it"; or "That name sounds familiar, but I can't recall what the person looks like",—are all indicative of implicit recognition. That is to say, there is no doubt about the object having been ex-

⁹ *Op. cit.*

¹⁰ Gamble and Calkins, Die reproduzierte Vorstellung beim Wiedererkennen, etc., Zeit. f. Psychol., Vol. XXXII, 177ff.

¹¹ Claparède, Recognition et Moitié; Arch. de Psychol. Vol. XI, 89ff.

¹² *Op. cit.*

perienced before, but the connection, the circumstances are lacking. Explicit recognition, on the other hand, is where the object is fully known either in all or in most of its tangible relations to the time, place, setting and circumstances in which it was originally experienced. It follows, of course, that all direct, immediate recognition is explicit, whereas indirect recognition has to go through a stage of implicitness.

Finally, recognition is either voluntary or involuntary. Indirect recognition is usually of the former type, while all direct recognition is of the latter.

It is no exaggeration to say that recognition and its correlate, discrimination, are among the most active and valuable factors of our mental make-up, yet as compared with what has been written on other faculties, such as attention, memory and feeling, very little space has been devoted to our subject.

It is only within very recent years that psychologists have begun to investigate such problems as the relation between recognition and association;¹³ the nature and cause of the errors of recognition;¹⁴ the relation between recognition and the number of items to be recognized;¹⁵ the relation between recognition and recall as well as the influence of different kinds of stimuli on these faculties;¹⁶ and finally, the relation between recognitive ability and the time interval.¹⁷

Small showing, indeed, for what James calls "one of the ultimate foundation-pillars of the intellectual life."¹⁸ For it must be obvious that the perception of likeness and unlikeness is the very bed-rock of consciousness. On it depends our knowledge of the unity of the self, as well as our knowledge of the uniformity of nature. In the process of mental evolution recognition follows perception and precedes ideation.¹⁹ Suppose we

¹³ Gamble and Calkins, *op. cit.*

¹⁴ Katzaroff, *op. cit.*

¹⁵ E. K. Strong, The Effect of Length of Series on Recognition; *Psych. Rev.*, Vol. XIX.

¹⁶ H. L. Hollingworth, Recognition and Recall; *Amer. Jour. Psych.*, Vol. XXIV.

¹⁷ Strong, Effect of Time Interval on Recognition. *Psych. Rev.*, Vol. XX.

¹⁸ James, *Principles of Psychology*, Vol. I, p. 520.

¹⁹ Sully, *Contributions to Psychology*, p. 11.

failed to recognize our friends and kinsmen, our homes and surroundings, our sensations and ideas every time they were presented to us; suppose every recurrent experience were a new experience—it is evident that our life from the cradle to the grave, if we could leave the former long enough to reach the latter, would be like that of the moth in the presence of the flame.

But recognition and discrimination are also operative in finer experiences than those we have had occasion to mention. We speak of “a subtle mind,” “a fine sense of discrimination,” of “ability to split hairs,” and so on. What do we mean by these expressions? Certainly the recognition of a friend does not involve a fine sense of discrimination, but the comprehension of the relation between a falling apple and the revolving moon does. Yet these two mental activities differ from each other in degree only. Is not the entire process of reasoning essentially one of recognizing the relations among ideas, or among “things” as modern logicians like to put it?

“Inference,” says Aikens, “is the *recognition*²⁰ of a new relation of things without which the relations asserted in the premises could not exist.” Mathematical reasoning is chiefly reasoning by parity. In geometry, for example, each successive proposition is based on one or more preceding propositions. Success in the solution of geometrical problems, therefore, depends not only on a knowledge of the foregone propositions, but also on the faculty of discerning what theorems are applicable to the problem at hand. In short, the highest type of mental activity we can think of involves recognition and discrimination. Hence Locke is right in saying, “In having our ideas unconfused, and being able nicely to distinguish one thing from another where there is but the least difference, consists in a great measure the exactness of judgment and clearness of reason.” And again, “Judgment . . . lies in separating carefully one from another ideas wherein can be found the least difference, thereby to avoid being misled by similitude and by affinity to take one thing for another.”²¹ Nor ought we to

²⁰ The Principles of Logic, p. 153.

²¹ Human Understanding, II, XI, 1, 2.

omit James' estimate of this faculty. "Some people," he says, "are far more sensitive to resemblances and far more ready to point out wherein they consist, than others are. They are the wits, the poets, the inventors, the scientific men, the practical geniuses. A native talent for perceiving analogies is reckoned by Bain, and others before and after him, as the leading fact in genius of every order."²²

We have now seen how recognition is defined; we have described some of its phases; we have illustrated in brief how it functions in life, and have noted how leading psychologists rate it in the order of human intelligence.

Let us now turn to our own investigation.

PRELIMINARY EXPERIMENTS

STATEMENT OF PROBLEMS

The preliminary experiments, undertaken in the fall of 1911, were chiefly for the purpose of devising means and methods.

Some of the problems I set out to investigate were as follows:

(1) What is the effect of distribution of attention on the cognitive process? Or in other words: How does the number of objects among which a thing is perceived and re-perceived affect recognition?

(2) How does recognition vary with the time of perception?

(3) What is the influence of similarity on recognition, and is there a mathematical relation between discriminative ability and degree of similarity?

(4) What is the difference between external and internal similarity, i.e., does similarity in form affect ideational recognition differently from similarity in meaning? It is well known that deception is not only sensory but also ideational.

(5) What is the difference between substitution (displacement of one object by another), and mere interchange of position?

(6) What is the influence of environment on recognition? Will a thing be recognized as readily when observed in a new

setting on the second exposure as when observed within the setting in which it was originally experienced?

Let us discuss these problems somewhat in detail.

Our experiments purport to investigate a representative cross section of the faculty of recognition. For that reason we tried to reinstate in the laboratory conditions that were analogous to those under which we perceive and recognize things in daily life. But not only must these conditions be representative of life situations, they must also be of such a nature that we can vary them quantitatively. For surely the average person's state of attention is different on a crowded thoroughfare from what it is on a lonely country road. Likewise the average woman's attention in the teeming department store is not the same as in the little village store. How can we reproduce these different forms of attention in the laboratory?

On the crowded business street, for example, a person's attention is constantly being distracted. Thousands of stimuli knock at the door of consciousness, and, as they are in a fleeting state and as the individual himself is moving rapidly, they can obtain but a very small fraction of his attention. But that is not all. Some of the things he perceives are more interesting than others; some are novel, others plain; some are familiar, others strange; some are pleasing, others displeasing; some set up thought processes, others do not. In short, with one sweep of the eye he takes in persons, vehicles, shop windows, signs, forms, names, colors, positions,—everything that falls within the periphery of his vision; not to mention the myriads of auditory sensations. Here is a very rich stream of consciousness indeed. How can the psychologist reproduce it in his subjects in the laboratory?

(1) Our own method was quite simple. If we simultaneously expose, let us say, 8 words of ordinary meaning for 4 seconds, and instruct our observer that he is to grasp those words well enough to be able to recognize some or all of them when they are re-exposed after a definite interval, we may rest assured that his state of mind will be quite representative of what it is on the crowded thoroughfare. As there, so here, some words, i.e., things, will be more interesting than others; some

familiar, others strange; some rich in associations, others poor; and as the subject knows the exposure time is limited, he will hurry from one word to another, just as his attention is hurried from one thing to another on the street.

The introspection of my observers affirmed the correctness of our method in this respect. Their attention was not distributed in the sense that they perceived 6 or 8 words in one pulse of attention. In fact, it is doubtful whether such a distributed state of attention can exist.²³

Wirth, who has done extensive work in this field,²⁴ did not obtain positive results. He found that within certain limits the spreading out of the attention over a number of simple objects causes a corresponding intensification of the attention with respect to each object. This, however, is no proof of a distributed state of attention. The very same thing is found with rapidly fluctuating or distracted attention. What, then, is the difference between the two? In distributed attention it is assumed that the same amount of attention is simultaneously given to each of several objects which enter consciousness; whereas in distracted attention, the attention is given more or less completely to one thing at a time. According to Hylan there is no distributed attention in the strict sense of the word. "Attention," he says, "really fluctuates and is not distributed."²⁵

For our experiment it really makes no difference whether attention actually can be distributed or whether the phenomenon that goes under that name is due to the rapid fluctuation of attention. Suffice it that we were able to produce in our subjects the states of mind that they experienced on the city streets. Thus by exposing a group of disconnected words simultaneously for a brief space of time, and instructing my observers to perceive all of them with sufficient clearness to recognize them a few seconds later or to note whether any one word had been

²³ J. P. Hylan, *The Distribution of Attention*, *Psych. Rev.*, Vol. X.

²⁴ Die Klarheitsgrade der Regionen des Sehfeldes bei verschiedenen Vertheilungen der Aufmerksamkeit. *Phil. Stud. I.* Also *Psych. Stud.*, Vols. I and III.

²⁵ *The Distribution of Attention*, p. 2.

displaced by another, they declared that they managed to read the words once, but they had a feeling of being hurried, like one who desires to see things on the street, but knows it is not good manners to stop and gaze, or that he has no time for that. Then, too, there was a decided rivalry of attention. That is, the subject having read one word went to the next, but immediately began to fear that he did not get the preceding one correctly, so that there was a tendency, a desire to look at it again, a sort of dragging back of the attention. This, however, was inhibited by the fear that if too much time were spent on one word, the shutter would go down before all words were read. And so he hastened on with the same feeling as the small boy who wishes to see the entire line of parade and at the same time to keep up with the head of it, too.

Now this is precisely the state we wished to produce. For, in the first place, it is characteristic of the normal and daily sort of attention, except when one is preoccupied of course; and in the second place, it really furnishes a means for giving the same amount of attention to each of several things which are complex in their nature—as all meaningful things are. When I say the “same amount” I mean the requisite or proportional amount. For unless the stimuli are of the simplest possible kind, such as pressure, or shades of gray, or pitch; unless they are measurable in terms of their own kind,—the same stimulus may not only require a different threshold for complete perception with different subjects, but stimuli of the same sort may differ from one another considerably in complexity.

This is the first serious error made by Bourdon²⁶ and Katzaroff,²⁷ the one using words, the other geometrical forms as stimuli in their experiments on the process of recognition. These authors thought that by exposing each word or figure for the same length of time, the whole of it would receive the same amount of assimilative attention. But that is not so. The words “rose” and “juste”,—two words used for comparative purposes by Bourdon, do not necessarily possess the same amount

²⁶ *Reconnaissance, Discrimination et Association*, Rev. Philos. Vol. XL.

²⁷ *Op. cit.*

of interest to the same person. Therefore, it is wrong to assume that by exposing them for the same length of time they will become equally impressed on consciousness. Likewise with Katzaroff's designs: the drawing of a house and a meaningless, complex trellis-like figure,²⁸ each exposed for 6 seconds, will not produce the same impression on the mind. The former has meaning, the latter has not; the former is rich in associations, the latter is not; the former has distinct, conceptualized features whereby it can be carried in memory, the latter has none of these. Hence the different kinds of recognition these authors describe—recognition by general impression, recognition by remembrance of some special feature, recognition by association, etc., may all be functions of the object and not different phases of the recognitive process.

Thus, if we examine some of the introspection Katzaroff reproduces with reference to the figures sketched in his well-written paper, we find that those designs which had meaning attached to them were recognized or discriminated in meaningful terms; the others on the basis of general impression. To assume that groups of objects so diverse as Katzaroff's figures have the same attention value and make the same impression simply because each of them is exposed for the same length of time, is like assuming that it is as easy to recognize potatoes as presidents, because they both begin with p.

Of course, it may be retorted that Katzaroff was studying the process of recognition in general, and therefore he had a right to use both meaningful and meaningless material. Even if that be granted, it is still doubtful whether it was proper to lump together the results of such diverse material.

But there is another objection to the indiscriminate use of meaningful and meaningless material in experiments on recognition, and that is that the subject, though he succeed in recognizing the meaningless object or its substitute on the second presentation, is often at a loss as to how he should describe it, or how impart his knowledge. Thus an undue burden is put on the introspective memory there is no checking and

hampering, confidence falls, doubt increases, and the observer either finds himself unable to judge at all, or else makes a guess judgment as the easiest way out. This is verified by the fact that when Katzaroff's observers introspect on the meaningless figures, they go about it in an indirect way and very often end in doubt. This is also the reason why the subjects in Moore's experiment on abstraction²⁹ found themselves unable to give a reason why certain figures looked familiar or strange.

In the experiments that form the basis of this paper, both of these sources of error have been avoided by employing uniformly conceptualized material, simultaneously exposed in definite groups for a definite length of time, and by allowing the subject to devote as much attention to each item as he felt was necessary, the only condition being that he perceive them all within the given time of exposure. And this, of course, he did because he never knew what word would be replaced. Hence, if he looked at some words twice and at others once, or if, when 8 words were exposed for 4 seconds, he did not look at each word for $\frac{1}{2}$ second, but at some for 1 second, at others $\frac{1}{3}$ second and at others still only $\frac{1}{4}$ second, it was, as introspection showed, because the former were somewhat unusual in his experience, whereas he felt he could never be deceived by the latter. Thus our technique once more showed its ability to stimulate, under experimental conditions, those factors of recognition that function in normal daily life. For, do we not, in actual life, when we wish to fix in our minds landmarks, signs, faces or what not, look at some a little "closer" than at others? But we shall revert to this later on.

(2) Not only do we usually look at some things closer than at others, but at some we also look a little longer than at others. Hence the second variable was the length of exposure. Besides keeping the fixation period constant and varying the number of objects exposed, I also kept the number of objects constant and varied the time of exposure.

(3) A third variable was the degree of similarity. In the

²⁹ The Process of Abstraction, Univ. of Calif. Publications in Psychology. Vol. I, No. 2.

preliminary experiments I did not measure the degree of similarity, but as this is one of the main objects of our investigation, we shall leave the discussion of this factor to its logical place. In these early tests I proceed on the assumption that there is some similarity between two words that begin or end with the same consonant, whereas there is no similarity, or zero similarity between words that do not begin or end with the same consonant, or do not have the same vowel sound. If this assumption were correct, then, other things being equal, the substitution of the former kind of a word ought to produce more false judgments than a substitution of the latter kind. Or in other words, suppose the conditions, with respect to length of exposure, and the number of words exposed, are such that the replacement of one word by another of zero similarity can be recognized 100 per cent of the times, then if there is any similarity between two words that begin or end with the same consonant, a substitution of one for the other ought to produce some false recognition.

(4) It is well known that diverse impressions may produce similar states of consciousness; therefore, if we employ stimuli that lack sensory resemblance but possess meaningful similarity, we can study the influence of similar ideas on one another, i.e., we can note the process of recognition within the realm of pure ideas. Then we can compare the results obtained with the two different sets of material and see whether our senses or our ideas deceive us most often.

(5) It was also our desire to find out whether a simple change of the position of two words within a list of a given size, would be recognized as easily as the removal of one word and the substitution of another for it. By this method we could determine whether recognition is stronger for content than for space relations, or vice versa.

(6) As the sixth factor—the influence of environment on recognition was not touched upon in the preliminary experiments, and as it forms an important part of the investigation, we shall defer its discussion at this point and take it up later on.

METHOD OF PROCEDURE

Since I was investigating ideational recognition and discrimination, it was imperative that I use meaningful material. To that end I employed, in these early experiments, bisyllabic words of ordinary usage, selected at random from the dictionary and varying in length from 5 to 7 letters. These words—nouns, adjectives, verbs and adverbs—were typewritten in small letters, on 3 x 5-inch plain filing cards. The words, triple spaced, were arranged in two vertical columns $\frac{1}{2}$ -inch apart. These typewritten cards came in pairs marked A and B on the back. The first was the 'standard' and the second (the variable) was a reproduction of the first with the necessary change affected.

Thus, if I wished to investigate the problem of imitation, i.e., the influence on the recognitive process produced by substituting one word for another similar to it in form, my cards would appear thus:

A		B	
pretty	justly	pretty	justly
hasty	early	hasty	early
improve	ledgy	impose	ledgy
kindly	glory	kindly	glory

If I wished to investigate the influence of change of position, card B would be a reproduction of A with the desired transportations made. Finally, the same card exposed twice, i.e. for fixation and trial, served as the control.

APPARATUS

The apparatus used in this investigation was very simple. It consisted of a box-shaped screen, completely separating subject from experimenter. In this screen was an oval shaped aperture through which the observer looked with both eyes at the printed card, or at the picture post cards of the later experiments, exposed on a black background. The aperture was opened and closed by a shutter which moved vertically in grooves. This

shutter was closed by two rubber bands attached to the lower end thereof and to the apparatus, while its opening was affected by means of a string secured to its top, running over a noiseless pulley and controlled by the experimenter.

This piece of apparatus was set on a low table so that the observer could look in through the opening conveniently while seated in a comfortable position.

The time of exposure, being one of the factors under investigation, varied with different series. The interval between the first and second exposure, however, i.e., between the fixation and trial, was kept constant throughout these and all succeeding experiments. This interval was 20 seconds.³⁰ It was not deemed advisable to make it longer, lest the minds of some of the observers should begin to wander. In that event we should have had recognition with mental concentration in some cases and recognition with distraction in others, and we should not know which is which. It was our desire to make conditions as uniform as possible for all observers.

I had six observers—five men and one woman—all of them graduate students and members of the Harvard Psychological Laboratory.³¹ They came to me singly, once a week. All sittings were held between 9 and 1 o'clock, each lasting one hour.

I gave my subjects the following instructions: In daily life one is occasionally confronted with the phenomenon of mistaking one name or one word for another, because they have some resemblance either in spelling, sound or meaning. Now, I am going to expose a group of words for x seconds, and after an interval of 20 seconds I shall reexpose the same group either intact or with some change. See if you can discover that change, and in your introspection at the end of the second exposure, tell me whether you carried any of the words in imagery, and if so, what kind of imagery it was, and how many you carried. Tell me whether you had any internal or

³⁰ I should like to have varied this factor also, but I had no time for that.

³¹ I wish to offer my thanks to Miss O. L. M. Seeley, Drs. V. V. Anderson, L. M. Elliott, R. C. Givler, and Messrs. E. C. Tolman and I. W. Williamson for their patience, politeness and consistency in assisting as my subjects.

external association, i.e., whether you associated some of the words in the group with each other, or with something extraneous to the group. Tell me what rôle, if any, imagery or associations play in the recognitions you make. Try to distribute your attention equally among all the words, i.e., do not concentrate your attention on some at the risk of failing to comprehend others. Try not to let your mind wander during the interval. Yet be natural, i.e., choose your own means for making the greatest number of correct judgments. In short, do your best, in your own way, to recognize a change if one occurs and tell me, if possible, precisely what that change is."

I made this last suggestion in order to reduce the element of chance as much as possible, for when 8 words are exposed and one of them is changed, the probability that the subject will guess the critical word as new is $\frac{1}{8}$, but the probability that he will be able to couple with it the word it had displaced, by mere chance, is $\frac{1}{64}$.

In the fore period I signaled my subjects to get ready about 2 seconds before the shutter went up. I did not give a similar signal before the second exposure because they had been advised to have their minds fixed on the experiment during the interval and to be prepared for the trial exposure. All forms of noise and distraction were avoided during the interval so as not to disturb the mental content of the observer. Exposure and interval were timed with a stop watch. The time it took the shutter to open or close was approximately $\frac{1}{4}$ of a second.

A correct judgment, "change" or "no change," according as one did or did not occur, was marked +, and incorrect judgment —, and a doubtful one, i.e. one whose certainty the subjects themselves rated as between 20 per cent and 79 per cent was marked \pm , and given half value in the computation of the results.

RESULTS

SERIES I

After putting my subjects through a period of training that lasted three weeks I undertook to find out the minimum ex-

posure time wherein, with a given number of words to the group, the substitution of one word for another totally unlike in form and meaning would be recognized 100 per cent of the times. I found that when groups of 6 words were exposed for 5 seconds, the replacement of the words

(A) broken, wander, career, native, patent }
by (B) simply, return, winter, extent, always } Case I
respectively, and

(A) couple, revolt, beyond, lesson, beacon }
by (B) hoping, money, assume, motive, voters } Case II
respectively, and

(A) parish, shiver, latest, insert, retort }
by (B) devote, rocken, hockey, chosen, demand } Case III
respectively, gave the following per cent. correct judgments:

Case I = 92%
" II = 98%
" III = 96%

calculated on the basis of 30 judgments in each case. These figures are sufficiently close to 100% to justify the acceptance of these conditions as affording perfect discrimination for totally dissimilar words.

What would happen if, instead of substituting wholly unlike words, I substitute words that bear some resemblance to the standard ones?

SERIES II

To answer this question was the purpose of the next experiment. To make the answer conclusive, however, it was necessary to keep all the conditions of the first experiment intact, i.e. not only was it essential to use 6-word groups exposed for 5 seconds, but it was imperative that I employ the very same words in the same settings and the same observers. Here was a difficulty. My subjects would surely recognize some of the words and they might even learn the words that had been

replaced on the previous occasion and thus make an effort to memorize them on the first exposure in these comparative tests. But inasmuch as I was proceeding on the assumption that similarity interferes with discrimination, I allowed this factor to stand because, if anything, it would militate against my theory and not in favor of it. Hence, should I on this second occasion obtain lower percentages than on the first, my results would have all the more value.

Therefore, after an interval of one month, using the same groups under precisely the same conditions, I replaced the words

(A) broken, wander, career, native, patent }
by (B) battle, window, coming, notion, police } Case I'

respectively, and

(A) couple, revolt, beyond, lesson, beacon }
by (B) control, remove, belong, letter, better } Case II'

respectively and

(A) parish, shiver, latest, insist retort }
by (B) parlor, shower, lowest, insert, report } Case III'

respectively, and obtained the following per cent. correct recognition based on the same number of judgments as the former percentages:

Case I' = 90%
" II' = 94%
" III' = 91%

The differences between the first and second sets of figures are not large. But if one bears in mind that the observers found nearly all the groups familiar this time, and that because of this familiarity they were able to carry more words by rote memory during the interval, one will have to admit that the substitution of one similar word for another does tend to inhibit recognition.

Now it will be asked why do I divide these words into cases I, II, III, and I', II', III'? The answer will be found by examining the words under each of these latter cases first, with

respect to those in the former ones, and then with respect to one another. The words in case I' have one critical letter in common, those in case II' have two initial letters in common, and those in III' have three or more symmetrically arranged identical letters in common. Hence each of the primed cases differs from each of the unprimed ones in absolute amounts of similarity, while they differ from one another in proportionate amounts. Therefore, if our theory is true, then the per cent of correct recognition ought to vary inversely as the degree of similarity. Now this is true with two of the primed cases. The reason why case I' is lower than either II' or III' is probably because some of the groups that fell within it were difficult to grasp or especially confusing. This is substantiated by the fact that in series I the analogue of this case, i.e. case I also stands lower than either of the other two.

That this is the true explanation is substantiated by the introspection of my observers. Some groups lent themselves to internal associations better than others; of some the words could be carried in memory during the interval more readily than of others. But the most important thing for us to notice is that the per cent correct recognition for each of the primed cases as compared with the corresponding unprimed ones is lower. Now the only way the former differed from the latter is that their variables possessed some symmetrically arranged letters in common with the standards. Otherwise the conditions were identical: case for case the setting was identical, the norm was identical, the observers were the same and so was the time of exposure. Only one circumstance is different, namely, in one instance the variables bear no resemblance to the standards, in the other they do. Therefore, this resemblance must be the cause of the diminution of cognitive ability.

SERIES III

In order to obviate the necessity of performing two parallel tests—with and without similar words—it was deemed advisable to have the critical words within the same series vary uniformly in degree of similarity. But how is this similarity to be deter-

mined? One way is to resort to the judgments of competent persons, as Thorndike did in the construction of his handwriting scale.³² This was done in another connection, as we shall see in due time, but in these preliminary experiments I decided to rate each pair of critical words on the basis of the number of symmetrically arranged identical letters which they possessed in common. If the scale of similarity thus established has any objective validity, then it ought to be made manifest by the recognitive ability of the subjects; i.e. the sum of all correct judgments rendered for the words of lowest similarity, ought to be higher than the sum of all correct judgments rendered for the words next higher in the scale of resemblance. The inverse relation should always hold. This is precisely the result obtained in this series as the following table shows:

TABLE I
6-word Groups Exposed for 3 Seconds.

No. Com. Let.	0	1	2	4	5	6
% C.	100	100	91	83	83	50
No. Judgments	8	12	12	12	18	8
% P. K.	100	90	90	100	100	75

The figures 0, 1, 2, 4, etc., in the first horizontal line indicate the number of symmetrically arranged identical letters that the critical words possessed in common. The next line shows the per cent correct recognition (% C) when such words were substituted for each other. The third horizontal line gives the number of judgments upon which the percentage is calculated. The fourth line (% P.K.) shows the per cent of perfect knowledge regarding the substitution, i.e. it shows the per cent of times that the observer was able to tell what word had been displaced every time he recognized a new word.

At this point I wish to forestall a certain objection that might be made. It might be said that the inverse relation shown to exist between degree of similarity and discriminative ability is inconclusive, because the separate percentages are based on altogether too few judgments. This objection would bear weight if it were not for the fact that in spite of the fewness of our cases the odds against such an alignment of % C as shown in

³² Handwriting, Teachers' College Records, Vol. XI, No. 2.

Table 1, occurring by mere chance, are very great. If I had attempted to find out how 6-lettered, bisyllabic words which have two symmetrically arranged letters in common, let us say, affect recognition, and, on the basis of only 12 judgments, declared that such words can be recognized 91 per cent of the times, or that their coefficient of confusion is 9%, then the objection would be valid. Because that would be like trying to determine the percentage of a metal in a given alloy, which contains four or five kinds of metals, by making only one analysis. That would not be right. The proper way for the chemist to proceed in such a case is to make two or three analyses for the required metal, and if his results agree each time or are approximately close to one another, he is sure of their validity. That is one way of obtaining certainty. But the chemist may employ another method. Instead of making three or four analyses for the required metal, he may make one analysis for each of all the constituents of the alloy, and if the percentages of the different metals yield a total of 100%, then he is just as sure of the correctness of each separate result as he would have been if he had made three or four tests for one metal alone.³³

Likewise with our problem. Suppose we state it in this form: Given five pairs of bisyllabic words which differ from one another uniformly in the number of symmetrically arranged letters that they possess in common, what is the probability that these words will produce confusion in the recognitive process in a uniform inverse relation on the basis of chance? Our answer is this: Suppose these critical words fall into five distinct groups determined by their critical letters, then the chance that words of a definite degree of similarity will be recognized the greatest number of times is $\frac{1}{5}$, because they might have been recognized next to the greatest number of times, or third to the greatest number of times, or fourth or the least number of times; and the same is true of words of any other amount

³³We assume that the several methods of analysis for the several metals are equally valid and equally easy of application. Actually this is seldom the case, but the practical difficulties do not militate against the logic of the

of similarity. Therefore, the probability that these words which differ from one another in five distinct ways will be uniformly discriminated in an inverse manner by mere chance is $(\frac{1}{5})^5 = \frac{1}{3125}$. Or the odds against such chance occurrence, with the minimum number of judgments necessary for calculation of percentages, are 3124 to 1. It is highly improbable, therefore, that the results in Table 1 would be due to chance. Hence, there must be a causal relation between degree of similarity and ideational recognition.

SERIES IV

In this series I attempted to find out the effect of transposing words within the same group—not displacing them. Six-word groups were employed, and the time of exposure was reduced to 2 seconds.

TABLE 2

Per Cent Correct Recognition when two Words were Interchanged within Groups of 6 words Exposed 2 Seconds.

No. Com. Let.	1	2	5
% C.	63	61	42
No. Judgments	12	30	12
% P. K.	100	87	88

My next step was to use larger groups exposed for longer periods. I commenced with 10-word groups exposed for 10 seconds. A comparative test was made among the following factors: (1) substitution of words of varying degrees of similarity; (2) transposition of two dissimilar words within the same group; (3) substitution of words of internal similarity. The environment or setting was new with each pair of critical words.

Table 3 shows the results of the first factor. It is apparent that the conditions of the experiment were not ideal. If they had been, we should have obtained more than 80% C. when the critical words had no letters, or O letters in common, and we should also have obtained more than 84% C. when the critical words were identical throughout. Whether the disturbing influence was due to the number of words exposed or to the length of exposure is difficult to say. But though the con-

ditions under consideration permitted about 18% error, the general results are by no means affected, for this amount of error would have distributed itself over all the words in the series, i.e. it would have entered into the words of each degree of similarity in definite proportion.⁸⁴ Hence, the figures in Table 3, which show the inverse relation between degree of similarity and recognitive ability, are just as conclusive as they would have been if the conditions of the experiment had been ideal.

TABLE 3

Per Cent Correct Recognition for 10-word Groups Exposed 10 Seconds.

No. Com. Let.....	0	1	2	3	4	5	6	Identical
% C.	80	..	90	76	74	54.4	46	84
No. Judgments	30	..	12	12	30	30	18	18
% P. K.....	63	..	100	43	93	93	80	

On transposing two totally dissimilar words within the same group, the change was noted in 62.6% of the cases. This calculation is based on 78 judgments. Obviously it is just as difficult to recognize the interchange of two words within a 10-word group as it is to discriminate one 6-lettered word from another when both have 4 or 5 symmetrically arranged identical letters in common.

We turn now to a consideration of the third factor—the discrimination of words of internal similarity. The material used in this instance consisted of 14 pairs of synonyms of two syllables, selected at random from Webster's International Dictionary. Each substitution was judged by five subjects⁸⁵ and out of a total of 75 trials, 63.7% were successful. It would appear then that the substitution of one synonym for another is just as difficult to detect as the interchange of two dissimilar words within the same group, or, as the substitution of one word for another both of which have between 4 and 5 identical letters in common.

But I refused to remain satisfied with this "average" result, and the chief reason is that there was considerable difference in the % C. of some of these synonym-substitutions. On asking myself what this might be due to, I could find no other viable

⁸⁴ This problem is discussed mathematically in the Appendix.
⁸⁵ Comparison of the results with those of the other experiments.

answer but that there might be a difference among the synonyms in degree of propinquity.

I had no objective standard whereby to measure the similarity of the synonyms. At first I hoped to find some scale of measurement by noting the serial order in the dictionary, in which one word appeared as a synonym of the other; but this order was reciprocal for only two of the fourteen pairs of words chosen. I therefore decided to classify the nearness of the synonyms by resorting to a large number of judgments. I established three classes or orders; into the first went those synonyms that were closest in similar meaning; into the second, the next closest; and into the third, the least closest.

The 14 pairs of synonyms were printed in two vertical columns. The list was given to each of 12 persons, with the instructions to judge the words in pairs on the basis of likeness in meaning, and to distribute them into the three groups mentioned above. When each individual had finished judging he was engaged in conversation for two or three minutes so as to eliminate from his memory all recollection of the values rendered. Then the same list, with the columns reversed was handed to him for a second valuation. This interval was not too short, because all the men declared that on going over the second list they did not remember any of the previous judgments, except the one rendered for the first pair of words.

The twelve men who judged the synonyms for me in this way were graduate students. Five of them were specializing in English, the other seven were men of high intellectual ability. From these twelve men I obtained 24 judgments for each pair of words. If we number these pairs 1, 2, 3, 14, and represent the classes into which they were placed by the Roman numbers I, II, III, we get the following table.

TABLE 4
Degree of Propinquity of Synonyms and Frequencies for Each Pair of Synonyms

Class	1	2	3	4	5	6	7	8	9	10	11	12	13	14
I	7	5	4	12	13	7	0	10	1	4	0	1	6	0
II	6	10	5	12	10	8	13	6	8		8	14	10	7
III	1	9	15	0	1	0	11	8	15		16	9	8	17
Av.	1.2	2.1	2.5	1.5	1.5	1.1	2.5	1.0	2.1	6	2.7	2.3	2.1	2.6

The manner in which most of the 12 subjects estimated the synonyms was to see how each member of a given pair would function in the same context. If the words could be interchanged in a given sentence without violating good usage, they were put into class one; if not they were put into one of the other classes. This is the reason why some of the words were put into a different class on the second reading from the one to which they were assigned on the first. Thus, one of my subjects gives the following reason for changing his judgment with the words "acquire-obtain."

"When I looked at the words in their first order, "acquire-obtain," I associated a noun with the first word so as to make sense, I said, "acquire-fame." Then I replaced "acquire" by "obtain" to see if it would make as good sense, and when I saw that "obtain fame" did not sound well, I considered the words to be unfit for the first class, and so put them into the second. But when I looked at the words in the reverse order—"obtain-acquire," I associated the first with the word "wealth," and when I replaced the first by the second and saw that "obtain wealth" and "acquire wealth" hang together equally well, I put the words into the first class."

Some of my judges being more critical than others, would try five or six different settings before giving their final judgment.

Having classified our synonyms on the basis of internal similarity, we are in a position to see how they influenced the processes of recognition. This result is shown in Table 5. The figures in the first column represent the order in which the synonyms were judged for their similarity, they correspond, of course, to the numbers 1, 2, 3, 14 of Table 4. The second vertical column contains the synonyms themselves, those under N being presented on the first exposure in the recognition tests, and those under V being substituted for the N's on the second exposure. The third column contains the similarity value calculated on the basis of 24 judgments. Column four shows the per cent correct recognition when the substitution was made. The other two columns explain themselves.

TABLE 5

Degree of similarity and per cent correct recognition when synonyms are substituted in 10-word groups exposed for 10 seconds.

Class	Word No.	V	Substituted for	N.	Degree ^a of Sim.	% C.	Av. Sim.	Av. % C.
I	1	tumult		uproar	1.3	66	1.5	54
	4	protect		defend	1.5	70		
	5	obtain		acquire	1.5	10		
	10	usage		custom	1.6	70		
II	8	compute		reckon	1.9	42	2.1	61
	2	maxim		axiom	2.1	33		
	6	afraid		timid	2.1	90		
	13	except		unless	2.1	80		
III	12	provoke		excite	2.3	83	2.5	72
	3	ordain		appoint	2.5	100		
	7	outrage		insult	2.5	80		
	9	disguise		conceal	2.6	50		
	14	busy		active	2.6	60		
	11	correct		improve	2.7	58		

The foregoing figures, especially those in the last two columns, again point to the fact that ideational recognition varies inversely as ideational similarity.

It may be interesting to ask just how one similar idea replaces another in consciousness and forces its own acceptance in place of the other, like some carefully disguised pretender? When the word "depend," let us say, is exposed among 9 other words for 10 seconds, the subject gets an ideational impression from it, which he either succeeds or does not succeed in associating with other ideas, and which he either does or does not carry in memory during the 20-second interval between the first and second exposure. As a rule my observers seldom carried over, from the first to the second exposure, more than 4 or 5 words, by rote memory. Not knowing what word would be replaced or if any replacement would occur at all, he looks at the group on the second exposure in a searching manner. Each of the repeated words, i.e. the words constituting the milieu, seems to "fit in." Finally he comes to the substituted word "protect". Now somewhere in the margin of his consciousness is the idea, not the word, but the idea "defend." Well, "protect" seems to express that idea so perfectly that he accepts it as the word which had induced the idea in the very beginning. Or suppose

^a The lower numbers stand for closest similarity.

the sight of the substituted word "protect" calls the word "defend" to the foreground of consciousness, or better still, suppose the observer even carries over "defend" from the first to the second exposure; yet nevertheless, because "protect" is so close to it in meaning, he will begin to hesitate, wondering whether "defend" did not creep into consciousness surreptitiously, or suddenly come up by association, and he will oscillate between the two words, often declaring that "protect" is the word he saw on the first exposure because "it fits in." Indeed, this was an expression frequently used in describing a word that was recognized as the "same"—"it fits into consciousness," or, "it fits in with the rest," or, "it arouses the same idea," or, "it fits in with the image I had of it," or finally, "the idea produced by this word fits in with the state of consciousness that is reproduced by the other words."

If we now bring together the results of these preliminary experiments, we note the following facts:

(1) Under the given conditions of the experiment the transposition of two dissimilar words within the same group is approximately equivalent to the substitution of one 6-lettered word for another, both of which have 4 or 5 symmetrically arranged identical letters in common.

(2) The substitution of one synonym for another of the first order is equivalent to the substitution of one 6-lettered word for another, both of which have 5 symmetrically arranged identical letters in common.

(3) The substitution of a synonym of the second order for another is equivalent to the substitution of 6-lettered words which have between 4 and 5 letters in common.

(4) The substitution of a synonym of the third order for another is equivalent to the substitution of 6-lettered words which have 4 letters in common.

(5) There is an inverse relation between degree of similarity and recognitive ability.

INDIVIDUAL DIFFERENCES

The individual differences among the observers can best be shown by the per cent distribution of their judgments. The

following table gives the results with respect to the absolutely correct (+), the doubtful (\pm), and incorrect (—) recognitions.

TABLE 6
Per cent Distribution of Judgments

Subjects	+	\pm	—
An.	51	13	36
Wi.	54	6	40
El.	64	9	27
Gi.	69	11	20
Se.	69	15	16
To.	74	9	17
Av.	63.5	10.5	26

PART II

THE QUANTIFICATION OF SIMILARITY AND RECOGNITION

The encouraging results obtained from the foregoing method of classifying our material—a material which at first sight would appear to lend itself least of all to measurement—justified the extension of this process in a more refined manner. If words that differ from one another in the number of commonly possessed symmetrically arranged letters, have their specific influence on recognitive ability, why can we not measure the similarity of these words in terms of per cent, and then see what relation exists between any given per cent. of verbal symmetry and recognition? This is our next task.

Peters in his experiment on Association by Similarity¹ made an attempt to measure verbal similarity in terms of per cent. But he used nonsense syllables, hence his task, which consisted merely in taking the mathematical ratio of the repeated consonants or sounds in two such words to the total number of consonants or sounds in those words, was much simpler than ours, inasmuch as he did not have the meaning element to contend with.

Thus he declares, "Roughly speaking, one might say that the presented syllable in which one out of three sounds is changed, shows a difference of 33% from its prototype."² By increasing the number of letters or sounds in his words he was able to make finer changes still. For example, the change from "gafum" to "mafum" = 20% difference; from "kugös" to "fulös" = 40% difference; and from "gaafes" to "raaful" = 60% difference—there being changes of 1, 2, 3 sounds respectively in a total of 5.

The same author also found that whether a change is made at the beginning, middle, or end of a word the effect on

¹Über Ähnlichkeitsassoziation *Zeitsch. f. Psych.* Vol. 1, 1901, p. 105

creasing similarity. It was my aim to build a 10-unit scale, but I could not find a sufficient number of words of the proper length and homogeneity to enable me to do this.

The 40 pairs of words were therefore divided into 8 equal groups on the basis of the difference above mentioned. This measurement of the words was, of course, wholly arbitrary. Whether the mathematical values assigned to them expressed their actual similarity I did not know. So it was deemed advisable to make a comparative test by having the words judged for similarity by a large number of individuals in the same way as the synonyms mentioned on page 23 were judged.

The words were typewritten in pairs horizontally, thus forming two vertical columns, and handed to each of 35 university men and women with the request that they estimate their similarity in terms of percentage. The average and mean variation of these 35 judgments were then calculated with respect to each pair of words, and said average was denominated as the psychological similarity (P.S.) in contradistinction to their mathematical similarity (M.S.)

The following list contains a representative word from each of the eight sub-groups. The numbers preceding the words show the order in which the latter were judged. Those under N were the "norms" in the recognition tests, to be mentioned presently, those under V were the variables.

Order of presentation both for valuation and recognition.		Comparative Similarity		
N	V	% P.S.	M.V.	% M.S.
2 CULLENDER	SOCIETY	0	0	0
4 LIBRETTO	LINOTYPE	25	16	25
7 SEDULOUS	SEDITION	45	19	37
14 ILLUSION	ILLUMINE	52	13	50
8 MUTINOUS	MUTINEER	73	15	62
11 DISPOSAL	DISPOSER	77	9	75
13 SENSIBLE	SENSIBLY	86	7	87
5 HOLINESS	HOLINESS	100	0	100

The following table contains the results of the psychological judgments rendered for each pair of words. The numbers in heavy type at the head of each vertical column represent the mathematical similarity (% M.S.) of the words within that column. The figures directly underneath represent the psy-

chological values assigned to each pair of words which are indicated by the small numbers to the left of the column, under W.

TABLE 7

Per Cent Psychological Similarity of Each Pair of Words Based on the Judgments of 35 Individuals.

% M.S. = 0		25	37	50	62	75	87	100
W	W	W	W	W	W	W	W	W
2 0	4 25	7 45	6 43	8 73	11 77	3 81	5 100	
9 0	15 40	16 30	14 52	18 38	20 77	13 86	12 100	
17 0	27 20	25 35	21 66	24 82	26 81	22 86	19 100	
29 0	35 22	32 25	31 40	28 46	33 85	44 73	30 100	
39 0	42 32	37 23	43 37	36 75	40 70	46 84	38 100	
Av. % P.S.=0	28	32	48	63	78	82	100	

The average psychological similarity (Av.% P.S.) of the words within any group corresponds sufficiently close to the mathematical value of the same group to justify the method of classification employed.

We have now ideational material that is uniformly graded—conceptualized stimuli that differ from one another in absolute units. Our next step was to find out what influence these measured stimuli have on the recognitive process. To this end each of the critical words was typewritten in capital letters on 3 x 5 in. plain filing cards, together with 7 other words of the same length called “fillers.” These cards came in pairs, as the following figures show. The position of the critical words varied with different pairs, but was kept constant with each pair.

There were three kinds of word lists: (1) Those comprising the critical words that varied between 0 and 87% S inclusive. In this case the setting was identical with the two members of each pair, but differed from pair to pair. (2) Those that were 100% S with the exposure made in the same setting both times. (3) Words that were 100% S but exposed in totally dissimilar settings the first and second times. Here it was my desire to see what effect the perception of the same thing in a new environment would have on recognition. We shall note, later on, that this last factor has considerable significance for theoretical as well as for applied psychology.

SAMPLES OF WORD-CARDS

FIG. I

45 A: 0%S

ENERGIZE	EXAMPLE
FOLLOWER	GERMINAL
HARDENED	IMPOSTER
INTERVAL	LIQUIFY

45 B: 0%S

ENERGIZE	EXAMPLE
FOLLOWER	ROADSIDE
HARDENED	IMPOSTER
INTERVAL	LIQUIFY

FIG. II

11 A: 75%S

DIFFICULT	ASSISTANT
REGISTER	INSOLENT
LIBERATE	DISPOSAL
ADVOCATE	GASOLINE

11 B: 75%S

DIFFICULT	ASSISTANT
REGISTER	INSOLENT
LIBERATE	DISPOSER
ADVOCATE	GASOLINE

FIG. III

5 A: 100%*
in N.E.

RECORDER	PLAINTIFF
RECENTLY	REGISTRY
REGIMENT	SACRIFICE
PARABLE	REMEDY

5 B: 100%S
in N.E.

SCIENTIST	MOBILIZE
MODESTY	OFFERING
MODERATE	OFFICIAL
PARABLE	PANOPLY

* N. E. = new environment; 'A' was the standard card, 'B' the variable.

These typewritten cards were then taken into the laboratory, and each of them exposed for 4 seconds. The interval between the first and second exposure of each pair was 20 seconds; that between any two pairs varied from 2 to 4 minutes, depending on the time it took to record the introspection. The order of presentation was the same for all observers.

The exposure apparatus was similar to the one already described, only much simpler. It consisted of a vertical cardboard screen which separated observer from experimenter completely. A drop shutter, controlled by a string running over a pulley, moved up and down, opening and closing a 6 x 6 inch window. Within 3 inches of this opening lay the printed card on a slightly inclined black background. The distance of the card from the observer's eyes, when he sat in a comfortable position, was about 12 inches. The time that the shutter occupied in opening or closing the window was approximately $\frac{1}{4}$ sec.

For this set of experiments I had four subjects—one woman and three men. All of them were trained psychologists and

For the purpose of this experiment, the subjects were trained psychologists and were not aware of the purpose of the experiment on this occasion.

thoroughly experienced in the kind of observation needed in this investigation, having been my subjects in the recognition of picture post cards—of which more hereafter—for three months previous to these tests.

I gave them the following instructions: "I am going to expose a group of 8 words for 4 sec. I want you to look at all the words carefully. Try to assimilate them. Yet do not dwell on one word at the expense of others, but try to distribute your attention over all of them equally. After the first exposure there will intervene a period of 20 sec. during which you must try to keep your mind fixed on the experiment. After this interval the shutter will go up and again remain open 4 seconds. Look at the second card and see if it differs from the first. In the second exposure there will exist four possibilities; either all the words may be changed,⁵ or all but one, or none may be changed, or only one. See which of these events occurs. And if you discover a change, tell me what it is, i.e. tell me not only what word you think is new, but also what word you think it has displaced. Note everything that goes on in your consciousness."

It should be obvious to the reader that I used words of 0% S and "identical" groups as controls in this experiment. For before I could establish a relation between degree of similarity and recognitive ability, it was necessary to find those conditions which (with the exposure of the largest number of items for the shortest possible time), would, on the one hand, yield 100 per cent correct discriminations when an object of 0% S was substituted, and, on the other hand, yield 100 per cent. correct recognitions when an object of 100% S was exposed, i.e. when no change was made at all.

The present condition—8 objects exposed for 4 seconds—did not give such ideal results; but as the aim of the investigation is to find that combination of t and N (t = time of exposure,

⁵ Actually I did not have cards on which all the words were changed, but it is advisable to have such.

N = number of objects exposed), that would give those kinds of results, we shall return to this problem presently.

Table 8 contains the results of the recognition tests carried on with these standardized words. Comparing % C with % S we note that as similarity increases, recognitive ability decreases. This may be called the first law of recognition. The only criticism that might be made is that I did not have enough observers in this set of experiments. But the results harmonize so well with those obtained from seven observers using similar material,⁶ as well as with those obtained from ten subjects in the picture post card tests,⁷ that they cannot be due to chance.

TABLE 8

Relation between Degree of Similarity and Recognitive Ability given in terms of Per Cent, when Words are Exposed in Groups of 8 for 4 Seconds.

% M.S.	⁰ W %C		²⁵ W %C		³⁷ W %C		⁵⁰ W %C		⁶² W %C		⁷⁵ W %C		⁸⁷ W %C		¹⁰⁰ W %C		¹⁰⁰ N.E. W %C	
2	87		4	100	7	75	6	75	8	37	11	37	3	75	10	50	5	50
9	87		15	87	16	50	14	13	18	38	20	25	13	0	23	100	12	100
17	87		27	83	25	87	20	50	24	[0]	26	13	22	13	34	100	19	50
29	100		30	100	32	75	31	50	28	38	33	0	44	37	41	75	30	75
39	100		42	50	37	87	43	75	36	87	40	[100]	46	0	48	88	38	63
45	87																47	100
Av. % C	= 91		84		75		53		40		35		25		83		73	
Corrected results	100		90		80		56		42		35		25		0			

¹ These were the identical groups presented in both exposures.

² In the second exposure only one of the words from the previous list appeared in a totally new environment. For meaning of "corrected results" see discussion that follows.

Let us now analyse the results of table 8. In the first place, it will be noticed that when a substitution of 0% S. was made, only 91% of the judgments were correct, i.e. the observers failed to recognize such substitution in 9% of the cases. On the other hand, when the identical group was re-exposed, they declared in 17% of the cases that one word was new. We see, then, that in one case there is a tendency of 9% to say that a thing is the same when it is absolutely different; in the other case there is a tendency of 17% to say that one of 8 things is different when all are identical. How shall we interpret the results for the intermediate values of S in the light of these

⁶ See p. 24.

⁷ See p. 26.

errors? As we have said on a previous occasion, these errors, if such we may call them, do not affect our general conclusion, namely, that there exists a simple inverse relation between degree of similarity and recognitive ability, because they enter proportionately into the judgments rendered for each unit of the scale. But inasmuch as it is our desire to discover the exact relation between any particular amount of similarity and recognitive ability, we must see in *what* proportion these errors distribute themselves throughout the entire scale.

We note that where the second exposures were identical with the first, the subjects declared that there was a change, 17% of the times. Does this mean, then, that the judgments of "change" rendered in those cases where a change actually occurred are 17% too high? No. For suppose this index to represent the tendency to make chance judgments of "change" with respect to any one of 8 identical objects, then the probability that such a chance judgment could have been rendered for the one particular word which had actually been changed among 8, is $\frac{1}{8} \times 17\% = 2.1\%$. This is assuming that the particular word in question is 100% S. But we cannot say that the same amount of error would enter into the judgments of change rendered for objects of less than 100% S. For such objects would possess a certain amount of dissimilarity which would have a tendency of its own to elicit the judgment "change." And in proportion as such objects were removed from 100% S, in that proportion would that inherent tendency increase, while the tendency to say "change" on the basis of illusion or mere chance would diminish. Moreover, it has been shown conclusively that it is much easier to recognize a new object as different than a previously experienced object as the same.⁸

Now we have said that there is a simple inverse relation between degree of similarity and recognition. Therefore, inasmuch as 17% of error was made with respect to 8 words of 100% S, the amount of error that would have been made with respect to words of 87% S is $0.87 \times 0.17 = 0.14$, i.e. 14%. But since only one word was changed among 8, the probability of

⁸ Meumann, *op. cit.*

saying that that particular one was changed, whether the change was noticed or not, would be $.78 \times .17 \times \frac{1}{8} = 1.7\%$. And so on down the scale, clear up to the point where the probability of declaring a word of 0% S changed, on the basis of mere chance, would be $0 \times .17 \times \frac{1}{8} = 0$.

Hence, we may say in general, let X be the per cent of wrong judgments, i.e. judgments of change when no objective change occurred, and let S be any particular unit on the similarity scale, and finally, let A be the number of items changed and B the total number of items exposed. Then our formula for the chance correct judgments is

$$SX \cdot \frac{A}{B} = K'$$

which value must be *subtracted* from % C corresponding to the %S in the formula.

On the other hand, when we turn to the zero end of the scale, we find that our observers failed to recognize 9% of the substitutions. Evidently the conditions of the experiment were such that that number of objects of 0% S or 100% D, (D = dissimilarity), escaped the attention of the subjects. Is it not likely, then, that the same thing happened with respect to the other units of the scale? Again we answer no. For we have no right to assume that 9% of the substitutions of 87% S also escaped recognition owing to the conditions of the experiment, because such words contained a certain amount of identity—87%—which would have a tendency of its own to elicit the judgment “the same.” Likewise with words of every value of S.

Suppose we let 0% S = 100% D, 25 % S = 75% D, and in general $x\% S = (100-x)\% D$,⁹ then inasmuch as the subjects failed to recognize 9% of the changes in a 100% D substitution, in a 75% D substitution they would fail to recognize $.75 \times .09 = 7\%$ of the changes. And so in general, the number of substitutions that our observers would have recognized, if the conditions of the experiment had been ideal, would have been

⁹ This relation is the same as that between the number of substitutions and the number of dissimilarities in a substitution.

in addition to those already recorded, $(100-S) \times .09$ for any value of S . This value must be *added* to % C in each case. If now we let Y stand for the per cent of the cases in which the observers failed to recognize the substitution of a totally dissimilar object, our Correction Formula becomes

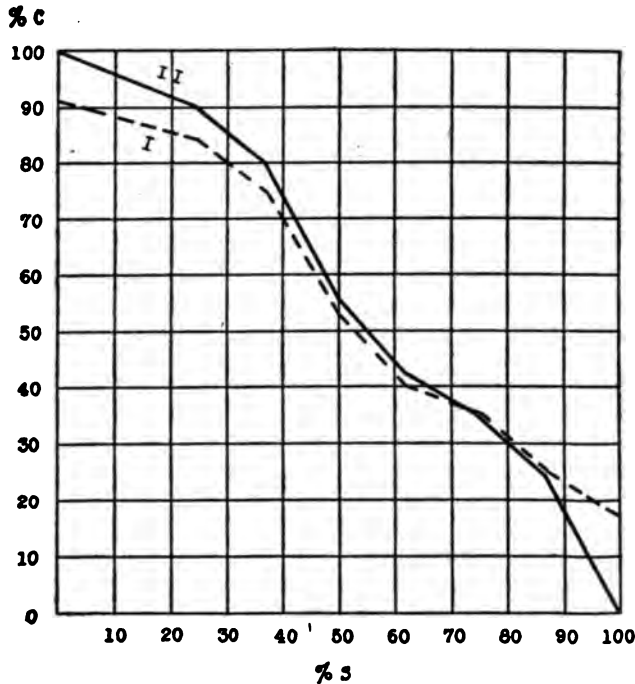
$$(100-S)Y - SX \cdot \frac{A}{B} = K$$

which value, with its algebraic sign, should be added to % C corresponding to the S in the formula. If we apply this formula to the Av. % C's in Table 8, we obtain the "corrected results" which appear in the last horizontal line of that table.

By arranging our stimuli along the abscissa and the recognition judgments along the ordinate, we obtain the curves in Plate I. Curve I is drawn on the basis of the uncorrected judgments, curve II represents the results after the Correction

PLATE I

Per Cent Correct Recognition when Tri-syllabic Words are Exposed in Groups of 8 for 4 Seconds.



Formula has been applied, i.e. as they would have appeared if the conditions of the experiment had been ideal. In reading the curves it should be understood that % C means per cent judgments of *change*, which judgments were correct in all cases except where S had a value of 100%.

NOTE—The full advantage of the Correction Formula will become apparent in the last part of this paper—the part on applied psychology. Suffice it to say at this point that it serves to establish a relation between degree of similarity and recognitive ability in terms of the 0- and 100% S ends of the scale, and in doing so it makes that relation practically absolute.

ANALYSIS OF RESULTS

How were the judgments distributed and why did some of them vary so largely with words of the same mathematical value and approximately of the same psychological value? For example, W(33) which equals 75% S yields 0% C, whereas W(40) also having a value of 75% S yields 100% C.

The first part of the question is answered by Table 9. If + = a correct judgment respecting a change or no change, according as one was or was not made; and if — = a wrong judgment, i.e. failure to note a change when one occurred or asserting that a change took place when none did; and finally, if ± = a doubtful judgment—one that varied between 20 and 79% certainty, then we see that the judgments were distributed in the following manner:

TABLE 9
Distribution of Judgments

% S =	0	25	37	50	62	75	87	100
+ =	19	12	11	7	6	6	3	14
± =	4	6	7	6	4	2	4	5
— =	—	—	1	6	10	12	13	1

Table 9 shows very strikingly how, with increasing similarity, the judgments pass from positive correctness through doubt to definite error. When the stimuli were totally dissimilar, none of them were definitely mistaken for each other. About 17% of them were doubtful. When they reached 25% S, 33 per cent of them became doubtful, but as yet none were mistaken. It is only when the stimuli become 37% S that a positive mistake occurs, and this happens once out of 19 trials, but the number of doubtful judgments decreases to 7 while the correct

ones diminish correspondingly. When the stimuli are 50% S, the judgments are distributed almost equally over all possibilities: 36% are correct, 32% are doubtful, and 32% wrong. Theory would call for 33% in each of the cases. That our distribution should come so close to the requirements of probability, argues most forcibly for the integrity of the similarity scale as a whole, as well as for the method employed in its construction.

As the similarity still increases the quality of the judgments continues to change in a definite way—the correct ones diminish, the incorrect ones increase, while the doubtful ones also decline because there is not so much possibility now for vacillation. Either the substitution is noticed or it deceives the observer completely. This relation occurs with increasing emphasis in the 75% S and 87% S units. Finally, when we come to the case where the second stimulus is identical with the first—100% S—we have a return to the conditions that obtained when the stimuli were totally dissimilar.

In trying to answer the second part of our question: Why do the recognitive judgments vary so widely with respect to words of approximately the same value? we are brought face to face with the burden of our thesis. But we have to analyse many factors that enter into the process of recognition before we can answer that question. Believing with Wundt that mental response is a function of two independent variables—the external stimulus, on the one hand, and the internal resistance, on the other—we are going to examine some introspective data with reference to the words that elicited them.

We have already stated that the recognitive process which we are examining, especially with the word material, is slightly tinged with immediate memory. That is to say, in some cases the observer was able to retain during the interval several words from the first exposure. Yet these memorized words played but a small rôle in the process of recognition. For in the first place, the average number of words carried in memory varied from 3 to 5, so that there was an equal chance that the observer did not carry the critical word. Secondly, introspection shows

that though the words carried in memory are identified with more certainty, yet any one of these memorized words can be easily replaced in consciousness by a perceived word which closely resembles it, i.e. the perception is frequently stronger than the memory. Finally, the fact that some of the words were retained in memory, only served to reinstate the recognitive process as it functions in daily life. For it is not only things which loom up suddenly and unexpectedly that awake the process of recognition. On the contrary, in the majority of cases we have to recognize persons, things and places of which we already have a more or less clear idea as to how they appear.

Our experiments enable us to analyse three mental factors: (1) The recognition of totally different things. (2) The errors that arise from the attempt to discriminate similar things. (3) The recognition of identical objects. The data for the examination of this last factor must be gathered from those cases where only one word was repeated in a new environment. Here we shall obtain a contrast between the feeling of familiarity produced by the repeated word and the feeling of strangeness aroused by the new setting,

Suppose we start with the first factor mentioned. One of the primary things that strikes our attention when a totally dissimilar word or object is substituted for another within the same milieu, is that in a large number of cases the perception of the second item knocks the idea or memory image of the first out of mind. But although the standard is lost from consciousness, the variable is none the less recognized as new with great positiveness and accuracy. Thus introspection reads:¹⁰

2:0%S

1	5
2	6
3	7
4	8

8 A = cullender, 8 B = society.

Ha. + (?): "Society" is new, 80 per cent sure.
I knew what 8 A was, but the impression of 8 B knocked the other one out of my mind.

Bu. + (—): I am sure 8 B is new. I do not remember what was there before.

FIG. IV

¹⁰ The rectangular figures represent the printed cards, the numbers within indicate the order in which the numbered letters appearing in intro

The formation of associations, however, may be helpful in the retention of the original impression.

17: 0% S. 6 A = Elliott, 6 B = Manual.

Ha. + (+): 6 B is new, 100% sure. I had associations with 6 A; it became strongly impressed. I had images of 5, 6, 7, 8; these and the others, except 6 B, look familiar.

Ke. + (—): "Manual" seems strange, 80% certain that it is different. I don't know what was there before. Sometimes it seems to me as if I had an image of the other word, but I cannot say what it was. I carried 1, 5, 7, 8 during the interval in mental imagery.

Sometimes the physiological processes set up by a totally dissimilar stimulus are so different from those experienced immediately before, that the substitution will be recognized in terms of the new psychophysical states it arouses.

39: 0% S. 4 A = endoderm, 4 B = typebar.

Ha. + (—): 4 B is new, 100% sure because it felt new in visual, kinaesthetic and associative terms. It is a word that calls up associations promptly. I don't know what word it displaces.

Bu. + (—): 4 B is new, 100% sure. The others are familiar. The pronouncing of 4 B in inner speech produces a sort of shock due to its unusual rythm, i.e. it does not have the same number of syllables as the other words. If it had been present in the first exposure, this rhythmical peculiarity would have attracted my attention.

Ke. + (—): 4 B is new, 100% certain. I knew what was in its place before, having carried 1, 4, 7, 8 in memory, but I cannot recall it now. The sound of 4 B requires more tension in the vocal muscles than the others, therefore it stands out from them as strange.

Frequently the totally dissimilar word manifests itself by the disturbance it introduces into the psychic processes aroused.—
Thus:

45: 0% S, 6 A = roadside, 6 B = germinal, one observer de-
spection are used for abbreviation. Sign in () indicates whether displaced
word was remembered

clares, Ke. + (—): 6 B is new, 100% sure. I do not remember what was there before. I had images of 1, 3, 5, 7 and 8. 6 B does not seem to fit in with anything in my consciousness as the other words do. On seeing the first, vague associations were formed, but now this word—6 B seems to break those associations up. Moreover, it has a verbal and meaningful strangeness of its own.

According to this same observer, the newness of a totally dissimilar word consists in that it does not seem to take up any of the mental processes started by the first list as the other words in the second list do. Instead of that it sets up a new mental process peculiar to itself.

The exposure time—4 seconds—was just long enough to enable the subjects to go over the list of 8 words once or one and a half times. It happened only on two or three occasions out of 150 exposures that a subject failed to read all the words before the shutter went down. The shortness of the exposure prevented the memorization of the words, but it did not prevent their apperception. It is true that in some instances the words did not have rich meaning, i.e. the real concepts they stood for did not always rise to the center of consciousness; but the observer always got enough meaning from the words to know that they were intelligible. They aroused a definite state of consciousness and produced a fixed attitude. The short exposure simply prevented them from being completely developed in mental imagery and from being interlocked by associations.

METHODS OF RECOGNITION

There are three possible ways of recognizing an object under the conditions of our experiment. (1) Imagery of the objects seen is carried throughout the interval between the first and second exposure. These memory images are carried either in visual, verbal or auditory terms. In the mixed mental types the terms will vary with the nature of the material. (2) The memory images may vanish during the interval and be revived by the re-perception of the original object or merely by the perception of the position originally occupied by the missing object.

In this case the revival of imagery helps to strengthen recognition or makes the recognition of a new item more certain. (3) The recognitive consciousness may be a matter of feeling, or of kinaesthetic attitude or of adjustment, or of the revival of a mood. The perception, or, in this case, the meaning of the word fits right in with the existing attitude. It is not necessary to pause to get the meaning of the recognized word, but, as one of the observers remarked, "it fits right into consciousness as a plug fits into its socket."

The ability to recognize an object in a totally dissimilar setting from the one in which it was originally seen, proves conclusively that recognition is independent of conscious association. Nevertheless, the fact that a totally dissimilar stimulus can obliterate its predecessor, as we have already shown, whereas a somewhat similar stimulus can revive its predecessor, as we shall presently show, indicates that some kind of association—perhaps a subconscious association may occur in some cases of recognition.

The fundamental factor of the process of recognition is the feeling of familiarity. But how is that feeling related to its opposite—the feeling of strangeness? Do these two feelings neutralize each other by fusion or do they intensify each other by contrast? I had ready means at hand for finding that out. Hitherto I had made substitutions in the same surroundings in which the standards had been seen, and obtained certain values with each degree of similarity. Now what would happen if the same substitutions were made in a totally different environment, i.e. instead of exposing the variable word in the same field in which the standard had appeared, what if it were exposed in a different one? Would the observer be able to recognize the imitation or the identity, as the case may be, in this second instance as easily as in the first? If not, then it would mean that the feeling of strangeness aroused by the new setting tends to inhibit the feeling of familiarity, or better still, tends to swamp it by suffusing itself over the whole of consciousness. If, on the other hand, recognition would improve in this second case, then it would show that the feelings

of strangeness and familiarity enhance each other by contrast, as complementary colors do.

Experiments with both kinds of material—words and picture post cards, substantiated the former proposition, namely, that the feelings tend to fuse, that one of them gaining the upper hand in consciousness which is fed by the larger number of stimuli. If we glance at Table 8 we shall see how the figures bear out this statement. When all 8 words were repeated the second time the subjects recognized them as identical in 83% of the cases. In other words, there were 17% of errors. But when *one* identical word was repeated among 7 new ones the errors rose to 27%. Even if we apply our correction formula, in order to subtract the number of judgments "change" which were made as a result of the precipitant tendency to say change, the error still remains 25%. We are justified in saying, therefore, that if, under the conditions of our experiment, a stimulus A, originally given in a setting BCD, is subsequently repeated in a totally different setting EFG, the odds that it will not be recognized are 1:3, or the chance of its recognition is 75/100; whereas if it is repeated in the same setting the chance of its recognition is 98/100.¹¹

Let us now see how introspection supports our data.

19: 100% S¹¹
in N.E.

1	5
2	6
3	7
4	8

FIG. V

all strange. I had a feeling that this second list was a new assortment. The words suggested new ideas. *And this feeling of strangeness increased in momentum as I went on reading the words.*

Bu: \pm : 2 seems familiar. I had no image of it, but it struck me as familiar.

¹¹ The theory of this has been discussed on page 40.

¹² 100% of N.E. ...
Fig. 1

Ma:— : All seem strange. I got a general impression of strangeness.

30: 100% S
in N.E.

1	5
2	6
3	7
4	8

FIG. VI

4 = heritage.

Ha: + : Heritage is the same, 100% sure. The others are new. As soon as I saw that more than one were strange, I began to look for a familiar word. And I got a decided feeling of familiarity when I came to 4, but when I began to think about it the feeling diminished.

This last statement is important in its bearing on the process of recognition. It is common knowledge that the recognitive process takes time to develop. I found that in my experiments, as we shall have occasion to see. Moore also found it in his investigation on abstraction.¹⁸ But now when we get the complement of this and note that the process of recognition also atrophies with time, we have double proof that recognition is an affective process, since feelings alone take time to develop. There were several occasions on which my observers declared that a word looked more familiar the instant they saw it than it did a few seconds later.

It should not be supposed that the task of discovering an old item in a new environment is more difficult than that of discovering a new object in the old setting, because it takes more time to apperceive new objects than those already experienced. That is not so. On the contrary, when the surrounding words in the second exposure are new, the subjects run over them much faster than over the same list repeated. The reason is that it takes less time to see that a thing isn't than that it is. We should be in a sad plight if it took us as much time to discriminate as it does to recognize. I go into a store to purchase a tie; with one sweep of the eyes I eliminate the colors and patterns that I do not want. Of course, I don't know their texture nor any of their virtues. But that does not matter. I know all I care to know about them—they are not what I want. This life fact is substantiated in our experiments. Referring to group 30 subject Ke. says:

¹⁸ The Process of Abstraction, T. V. Moore, Univ. of Calif. Pubs., in Ps Vol 1, No. 2 p. 16.

"Heritage is the same; 100% certain. I had a lot of images during the interval. It was easy to solve this problem. On seeing the second card I discovered one word (4) of which I had an image during the interval. I looked for others, but finding none, I kept rushing over the list. *I did not stop to get the meaning of the words. I could see at a glance that they were not familiar ones. So I hastily dismissed them.* But when some or all the words are repeated, I have to read them carefully in order to see whether they are all the same or not. It is easier to distinguish what you do not want than to pick out what you do want."¹⁴

Eliminating, therefore, the only objection that could possibly be raised, we must conclude that the reason why it is so difficult to recognize an identical item in a new environment, is that the feeling of strangeness produced by the new setting, suffuses itself over the whole of consciousness and blots out the opposite feeling—that of familiarity, according to the principle of what in chemistry is called "mass reaction." The following case proves this.

38: 100% S
in N.E.

1	5
2	6
3	7
4	8

FIG. VII

7 = strategy.

Bu: —: All different, 90% certain. I had images of 3, 5, 6, 7, 8. When the shutter went up the second time I began to read column 2 downward rapidly and saw that each of the words was new. The feeling of strangeness kept growing on me and developing the judgment, "All different." I was on the look-out for a repeated word in column 1, but found none. (At this point I told the subject that "strategy" was the same, and I asked him why he failed to identify it. He replied), The reason why I did not recognize "strategy" is because I read too rapidly, I think. Probably, too, by the time I had read the first two words the feeling of strangeness had spread itself over "strategy" giving it also the coloring of newness.

¹⁴ Meumann in the article already referred to, finds practically the same thing.

ERRORS OF RECOGNITION

Error of recognition or inability to recognize, may be due to any one or to a combination of the following factors: (1) Lack of memory. (2) Incomplete perception of the standard stimulus when first presented. (3) Incomplete perception of the variable stimulus, mistaking it for N, the normal. This, I postulate, is due to the superiority of the ideo-motor force over the sensory-motor force. (4) Obliteration from memory of the original N impression by the perception of the V stimulus—due to the superiority of the sensory-motor force over the ideo-motor force. (5) The collision of the I. M. and S. M. forces with equal momenta resulting in a doubtful judgment. (6) The induction by the represented stimulus of a new association or a new mood by virtue of which said stimulus appears novel or strange. (7) The inability of the original stimulus to revive an association or an attitude induced by it when first presented.

Let us consider these factors in their order.

(1) Inability to recognize owing to lack of memory manifested itself in those cases where the whole of the second exposure was identical with the first. The subject would doubt a word among the 8 identical ones because he could not recall having seen it before. Thus, for groups 48 subject Ke. says, "I could not remember "oration" on seeing it the second time, therefore, I think it is new; 75% certain. I carried 5, 7 and 8, and I recognized these and the others, but not "oration". It does not look strange, but I simply cannot recall it."

What subject Ke. means, of course, is that the perception of the word "oration" did not revive the memory idea of having been seen before. This does not say, however, that recognition depends on memory altogether. It simply means that where the repeated stimulus fails to revive its particular mood, or where it fails to make a sufficiently deep impression on consciousness on its first presentation, it has to awake a pure memory idea if it is to be recognized at all. This memory idea is frequently colorless. The subject would often say that he

remembered the item in the same way that one remembers a fact or a date.

(2) Where it was a case of incompletely perceiving the object when first presented, the observer would either fill in the missing details or else he would suspend judgment on the second presentation of that item, and declare that he recognizes only so much of it as he had originally seen, but cannot say anything about the rest of it. These cases were more frequent with the picture post cards than with the words. The following case is characteristic.

Group 33: Subject Ke:—: No change. But I am puzzled. I did not have time to read the 6th word, "occupant". All I saw was that it began with "o" and ended with "nt;" so I thought during the interval that the word was "occident". But now I see that it is "occupant." I do not know what to say. I am sure the others are the same, and I think "occupant" was there before also.

(3) It will be remembered that the second exposure was the same length as the first—4 seconds. The reason for this is that even in the very act of recognition it was desired to duplicate the very conditions of life. Not only do we generally perceive things hurriedly, but in the rush and tear of life we have to pass judgment upon them more or less rapidly and under trying circumstances. Afterwards we may think about our reaction to the critical situation—be that reaction in the form of a discussion or an opinion or a purchase—as much as we please; but in actual life it is never given us to live through the same critical moment twice. Likewise in our experiment. The individual had to get the decisive impression from the object or its substitute in a limited time. He could, after the shutter went down, take all the time he desired to render his judgment. But that judgment would have to be based on an impression that was limited in duration, that occurred when attention was distributed, when there was a conflict between several motives, and when consciousness was flooded by a multitude of ideas.

The shortness of the exposure then permitted the study of this very important phase of human recognition, namely the in-

complete perception of the substituted item. It should be understood, however, that this is not the entire cause. On the contrary, it is only a contributive cause, the main one being the "push" that is given to the perceptual process by the ideo-motor force which had been induced by the standard stimulus. Hence, it follows that this kind of error can occur only where there is considerable objective similarity between the N and V stimuli. There was not a single instance of its occurrence with stimuli below 50% similarity.

How are the errors resulting from the incomplete perception of the variable stimulus caused? I show a person a list of 8 words, among which is a given word whose structure is ABCD. After a short interval I re-expose the same list with the given word replaced by another of slightly different structure—say ABCX. And the observer reads the second list, and says, "Yes, I find all the words there, including ABCD." Now this is not a case of merely false perception. It is not simply a matter of mistaking an X for a D. But it seems that the brain process set up by the first word, absorbs the second word owing to the common elements ABC. The extent to which this will happen is directly proportionate to the ratio of the common elements, ABC, to the variable elements, D and X. Before a man can see a bear in a bush he must have a strong idea of the bear, on the one hand; and the bush must have some resemblance to the bear, on the other. So with my subjects. They had a clear and distinct idea of what the original word was, and now they sense an item that begins to follow the same cerebral path pursued by the original stimulus. Before they are aware of it, however, this second impression is drawn off completely into the brain path developed by the standard stimulus. The occurrence of this event, therefore, depends on two factors; first, the ratio of the common elements, ABC, to the variable elements, D and X; and secondly, it depends on the strength of the original idea or the ideo-motor force. That this error is not due to the shortness of the exposure, but is rather a function of the % S and I. M. F., is manifested by the fact that it occurred only among

those stimuli that were over 50 per cent similar, with a frequency directly proportionate to increasing similarity.

Let us cite a few cases.

22: 87% S

1	5
2	6
3	7
4	8

3 A = sanctify.

3 B = sanctity.

Ha. —: No change, 100% certain. Visual and auditory images of 2, 5 and 3 A, but others fitted in so smoothly that I feel sure of them. I formed

FIG. VIII

an association between 2 (yeomanry) and 3 A. Such an association tends to make the constituents persist longer in memory during the interval. I found 2, 5 and 3 A on the second exposure.

Ma. —: All seem the same; 100% sure. The impression of familiarity that I received from those that I am positive of, seems to spread itself over the rest of the words.¹⁵ The ones I am sure of are 3 A, (sanctify), 4, 5, 6, 7. I think I looked at the whole of 3 A on the second exposure. (When told that it was 3 B (sanctity) and not 3 A, he said), Well, when I saw the second exposure I read the word as "sanctify" and it sounded familiar; so I took it for granted that it was a repetition of the word I had seen there before.

20: 75% S

1	5
2	6
3	7
4	8

5 A = rebuttal.

5 B = rebutter.

Bu. —: No change; 80% sure. I had images of 4, 5 A, 6, 7, 8, and I found all these on the second exposure. When the shutter went up the second

FIG. IX

time I looked at column 2 and found that all the words there were the same. That gave me a feeling of satisfaction. Then I went to column 1, and though I got a neutral feeling from the words within it, I nevertheless judged them to be the same. I went over the list once on the second exposure and saw each word distinctly.

Thus we see how the subject carrying in his mind the idea "sanctify" or "rebuttal" finds these ideas in the structurally similar words "sanctity" and "rebutter."

¹⁵ It is interesting to note in connection with this that Katzaroff (*La Recognition*, p. 57), also found that "the feeling of certainty is more or less contagious."

Of course, such phenomenon is a common occurrence, especially among authors who try to correct their own manuscript. Despite repeated readings, they continue to omit words or to overlook misspelling simply because they are so imbued with the subject-matter that they supply the missing items mentally, thus failing to supply them materially. And the more knowledge they have of their subject, the more errors in writing will they overlook. Likewise with my observers. If they had a clear memory of the standard word they did not pause to get a distinct perception of the variable. On the contrary, in such a case the bare perception of the initial syllable or two would so reinforce the memory idea of the original word as to make them think that this clearness had an external source, i.e. that the standard word was there before them. For reasons that I shall presently make clear, I call these kinds of errors errors of "is", meaning thereby that the variable *is* the normal; and I postulate that indirectly they are due to the degree of similarity between N and V and directly to the superiority of the ideo-motor force over the sensory-motor force.

(4) But this is not the greatest source of false recognition. On the contrary, errors of this kind are relatively small as compared with the errors that result from the superiority of the sensory-motor force over the ideo-motor force. An impression ABCD is given to a person and shortly afterwards another one ABCX, but instead of declaring that he still perceives ABCD, as in the previous case, he now asserts that ABCX is what he had experienced the first time also. These kinds of errors we shall call errors of "*was*". How is this phenomenon to be explained? It is not merely the obliteration from memory of the first impression by the second. For while such obliteration can and does take place, it does not follow that the second perception will be taken for the first, or that perception B will be taken for perception A simply because it has expelled the latter from memory. Let us cite a few instances.

Obliteration is a very common occurrence when the N and V stimuli are 0% S. The V comes along in the form of a perception and drives the memory image, or the pure idea of N so

completely out of mind that the observer has not the faintest recollection of what was there before, yet never will this variable be mistaken for the normal. It produces a mental process all its own, and thereby suffuses itself over consciousness so thoroughly that the other contents vanish sometimes temporarily, sometimes permanently. (See introspection for group 45, page 43.) But it is always recognized as new by virtue of its uniqueness. Not infrequently the subject would say that the 0% V struck him like a flash of light thrown into the eyes, and just as such a flash wipes everything out of sight, so does this V wipe out all knowledge of what was in its place. But it forces its newness on consciousness with an irresistible force.

Where similarity was almost zero, say 25% S, obliteration would also occur without diminution of recognition. That is, the difference between the V and N was still large enough for the former to produce a conscious shock of its own and at the same time to destroy the reverberations of the latter. The following instance is characteristic:

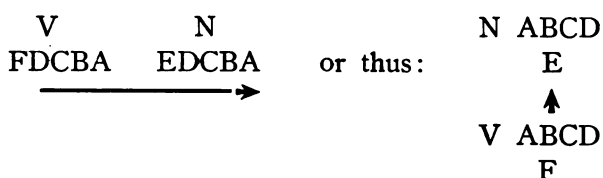
Group 37: 37% S: 7 A = politics, 7 B = polander.

Ke. + (—) 7 B is new; 100% sure. I cannot say what was there before. 7 B stands out both in meaning and imagery from all the other words. All the rest are familiar. I read the second list in the same order as the first. When I came to 7 B there was a sort of break or check; I had to stop to get both its meaning and sound. It is a feeling like that which one experiences when one is running down a hill at full speed and suddenly comes up against a fence. He is upset, but he knows what upset him.

The errors we are now considering are, therefore, not due to the obliteration alone, i.e. they are not the result of the relation between the S. M and I. M. forces alone, but they also depend upon the degree of similarity between the N and V stimuli. This is demonstrated by the fact that they rise in frequency with every increase in similarity. The following table bears this out.

		APR 10			
		Definitions Error			
		25	50	75	87
Error					
Error					

I trust no one will accuse me here of trying to maintain that we read words letter by letter. That this is not so has been sufficiently demonstrated by Cattell¹⁶ and others. But we do perceive words after they have set up certain cerebral processes. The exact nature of these processes is not our immediate concern; neither does it matter whether they are to be measured lengthwise or breadthwise. That is, we may conceive the V impression running along the single brain track produced by N in the following manner:



In either case this kind of false recognition involves three factors: (1) The V impression must run along part of the path of the N impression; (2) it must set up a brain process of its own in order to be perceived; and (3) it must destroy the cerebral reverberations of N in order to drive it out of memory. The first of these three factors depends on per cent S; the last depends on the relative strengths of the sensory- and ideo-motor forces. Table 11 shows that this ratio is about 7:1.

How do we know that all these three factors contribute to false recognition? May it not be, for instance, that the impression of V does not actively obliterate the memory of N? May it not be that the observer has forgotten during the interval what N was, so that he has not the faintest idea of it even before the shutter goes up for the trial exposure? Moreover, according to the law of association by similarity, the more similar V is to N, the more likelihood should there be that the perception of V will revive the idea of N instead of blotting it out.

We have already shown, however, that obliteration actually does occur when the similarity between V and N is 0%.¹⁷ It is now incumbent upon us to prove that it also takes place when

¹⁶ *The Time it Takes to See and Name Things*, *Mind*, Vol. X (1906).

¹⁷ See pp. 53-4.

the similarity is high. This proof is furnished by the following facts:

(5) With every degree of similarity there were cases when the observer was confronted by both the memory of N and the sense impression of V. Thereupon a doubt arose in his mind as to whether the substituted word was the one he had seen on the first exposure or whether the memory idea carried over from the first exposure was a delusion. And in proportion as the similarity increased the observers dismissed their memory ideas, which were correct in all instances, and decided in favor of the immediate sense impression.

The following table shows the number of times conflicts between the memory ideas and sensory impressions occurred with each degree of similarity, and how the decisions were distributed. It should be understood that by conflict I mean the actual balancing of memory against perception. Naturally, these figures represent only those cases in which the V did not produce a clear shock of strangeness, and so needed the memory of N in order to be discriminated.

TABLE II
Final Decisions Resulting from Conflicts between Memory and Perception,
i.e. between I.M.F. and S.M.F.

% S =	0	25	37	50	62	75	87
+	1	1	1		2		
±		1	1		2		1
—					3	1	2

We see how, with increasing similarity, it was the impression that won out and the memory that lost. Does not this indicate the superiority of the S.M. F. over the I. M. F.?

A few samples of introspection will show how the battle between these two forces was waged.

Group 17: 0% S. N = Elliott, V = manual.

Subject Ke. + (—): Manual seems strange, 80% sure that it is different. It seems as if I had an image of what was there before, but I cannot pronounce the word. I feel as if it were on the tip of my tongue.

Group 22 27% S 3 A — sanctify, 3 R — sanctity.

Subject 22 27% S 3 A — sanctify, 3 R — sanctity.

second time. It seems as if on the first exposure I had seen 3 A and now I see 3 B, but I do not know whether I saw it clearly or not this second time. It may be my memory is incorrect. The only doubt is about one letter. The first part of the word "sanct-" is perfectly familiar. As soon as the shutter went up the second time I read the whole list rapidly, and found them all familiar. Then I went back to 3 and hesitated. Even now, as I talk to you, there is a conflict in my mind between 3 A and 3 B, and I don't know what to say.

Group 20: 75% S. 5 A = rebuttal, 5 B = rebutter.

Subject Ha.—: I thought you had "rebuttal" there before, but now I see "rebutter". It may be that I did not see the last part of "rebutt-" in the first exposure and that during the interval I simply switched over to the more familiar word of the two, for 5 A is more commonly used than 5 B. I will therefore say no change, 80% sure.

Group 46: 87% S. 5 A = passible, 5 B = passibly.

Ke. — : I don't think there is any change, 90% certain. I carried 6 and 5, i.e. I carried number 5 in memory as "passible". But when I saw "passibly" on the second exposure, I took it for granted that that was the word I had seen the first time, and that I had attached a wrong end to it either while reading or thinking about it, i.e. I had put "ble" where "bly" was. That is the only way I can explain it. I'll say no change.

Group 28: 62% S. 7 A = manifold, 7 B = manifest.

Ha. + (?): 7 B gives me a feeling of strangeness; 100% certain that it is new. I must confess though that the moment I began to think about 7 B the idea occurred to me that I had seen it in that position before, so I was going to say no change. But the first psychological impression I got from it was one of decided strangeness. This feeling of strangeness, however, began to diminish as I continued to think about the word. The more I think of it even now, the more does it seem familiar. I had visual and auditory images of 1, 2, 4, and "manifold". I do not know where I got the word "manifold", neither do I know why it kept running through my mind during the interval. It now occurs to me that the persistency of "manifold" during the

interval accounts for the fringe of familiarity that attaches to "manifest". I say this because somehow the two words are entangled in my mind. As I compare them I see, of course, that they overlap in some letters. The confusion may be due to that.

Bu. — : I carried "manifold" instead of "manifest" during the interval, yet "manifest" seems familiar. I do not know how to account for this phenomenon. I think the confusion is due to my own carelessness; I probably did not pay enough attention to word 7 when it first appeared. Or perhaps I just read the first part of it—"mani"—and tacked on the last part—"fold". It is also probable that I simply read "manifold" instead of "manifest". Whatever the cause may be, I am certain that I carried "manifold" in my mind during the interval. But since "manifest" looks familiar I'll say no change. I had images of 1, 2, 5, 6 and 7 A.

Group 24: 62% S. N = tyrannous, V = tyrannize.

Ha. — : None changed, certain. I had little imagery during the interval. But they all look familiar, though I confess that I really expected to see "tyrannous" where "tyrannize" is now. I cannot say why I had this expectation. Perhaps a vague image of "tyrannous" had crept into my mind during the interval and persisted there. (Here I asked the observer, If you had found tyrannous where tyrannize is now, would you have accepted it as familiar?) "Oh, there is no doubt, for it would have fitted in with my expectation", was the reply.

It was enough for the new word to blot out the non-common element of the old word and to intensify the common part, thereby making itself acceptable in consciousness while it revived, or better still, allowed only a partial echo of the previous one to resound. In some instances this partial echo, like the ghost of Hamlet's father, had the strength to cry out against the intruder; in others it was "very faint." Note the following case:

Group 36: 62% S. 4 A = fabulous, 4 B = fabulize.

Ke. + (?): "Fabulize" seems new, 100% certain. The effort of inner speech in trying to pronounce it is very marked, especially with the last part of the word—"ize". It is a sound

that involves more mouth movement. The first part of the word seems familiar in visual terms. During the interval it seems that I thought of the word "fabulous." That is the word that must have been there before, but I am not sure, for it may also have come into my mind by association on seeing "fabulize".

Ma. \pm (?): "Fabulize" seems new. I'll give it 60% as being new on the basis of sound. This word differs from the previous one in the z sound.

No further proof seems to be necessary to show the triple way in which the substituted word functions in having itself accepted for the genuine one. Even where it fails to obliterate the memory of the previous word entirely, it tends to do so with increasing force as it approaches the normal word in degree of resemblance. This is indicated by the figures in Table 11. As the similarity increases the subjects tend more and more to say to themselves that the echo of the N word is a delusion, or that it is too faint to rely upon, and so they accept the variable for the normal with increasing assurance, thus committing more definite errors. Here we have concomitant variation. It is only logical to conclude, therefore, that all the errors of "was", (where V was said to have been N), result from the superiority of the sensory-motor over the ideomotor force.

(6) It happened occasionally that a subject would mistake one of the words constituting the milieu as being new because it produced an association or an attitude not experienced on the first exposure. Thus in group 35 "oiliness" was one of the 7 words making up the setting. When this group was exposed the second time, observer Bu. said, "I am doubtful about "oiliness" because if it had been present before, I should have had tactual imagery with it such as I get on seeing it now." The same subject declared he was positive that he saw all the words on the first exposure.

(7) It is difficult to draw a sharp line between the cases that come under (6) and those where the repeated stimulus was mistaken for new because of its failure to revive an image

or an association previously experienced. I have only one such instance that bears sharp lines of distinction.

Group 41: 100% S. Ha. — : "Polemic" is new; 100% sure. It does not fit in with the association of the first exposure. It is unusual. But as I think about it, I feel less sure that it is new.

NOTE.—Because form is the most conspicuous feature in the visual realm which causes confusion we have taken that as the basis of differentiation in the construction of our similarity scale. Our method has been justified both by the psychological judgments and by the results of the recognition tests. But now, what should be done with those few words which, possessing the same structural similarity yielded such large divergent results in the tests on recognition? (See Table 8, W. 33 and 40.) Our answer is that such words, whenever they occur, should be treated like other exceptional phenomena in nature—the cause of their apparently exceptional character should be sought for.

The similarity of words No. 40 was 75 per cent, yet the substitution of one for the other was recognized 100 per cent of the times. An examination of the protocol shows the reason why. The non-common elements in these words are so diverse that they sufficed to counterbalance the elements that are common. Thus introspection reads: 40:75% S. 2A = parallel, 2B = parallax.

Ha. + (—): 2B is new; 100 per cent sure. I do not know what was there before. It felt new the instant I saw it. It did not fit in with any of the associations I had received from the first exposure, and it awakened a lot of new associations. It has an unusual combination of sounds; *the end especially sounds very queer*. All the others are familiar.

Bu. + (—): 2B is new; absolutely certain. That 'x' at the end of 'parallax' is altogether strange in sound. The first part of 2B reminded me of 2A.

Ma + (+): 2B struck me as quite strange, and at the same time I got a verbal image of 2A. 100 per cent certain 2B is new. All words seemed familiar until I came to 'parallax,' when the 'ax' struck me as strange. It did not seem to sound just right.

CONCLUSIONS

The results of our experiments up to this point seem to justify the following conclusions:

(1) Among words there are two kinds of similarity—internal or meaningful similarity and external or structural similarity—both of which are capable of producing confusion in the recognitive consciousness.

(2) Internal similarity has no fine gradations, the highest attainable degree being that which would fall about midway between the two extremes of structural similarity.

(3) There is a simple inverse relation between degree of similarity and cognitive ability, the one being to the other, in terms of percentage, as $X : (100-X)$.

(4) It is more difficult to recognize the same thing in a totally new environment than in the same environment—the ratio of difficulty being approximately as 12:1.

(5) It is more difficult to recognize the same thing in a totally new setting than a totally different object in the same setting—the ratio of difficulty being 3:1. It is understood, of course, that the variable and normal items must belong to the same category of objects.

(6) The feelings of familiarity and strangeness tend to neutralize each other by fusion, that one gaining the upper hand in consciousness which is induced by the more intense stimulus.

(7) With high similarity the majority of errors are due to the obliteration from memory of the original N impression by the perception of the V stimulus, owing to the superiority of the sensory-motor force over the ideo-motor force.

(8) Doubt arises from a conflict of these two factors—the perception or the memory triumphing according as objective similarity is high or low.

PART III

EXPERIMENTS WITH PICTURE POST-CARDS

The question now arises, to what extent are the foregoing results true for recognition in general? In order to verify them we looked about for material that would be less artificial than words and more representative of real life situations. Still applying ourselves to the visual realm where, as Katzaroff says, the largest part of life's experience is cast, it occurred to us that picture post-cards would be the ideal material; since they represent all kinds of scenes and situations—such as streets, parks, buildings, landscapes, sea-scapes, plants, animals, and men, women and children in different activities and groupings;¹ in short, everything that anybody can conceive of perceiving in daily life and be called upon to recognize later on.

In order to grade these cards we resorted to the judgments of 15 mature and competent men. It has already been shown in the word series that such judgments of similarity are fairly reliable, and, in the main, quite representative of the objective similarity determined by a mechanical process. These psychological judgments, moreover, were found to harmonize not only with the mathematical values of the words, but also with the recognition values obtained from different individuals. This, it seems, is sufficient proof of the reliability of a large number of judgments, rendered by competent individuals, concerning objective similarity.

We, therefore, proceeded to grade these picture post-cards in the following manner: Two cards that were totally dissimilar—one representing a large red rose and the other a gray church building outlined against a blue sky—were put side by side and given the arbitrary value of zero similarity, (0% S). Two other cards that were identical were placed beside the first pair and labeled 100 per cent similar (100% S). These two pairs of

¹ This material is now filed at the Harvard Psychological Laboratory.

cards were hung upon a wire rack. Beneath them, in three or four horizontal rows, were strung up the 25 or 50 pairs that were to be judged. The backs of these cards were marked 1 A, 1 B; 2 A, 2 B; . . . nA, nB. The A card was placed to the left, the B to the right in the standardization process, and the former used as the standard the latter as the variable in the recognition tests. Fifteen individuals were then called into the experimenting room, one at a time, and given the following instructions: "You have before you a number of picture post-cards which come in pairs horizontally arranged. I want you to look at them and to tell me how similar you think the members of each pair are to one another, expressing your judgment in terms of per cent. Let the upper two pairs of cards serve as the opposite ends of the scale, i.e. as the zero and identity ends, and between these two extremes you will try to locate the rest in terms of 10 or multiples of 10. You may look over all the cards before you begin to judge, but give your judgment on the basis of the first impression in each instance."²

These 15 judgments rendered, I calculated their average and called that the objective similarity of the corresponding pair of cards. Each pair was then placed into an envelope which was labeled with the number and value of its contents and filed for use. The reliability of this arithmetic average was, of course, determined by the mean variation. The mean variation differed with every degree of similarity owing to the fact that it was a function of the range of judgment, as it should be. That is to say, cards that were rated at 45% S had a larger M. V. than those that fell within the 0% or 80% unit of the similarity scale. The reason is that cards of 45% S afforded a wider range of judgment—a range that theoretically varied between the 0 and 90% units of the scale—than the cards which fell into either of these units. The following table gives the scale of resemblance, the average M. V. corresponding to each unit of the

²I owe many thanks to Prof. Münsterberg, Dr. S. M. Graves, C. F. Hawkins, C. L. Hedden, W. W. Reed, Miss Wilkins; my colleagues in the laboratory and the rest of those who showed real friendship as well as a genuine interest in the promotion of science, when they condescended to sacrifice much of their valuable time in judging the cards.

scale, and the number of pairs of cards that entered into the construction of each unit.

TABLE 12

% S	0	25	35	45	55	65	75	80	85	100
Av. M.V.	0	12	12	13	12	11	9	6	4	0
No. of Cards	50	15	25	43	57	78	61	10	23	50

It will be remembered that in the case of the words I grouped the psychological judgments on the basis of structural similarity, but I had no such external guide for the grouping of the judgments rendered with respect to the picture post-cards. Hence, I adopted the following system of classification: All cards whose values ranged between 1 and 9% were put into one class, those that varied between 10 and 19% were put into another class, and so on for cards that varied between 20 and 29%; 30 and 39%; 40 and 49%, etc. In general, all cards whose values ranged $10x$ and $(10x + 9)$, where x is a positive interger, were grouped together, their average taken, and that average made to constitute a unit of the similarity scale. The construction of a series for the recognition experiment was then a simple matter. All that was necessary to do was to take from the filing cabinet an equal number of pairs from each group. So that in each series there would be 3 or 4 pairs of 0% S, an equal number of 20% S, 30% S, and so on.

EXPERIMENTS ON RECOGNITION WITH PICTURE CARDS

With my material thus standardized and classified, I proceeded to investigate with it the following three factors: (1) The relation between degree of similarity and recognitive ability. (2) The influence of N —the number of objects exposed on recognition. (3) The influence of t —the time of exposure on the recognitive process. The first factor was really our main concern; the other two were varied in order that we might discover the ideal conditions under which a definite mathematical relation might be established between similarity and ideational recognition. This condition, as we have remarked on a previous occasion, must be one such that, with the largest number of items exposed for the shortest possible time, the substitution of cards

of 0% S will be recognized 100% of the times, and the repetition of identical cards will also be recognized 100% of the times, or almost that often; while the substitution of cards of intermediate values would be recognized in some definite ratio to their similarity. What that ratio was is what we wished to find out.

For these experiments I used the apparatus described on page 15. The critical card was exposed among a fixed number of other cards, and after an interval of 20 seconds, the same setting was re-exposed with the variable card in place of the normal or with no change at all, as the case may have been. The position of different pairs of critical cards was varied from group to group, but that of the same pair was kept constant for all observers. The milieu for each alternate exposure, i.e. for each pair of critical cards was always new, and never was a card from one setting shown in another. Not only were the critical cards kept in the same position for each observer, but the entire pattern or layout of the setting was also kept intact for all ten subjects.³

I had 10 subjects for these experiments—all of them members of the Harvard Psychological Laboratory, and, for the most part, trained psychologists. Eight of the observers were men and two women.⁴ Each of them came to me separately, at intervals of one week. The sitting with each observer lasted one hour, during which time an average of 15 or 20 judgments were recorded. All sittings took place between 9 and 1 o'clock.

Each series, comprising from 25 to 30 pairs of cards, was devoted to the examination of a different combination of N and

³ An easy way to insure uniform conditions with respect to this matter is to paste the "fillers" on a cardboard, and to leave a vacant place, of the proper dimension, for the normal, which, after the first exposure, can be quietly removed and the variable put in its place. One should take care, however, that the loose critical cards lie flat against the background, or else the observers will learn to distinguish them from the "fillers" on the first exposure, and this, of course, will vitiate the experiment.

⁴ For their participation in these experiments I am very grateful to Misses O. E. Martin and O. L. M. Seeley, to Drs. R. M. Elliott, R. C. Givler, H. S. Langfield, H. T. Moore, and Messrs. J. H. Picken, A. A. Roback, N. N. Sen Gupta, and E. C. Tolman.

t—size and length of exposure. I tried to have as many units of the similarity scale as possible represented in each series. Not always, however, was I successful in this, because it was difficult to find cards of very low and very high similarity, i.e. below 30% and above 80%. Had I been able to find such, I should have had a scale of 11 units, but owing to the lack of such cards the scale will be found to contain from 8 to 9 units. Inasmuch as each unit in a given series is represented by 3 or 4 pairs of cards, the recognitive value of that unit is based on 30 or 40 judgments.

Cards from two or three different series were exposed at each sitting so as to avoid the inequality that would result from improvement due to practice. In this manner one-half of all series were exposed during the first semester of 1912-1913, and the second half during the second semester. The 10 subjects who participated in this investigation were never allowed to see any of the cards either before or after the experiments, nor were they ever acquainted with their achievements.

The following tables embody the results of our investigation with the picture post-cards. They should be interpreted thus: The first vertical column contains the symbols whereby each pair of cards were designated; the second column, marked % S, contains the corresponding similarity of the variable card to the normal in that pair. The three columns under the heading "Distribution of Judgments", show how the substitutions were recognized by each of the ten observers, i.e. whether they were recognized positively (+), or not at all (—), or doubtfully (\pm), namely, with a degree of certainty that varied between 20 and 79%. The sixth column, marked % C, represents the per cent correct recognition, of course, they are judgments of "change" in all cases except those of 100% S, for these latter they are judgments of "no change". The next column shows the total number of judgments falling within each unit. The column following, marked Av. % S, contains the average per cent similarity of the several cards within the sub-group, while the column headed Av. % C gives the average per cent. correct recognition for those cards.

The last column needs a little fuller explanation. It will be remembered that I advised the observers not only to point out or to name the item which they thought was new, but also to mention what it had displaced. The ability to do both of these things is called perfect knowledge, (P. K.), and the percentage of times that the subjects were able to render such descriptions is recorded in the column % P. K. This, it should be noted, served to indicate whether the observers actually recognized the substitution or merely guessed it; also whether the variable card called up the normal by association, or whether the normal was carried during the interval as a memory idea, or as an image, or, finally, whether the variable tended to obliterate the normal from consciousness. Obviously perfect knowledge of substitution could exist only where the observers were 100% certain of their judgments of recognition. Hence the figures in the last column refer only to the judgments recorded under the sign +; they have nothing to do with the doubtful decisions. Where, therefore, as a result of the application of the Correction Formula a subtraction occurs from the total number of correct judgments that subtraction finds its justification in the existence of the doubtful judgments, (\pm), assuming that P. K. = 100% in the given case.

Let us make a trial reading. Suppose we take a pair of cards that fall within the group of those that vary in similarity between 70 and 79%. Let it be No. 309. If we read across horizontally, we note that they were 74% S, that 6 subjects recognized the substitution definitely, that 2 failed to recognize it altogether, and that 2 were doubtful about it. By employing the formula

$$\frac{C + D}{2}$$

where C stands for the number of correct, D doubtful, and

$$C + W + D$$

W wrong judgments, we note in the next column that the per cent correct recognition for card 309 is 70. Since 5 pairs of cards whose similarity varied between 70 and 77% were used, and since each substitution was judged 10 times, a total of 50 judgments were recorded. The average similarity of the cards within this group is 74%, and the average correct recognition of their substitutes is 67%. Hence we conclude that under the given conditions of the experiment—4 cards exposed 10 seconds—the replacement of one by another of 74% S will be recognized about 67% of the times. This conclusion is not based on the 50 judgments alone. It receives its strongest

support from the fact that there exists an inverse relation between recognitive ability and degree of similarity for the entire series, as the figures show, and that this inverse relation also holds for the particular unit in question.

The experiments are divided into different sets—A, B, C, etc., in each of which one factor is kept constant while the other two are varied. Thus in set A the exposure time—*t*, is kept constant while the number of objects, *N*, and degree of similarity, *S*, are varied.

EXPERIMENTS A

TABLE 13
Series I: 4 Cards Exposed 10 Sec.

Card No.	%S	Distribution of Judgments			%C	Total Judgments	Av. %S	Av. %C	% P.K.
		+	—	±					
286	0	10			100				
300	0	9			100	29	0	100	72
290	0	10			100				
33x	35	8		2	90	10	35	90	100
32x	41	9	1		90				
223	47	7	3		70	30	46	87	100
31x	49	10			100				
36x	55	8		2	90				
168	55	5	4	1	55	30	56	82	100
37x	58	10			100				
35x	63	9		1	95				
38x	66	3	3	4	45	30	65	78	95
263	67	9		1	95				
34x	70	5	1	4	70				
39x	73	8	2		80				
309	74	6	2	2	70	50	74	67	100
182	77	7	2	1	75				
299	77	3	5	2	40				
40x	91		8	2	10	10	91	10	
12a	100	8		2	90				
17a	100	10			100	30	100	97	
21a	100	10			100				

TABLE 14
Series II: 5 Cards Exposed 10 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
150	0	10			100				
18b	0	10			100	40	0	100	73
21b	0	10			100				
29b	0	10			100				
105	25	10			100	10	25	100	?
124	42	10			100				
131	44	5	4	1	55	40	46	78	92
163	48	6	3	1	65				
245	49	9	1		90				
97	50	9	1		90				
259	51	9		1	95				
8x	55	4	2	4	60	50	54	76	94
7x	57	4	4	2	50				
305	59	8	1	1	85				
85	60	7	1	2	80				
176	64	7	1	1	83				
2x	66	5	3	2	60	48	64	65	100
3x	68	4	4	2	50				
304	68	3	3	3	50				
4x	70	5	4	1	55				
262	71	8	2		80				
319	75	7	2		77	49	74	54	91
77	76	3	5	2	40				
5x	79	1	7	2	20				
1x	80		10		0	20	81	25	?
10x	81	4	4	2	50				
9x	94		9	1	5	10	94	5	
16a	100	10			100				
20a	100	10			100	40	100	96	
24a	100	9		1	95				
27a	100	9	1		90				

TABLE 15
Series III: 6 Cards Exposed 10 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
15b	0	10			100				
281	0	10			100	30	0	100	60
311	0	10			100				
221	45	9	1		90				
115	47	7	3		70	30	47	82	100
314	49	8	1	1	85				
159	50	7	1	1	83	19	55	81	91
45x	59	7	1	2	80				
47x	61	6	2	2	70				
41x	66	7	1	2	80	30	64	73	100
239	66	7	3		70				
44x	70	7	1	2	80				
172	73	5	3	2	60	30	73	67	?
42x	75	5	3	2	60				
49x	83	4	3	3	55	20	85	40	
46x	86		5	5	25				
1a	100	9	1		90				
2a	100	6	1	3	75	30	100	88	
3a	100	10			100				

On turning our attention to the main results of the three foregoing tables we find. (1) that there is a marked inverse relation between degree of similarity and recognitive ability; and (2) that recognition becomes more difficult as the size of the group increases. Series II as compared with Series III, seems to be an exception to the second statement. But this is because Series II was the first one experimented with, hence, the observers being entirely unpracticed, yielded lower results with it than with the larger exposure of Series III.

The results given in the next and subsequent Comparison Tables are based on the Av. % C modified by the Correction Formula. The letters N_4 , N_5 , N_6 , N_x stand for the groups—the subscripts representing their size; while t_4 , t_5 , t_6 , t_x represent the length of exposure. Therefore, N_4-t_{10} means a group of 4 cards exposed 10 seconds, etc.

TABLE 16

Comparative Per Cent Correct Recognition when Cards of the Same Degree of Similarity are Exposed for 10 Seconds in Groups of 4, 5, and 6

% S =	0	25	35	45	55	65	75	80	85	90	95	100
N ₄ — t ₁₀	100		90	87	82	78	67			9		100
N ₅ — t ₁₀	100	100		78	76	65	54	25			4	100
N ₆ — t ₁₀	100			81	80	72	66		38			100

EXPERIMENTS B

The next set of experiments were conducted along the same line as the previous ones, except that the exposure time was reduced from 10 to 7 seconds.

TABLE 17

Series IV: 4 Cards Exposed 7 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	—	±					
149	0	10			100	20			
149x	0	10			100		0	100	70
107	23	9	1		90	10	23	90	?
145	32	9	1		90	10	32	90	?
87	40	10			100				
130	45	9	1		90	30	44	85	100
249	48	5	2	3	65				
63x	50	6	2	2	70	20	52	78	92
192	53	8	1	1	85				
74	62	8	1	1	85				
216	66	6	4		60	30	65	65	90
116	68	3	3	4	50				
70x	70	7	2	1	75	20	72	63	100
110	73	4	4	2	50				
323	88	1	7	2	20	10	88	20	
72a	100	9		1	95				
73a	100	9		1	95	30	100	97	
742	100	10			100				

TABLE 18
Series V: 5 Cards Exposed 7 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
61x	0	10			100				
62x	0	10			100	30	0	100	40
63x	0	10			100				
217	44	9		1	95				
50x	46	6	3	1	65	40	46	84	94
106	47	10			100				
56x	48	7	2	1	75				
104	50	9	1		90				
57x	53	5	3	2	60	30	53	75	95
99	55	7	2	1	75				
53x	67	7	3		70				
54x	67	4	3	3	55	30	68	70	94
52x	69	8	1	1	85				
118	70	4	5	1	45	20	74	55	92
55x	78	5	2	3	65				
51x	80	6	4		60				
303	82	2	6	2	30	30	82	40	100
326	83	4	6		40				
59x	90	2	6	2	30	10	90	30	
58a	100	9		1	95				
4a	100	10			100	30	100	93	
5a	100	8	1	1	85				

TABLE 19
Series VI: 6 Cards Exposed 7 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judgments	Av. %S	Av. %C	% P.K.
		+	-	±					
148	0	9	1		90				
16x	0	10			100	30	0	97	
324	0	10			100				
141	23	7		3	85	10	23	85	?
63x	41	4	2	3	61				
62x	43	6	1	3	75	39	47	65	92
227	47	6	3	1	60				
16	49	6	4		60				
65x	52	9	1		90				
182	53	5	3	2	60	30	54	62	100
71x	58	2	5	3	35				
199	60	6	2	2	70				
52	60	6	2	2	70				
52	61	5	3	2	60	40	64	61	80
48	67	6	3	1	65				
179	69	4	4	2	50				
62	70	1	6	3	25				
91	74	1	7	2	20	29	72	22	
184	75		5	4	22				
119	80	2	5	3	35				
66x	87	1	7	2	20	30	84	20	
71	86		9	1	5				
15a	100	8		2	90				
22a	100	8		2	90	30	100	92	
24a	100	9		1	95				

TABLE 20
Comparative Per Cent Correct Recognition when Cards of the Same Degree of Similarity are Exposed for 7 Seconds in Groups of 4, 5, and 6.

% S =	0	25	30	45	55	65	75	85	90	100
N ₄ - t ₇	100	90	90	85	78	65	63		19	100
N ₅ - t ₇	100			84	74	69	54	39	29	100
N ₆ - t ₇	100	87		66	63	61	22	19		100

Here we note once more that on the whole recognitive ability varies inversely (1) as degree of similarity, and (2) as the number of items exposed.

The best way to compare one series with another, when t is constant, is to divide the sum % C of one by the sum % C of the other which exist for the *same* units of similarity,—discarding the values of C for 0% S and 100% S. Thus the units of similarity common to series IV and V, i.e. N_4 and N_5 , are 45, 55, 65, 75, and 90. Now Σ % C for $N_4 = 310$, and Σ % C for $N_5 = 309$.

Therefore, $N_4 : N_5 = 1 : 1$.

Similarly, $N_5 : N_6 = 1 : .75$.

and $N_4 : N_6 = 1 : .78$.

EXPERIMENTS C

The third set of experiments still follow along the same lines, only this time the period of exposure is once more reduced—from 7 to 5 seconds. Here we add a group of 3 cards to those of 4, 5 and 6, in order that we may compare the difference between exposing 6 cards for 10 cards, (see Table 15), and half that number for half that time.

TABLE 21
Series VII: 3 Cards Exposed 5 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
67x	0	10			100				
68x	0	10			100	30	0	100	100
73x	0	10			100				
220	39	10			100	20	39	100	95
273	39	10			100				
55	40	8	2		80				
84	45	6	1	3	75	40	44	79	100
180	45	7	2	1	75				
318	45	7		3	85				
133	51	9		1	95				
29	52	7	2	1	75	40	54	77	89
33	57	5	1	4	70				
255	57	5	1	4	70				
54	62	5	3	2	60				
4	66	5	3	2	60	30	64	72	93
170	66	9		1	95				
93	70	3	4	3	45				
47	74	5	2	3	65	39	75	53	100
61	77	2	2	6	50				
117	78	4	4	1	50				
14	81		5	5	25	20	81	50	100
253	81	6	1	3	75				
20a	100	9		1	95				
21a	100	8		2	90	40	100	94	
26a	100	9		1	95				
27a	100	9		1	95				

TABLE 22
Series VIII: 4 Cards Exposed 5 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judgments	Av. %S	Av. %C	% P.K.
		+	-	±					
146	0	9		1	95				
16b	0	10			100	40	0	100	63
17b	0	10			100				
28b	0	10			100				
127	30	7	3		70				
289	30	9	1		90				
193	38	9		1	95	40	34	85	84
89	39	8	1	1	85				
76	42	8		2	90	20	43	82	100
9	45	6	1	3	75				
88	50	4	3	3	55				
96	57	5	1	4	70	30	55	62	90
30	58	5	3	2	60				
1	60	4	3	3	55				
151	62	3	1	5	61	39	64	55	87
82	67	5	4	1	55				
36	68	4	4	2	50				
165	70	3	3	4	50				
72	70	4	5	1	45	40	74	46	91
126	78	5	3	2	60				
40	78	2	6	2	30				
79	80	1	7	2	20	20	80	35	100
109	80	5	5		50				
28a	100	10			100				
29a	100	9		1	95	40	100	95	
30a	100	9		1	95				
31a	100	8		2	90				

TABLE 23
Series IX: 5 Cards Exposed 5 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
229	0	8	1	1	85				
232	0	9		1	95	30	0	90	44
237	0	10			100				
166	20	6	3	1	65				
167	23	6	3	1	65	40	25	79	75
215	26	9		1	95				
207	29	9	1		90				
103	33	6	3	1	65	19			
174	39	6		3	83		36	74	91
100	44	7	3		70				
186	44	6	2	2	70	30	46	67	100
59	49	3	1	6	60				
236	53	7	1	2	80				
6	54	4	4	2	50	40	55	61	91
18	57	6	3	1	65				
162	57	5	5		50				
21	60	4	3	3	55				
32	64	1	6	3	25	30	64	35	100
48	67	2	7	1	25				
49	70		6	4	20				
161	74	2	8		20	20	73	20	100
42	80	3	6	1	35				
37	89		9	1	5	20	85	20	
32a	100	9		1	95				
33a	100	8		2	90	40	100	91	
34a	100	9		1	95				
35a	100	8	1	1	85				

TABLE 24
Series X: 6 Cards Exposed 5 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judgments	Av. %S	Av. %C	% P.K.
		+	-	±					
29b	0	9	1		90				
30b	0	10			100	30	0	90	22
31b	0	8	2		80				
246	30	8	2		80				
81	32	6	4		60	40	35	74	92
270	37	8	1	1	85				
95	39	6	2	2	70				
198	47	7	1	2	80	20	48	70	83
78	48	4	2	4	60				
23	50	2	6	2	30				
173	54	4	3	3	55	30	53	40	100
8	58	3	6	1	35				
10	60	2	6	2	30				
258	65		8	2	10	30	64	27	60
75	66	3	5	2	40				
241	72	1	7	2	20				
136	77	1	8	1	15	30	75	23	66
140	77	2	6	2	30				
108	80	2	7	1	25				
151	82	1	8	1	15	40	83	21	100
256	83	2	7	1	25				
73	86		6	4	20				
38a	100	7		3	85				
39a	100	10			100	40	100	91	
40a	100	9		1	95				
41a	100	8	1	1	85				

TABLE 25

Comparative Per Cent Correct Recognition when Cards of the Same Degree of Similarity are Exposed for 5 Sec. in Groups of 3, 4, 5 and 6.

% S =	0	25	35	40	45	50	55	65	75	80	85	100
N ₃ — t ₃	100			99	78		76	71	51	48		100
N ₄ — t ₄	100		86	82			62	54	45	34		100
N ₅ — t ₅	100	87	80		71		64	37	20		20	100
N ₆ — t ₆	100		80			74	44	30	23			100

If we compare any two series by taking those values of C which exist for the similarity units common to them, (discarding the extreme ends) we find further proof of what may be called the second cognitive range, that cognitive ability

varies inversely as the number of objects perceived. Is there a definite mathematical relation between N and C , where N stands for the size of the group and C for correct recognition? Of course, much will depend on time of exposure— t . But since this third factor is constant in the above series it may be cancelled out. If we do so, we find, with respect to the remaining factors, that their product in any given series divided by their product in any other series is always equal to a constant K , the value of which is $1 \pm .1$.

That is,
$$\frac{N_m C_m}{N_n C_n} = 1 \pm .1 .$$

Thus suppose we take N_3 and N_4 . The similarity units common to them are 40, 55, 65, 75, 80. Now the sum of % C common to these units for N_3 is 345, and for N_4 , 277. There-

fore,
$$\frac{C_3 N_3}{C_4 N_4} = \frac{345 \times 3}{277 \times 4} = .94 \dots (1) .$$

Likewise,
$$\frac{C_4 N_4}{C_5 N_5} = \frac{247 \times 4}{201 \times 5} = .98 \dots (2) .$$

while
$$\frac{C_4 N_5}{C_5 N_6} = .93, \dots (3) \qquad \frac{C_3 N_5}{C_6 N_6} = 1.00, \dots (4)$$

$$\frac{C_4 N_4}{C_6 N_6} = .95, \dots (5) \qquad \frac{C_3 N_3}{C_5 N_5} = .86, \dots (6)$$

Of these six equations only one—(6)—varies considerably from the standard equation. Obviously, in this particular combination, % C for N_3 is too low. If we turn to the individual values of % C in this group, we find that the value for 45% S is only 78, whereas the one immediately before it is 99 and the one immediately following is 76. For this reason, then, and because the other equations give results that are so near unity, we may conclude that the standard equation is correct.⁵

*Whether or not the foregoing results are due to chance can be easily determined by asking ourselves the following question: Given four independent variables— a , b , c , d ,—what is the probability that the combination of any two of them will always give rise to the same event?

The possible ways in which the 4 variables can combine taken 2 at a time, are 6. That is, they can give rise to 6 different events. Suppose we represent them thus:

$ab = A, be = B, cd = C, ac = D, ad = E, bd = F.$

Now since $\frac{C_1 N_2}{C_2 N_1} = 1$, we get, by transposing, $C_1 N_2 = C_2 N_1$.

Therefore, $\frac{C_1}{C_2} = \frac{N_1}{N_2}$; similarly, $\frac{C_2}{C_3} = \frac{N_2}{N_3}$, and in general, $\frac{C_m}{C_n} = \frac{N_m}{N_n}$.

Whence we may conclude that under the conditions of the experiment, and with a constant exposure time of 5 seconds, the recognitive ability of one group is to another inversely as the size of the first is to the size of the second. Thus if a card of x% S is recognized y% of the times in a group of 3 items exposed for 5 seconds, then in a group of 4 times it will be recognized $\frac{3}{4}$ y% of the times.

If we now take the values of % C obtained with $N_6 t_{10}$, (Table 15), and compare them with those of $N_3 t_5$, corresponding to the same units of similarity, we find that $\frac{\sum C_6}{\sum C_3} = 1.04$. This indicates that for cards exposed at the rate of one per $1\frac{2}{3}$ sec., the doubling of the time of exposure fully compensates for the doubling of the number of cards exposed—within certain limits of course. This fact should be kept in mind, for it will be shown that this relation does not hold for cards exposed at the rate of one per 1 sec.

EXPERIMENTS D

The final set of experiments were made with cards exposed at the rate of 1 per sec. in groups of 3, 4, 5 and 6. The following tables give the itemized results,⁶ while Table 29 contains the main results modified by the Correction Formula. It is on the basis of these latter that the curves in Plate II are drawn.

Since we do not know beforehand which one of these occurrences *ab* will give rise to, and since it *must* give rise to one of them, the probability that it will produce its effect, A, let us suppose, = 1. But now what is the probability that *bc* will give rise to A and not to something else, such as B, or C or D? Manifestly $\frac{1}{6}$. Likewise the chance that *cd* will give rise to A only, = $\frac{1}{6}$; and so on for *ac*, *ad* and *bd*. Therefore, the probability that all combinations will give rise to the same event on the basis of chance = $1 \times \left(\frac{1}{6}\right)^5 = \frac{1}{7776}$. That is, the odds in favor of our results being due to cause and not to chance are 7776 to 1.

⁶For cards exposed at 1 per sec. and 1 per $1\frac{2}{3}$ sec.

These curves show the relation between % C and % S obtained with cards exposed at the rate of 1 per sec. and words exposed at the rate of 1 per $\frac{1}{2}$ second.

TABLE 26
Series XI: 3 Cards Exposed 3 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
33b	0	10			100				
34b	0	10			100	40	0	99	90
269	0	9		1	95				
260	0	10			100				
228	19	10			100	20	20	100	90
225	27	10			100				
153	33	10			100				
195	33	10			100	30	34	98	100
268	35	9		1	95				
12	45	6	2	2	70	20	46	78	100
205	47	8	1	1	85				
121	54	5	4	1	55	20	55	58	100
133	57	5	3	2	60				
68	60	6	3	1	65				
132	63	2	3	5	45	30	62	50	95
53	64	2	4	4	40				
7	70	6	2	2	70				
46	72	3	4	3	45	30	72	46	100
19	73	1	6	3	25				
178	82	2	6	2	25				
247	85	1	9		10	40	85	20	100
70	87	1	6	3	25				
152	88	1	8	1	15				
42a	100	10			100				
43a	100	9		1	95	40	100	93	
44a	100	8		2	90				
45a	100	8	1	1	85				

TABLE 27
Series XII: 4 Cards Exposed 4 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
36	0	10			100				
230	0	10			100	30	0	97	70
233	0	9	1		90				
226	20	9		1	95				
224	25	9		1	95	30	24	97	92
185	27	10			100				
209	30	9	1		90				
211	30	8	1		88	39	33	83	100
196	35	7	2	1	75				
222	39	7	1	2	80				
28	45	9		1	95	20	46	82	85
210	47	5	1	4	70				
254	50	4	2	4	60				
16	52	8	1	1	85	30	53	68	88
234	56	5	3	2	60				
50	63	5	3	2	60				
155	63	1	5	4	30	40	64	45	80
191	65	6	4		60				
11	69	1	5	4	30				
57	70	2	3	5	45	20	72	35	—
35	73	1	6	3	25				
66	81	1	8	1	15				
204	83	2	5	3	35	30	84	22	—
257	89		7	3	15				
47a	100	9		1	95				
47a	100	9		1	95	40	100	95	
48a	100	10			100				
47a	100	8		2	90				

TABLE 28
Series XIII: 6 Cards Exposed 6 Sec.

Card No.	%S	Distribu- tion of Judgments			%C	Total Judg- ments	Av. %S	Av. %C	% P.K.
		+	-	±					
147	0	7	1	2	80				
153	0	8	1	1	85	40	0	86	37
231	0	8	2		80				
250	0	10			100				
219	25	10			100				
208	25	7	1	2	80	40	26	81	93
183	27	8		2	90				
176	28	4	3	3	55				
200	34	8		2	90	20	36	77	100
98	38	6	3	1	65				
143	43	3	4	3	45				
25	45	5	3	2	60	40	45	56	93
26	45	5	3	2	60				
69	46	3	1	6	60				
139	53	2	4	4	40	20	55	38	—
169	56	2	5	3	35				
63	63	2	5	3	35				
65	63	2	4	4	40	40	65	34	90
128	65	1	8	1	15				
111	67	2	3	5	45				
112	72	3	3	4	50				
122	76	2	6	2	30	40	76	30	80
39	77	1	6	3	25				
44	78	1	8	1	15				
212	84	2	8		20				
321x	85	1	8	1	15	40	86	14	—
298	87		10		0				
243	88	2	8		20				
50a	100	7	1	2	80				
51a	100	8		2	90	40	100	88	
52a	100	8	1	1	85				
53a	100	8		2	90				

TABLE 29
Comparative Per Cent Correct Recognition When Cards of the Same Degree
of Similarity are Exposed at the Rate of 1 per Second in
Groups of 3, 4, 5, and 6.

% S =	0	25	35	45	55	65	75	85	100
N ₁ — t ₃ =	100	100	97	77	57	49	45	18	100
N ₁ — t ₄ =	100	99	85	83	69	45	35	22	100
N ₁ — t ₅ =	100	87	80	71	64	37	20	20	100
N ₁ — t ₆ =	100	88	83	61	44	33	31	14	100
Av. =	100	93	86	73	58	41	33	18	100

From these figures we find that the sum of % C of each group—discarding the values for 0% S and 100% S—is

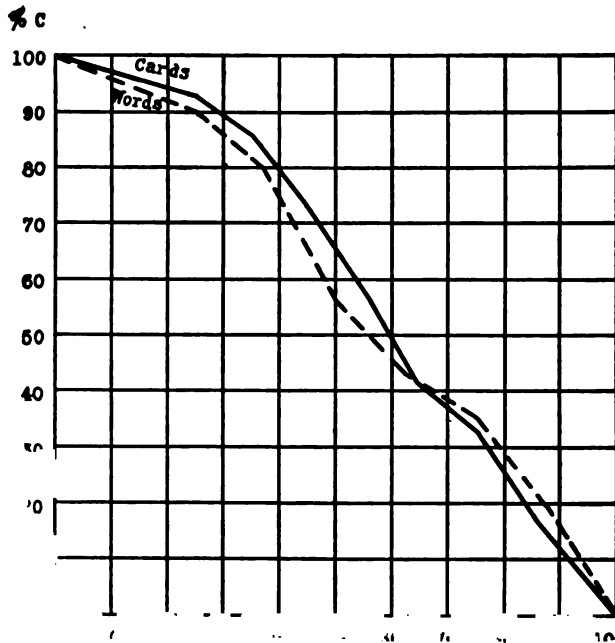
$$\Sigma C_3 = 443, \Sigma C_4 = 438, \Sigma C_5 = 379, \Sigma C_6 = 353.$$

Whence it appears that recognitive ability varies inversely as the number of objects exposed when the length of exposure for each object is the same and not more than 1 second. This may be called the third law of recognition.

On the other hand, it was found; (page 79), that no diminution in recognition occurs if $1\frac{2}{3}$ seconds are added to the exposure time for each additional card. Hence, we may conclude that under the conditions of the experiment, size of exposure stands to length of exposure in the ratio of $1 : (1 + \frac{2}{3} - x)$, when $x < \frac{2}{3}$.

PLATE II

Curves Showing Av. % C for Cards Exposed at the Rate of 1 per Sec. in Groups of 3, 4, 5, and 6, and Words Exposed at the Rate of 1 per $\frac{1}{2}$ Sec. in Groups of 8.



We have thus far considered the influence of three factors on the process of recognition: (1) degree of similarity, (2) length of series when exposure time is constant, (3) length of series when exposure time is proportionate. And we have found that recognitive ability varies inversely as each of these moments. There is still a fourth factor to be considered, namely, the influence on recognition of the varying periods of exposure when length of series is constant.

The following table contains data from Tables 16, 20 and 25, arranged in such a way as to show the % C for groups of 4, 5, and 6 cards when each was exposed for 10, 7 and 5 seconds successively. The reader should remember that there were three series of each size, so that altogether 9 series are represented. In order to make the comparison meaningful we employ only those values of C which have the same units of similarity in all 9 series. Hence the figures in the last column represent Av. % C for $S = 45, 55, 65$, and 75% .

TABLE 30
Influence of Time of Exposure (t) when Size of Group (N) is Constant.

										Av. % C for 4 units of S
% S =	25	35	40	45	55	65	75	80	85	
$N_4 \left\{ \begin{array}{l} t_{10} = \\ t_7 = \\ t_5 = \end{array} \right.$		90		87	82	78	67			79
	90	90		85	78	65	63—			73
		86	86	82	62	54	45	34		61
$N_5 \left\{ \begin{array}{l} t_{10} = \\ t_7 = \\ t_5 = \end{array} \right.$	100			78	76	65	54	25		75*
				84	74	69	54		39	70
	87	80		81	64	37	20		20	48
$N_6 \left\{ \begin{array}{l} t_{10} = \\ t_7 = \\ t_5 = \end{array} \right.$				81	80	72	66		38	73
	87			66	63	61	22		19	53
		80		74	44	30	23		21	43

* Interpolated as the probable value if this series had not been exposed to the observers at the beginning of the experiment, i.e., when they were still unpracticed.

From these figures we gather that for any given value of N recognition varies directly as the length of exposure. This may be called the fourth law of recognition. It seems that it makes little difference whether groups of 4 or 5 cards are exposed for 10 and 7 seconds successively. A decided difference first appears when $t = 5$. This should not be sur-

prising, for if the exposure time is long enough to enable complete or even partial memorization of the group, then it does not matter whether one extra item is added or not.

There is another kind of comparison to be made. On the one hand, we may let $C = 100\%$ when $t = 10$ sec. for every value of N , on the other hand, we may let $C = 100\%$ when $N = 4$ for any value of t . In this way we shall obtain results which will enable us to tell at a glance whether the exposure of the same number of cards for periods varying in the ratio of 10:7:5 sec. gives the same results as the exposure for equal units of time of groups varying in the ratio of 4:5:6 cards.

In the following table part A shows the gradations of C when N is constant and t varies; part B when t is constant and N varies. The calculation in each case is made for the same units of the similarity scale.

TABLE 31

A				B			
Effect of t on Recognition				Effect of N on Recognition			
	N_4	N_5	N_6		t_{10}	t_7	t_5
t_{10}	100	100	100	N_4	100	100	100
t_7	92	94	73	N_5	96	97	88
t_5	78	72	59	N_6	90	72	70

Here we find that for any value of N , the reduction of t first by 30%, than by 50% produces a larger variation in % C than the increase of N first by 25% then by 50% does for any value of t . This is only another way of proving that for each additional item added to a group of x items exposed for y seconds, we must add not 1 second, but $1 +$ a fraction of a second if we wish to obtain the same results. In this connection see conclusion on page 83.

And now if we let % C obtained from the combination of $N_4-t_{10} = 100$, the relative values of any combination of N and t can be seen from Table 32.

TABLE 32

Relative Values of $N \times t$ Determined to the Base of $N_4 - t_{10}$				
	N_4	N_5	N_6	N_7
t_{10}	100	95	92	
t_7	92	90	87	
t_5			83	
t_4	-8	8	4	80
t_3	72			

Whence we derive the following equations or pairs of equivalent formulae:

$$\begin{aligned} (1) \quad N_4 - t_7 &= N_6 - t_{10} \\ (2) \quad N_4 - t_8 &= N_8 - t_5 \\ (3) \quad N_6 - t_8 &= N_6 - t_7 \\ (4) \quad N_6 - t_7 &= N_8 - t_5 \\ (5) \quad N_6 - t_5 &= N_8 - t_7 \end{aligned}$$

Of these, the analytical tables indicate that the most ideal combination of N and t , i.e., those which give the most evenly graded correlations between C and S , are: N_6-t_7 , N_4-t_5 , N_5-t_8 , N_4-t_4 and N_6-t_8 . The last one being the best. But as the closest one-to-one correspondence obtains between N_4-t_4 for post-cards and N_6-t_4 for tri-syllabic words, these two may be used with advantage for practical purposes.

This is the goal of our quest.

Summary of Tests with Picture Post-Cards

And now if we bring all our results together, we obtain Table 33, which shows the average per cent correct recognition for each value of S determined on the basis of all judgments rendered for all substitutions of that particular S .

TABLE 33

% S =	0	25	35	45	55	65	75	80	85	100
Total No. Judgments.....	409	150	228	379	389	447	437	90	230	470
% C =	100	92	87	77	67	55	45	34	21	100
Total + Judgments	389	77	184	227	228	201	122	12	22	
% P.K.	61	93	95	95	93	91	94	?	?	

This is the curve we should expect to obtain when groups of 4 items are exposed for 5 seconds.

There are two interesting questions that present themselves in connection with the above figures. (1) Why is it that the recognition of a 25% substitution is almost as high as that of a 0% substitution? (2) Why is it that % P.K. is so low when $S = 0\%$? Both of these facts came out in the word experiments as well as in these latter tests.

With regard to the first question the only explanation to be offered is that similarity in the visual realm must reach a certain minimal value before confusion will be produced in the recog-

nitive consciousness. This condition is entirely analogous to what we have in sensation—the stimulus must reach a certain minimal intensity before it will produce a just perceptible sensation. Likewise in ideational discrimination—no confusion between two stimuli will result until the similarity reaches at least 20%. Beyond that recognition seems to vary inversely as similarity in the ratio of $X : (100-X)$ expressed in terms of percentage.

The other question demands more careful consideration. It has already been remarked (page 42 ff) that the totally dissimilar impression seems to wipe out all memory of the previous one. The theory advanced was that owing to its great dissimilarity, it sets up a totally different brain process which, for the time being, eliminates the one previously induced.

Introspection bears this out. Substitutions of O similarity invariably produced a shock. Thus in Se IX-237⁷ the subjects say: A: + (+) V is new, 100% sure. I had a general impression of all cards and V did not fit in with the rest. It gave me a shock when I saw it. I know what was there before.

B: + (—) V is new, 100% certain. I knew during the interval what card was there before, but this card, V, knocked it right out of memory. This new picture post-card is so prominent that it seems to inhibit the other one every time I make an effort to call it up.

Se. III-311. A: + (—) V is new, 100% sure. I never saw it before. It seems like a thing in a wrong setting; it does not harmonize with the others. It gives me a new kinaesthetic feeling. I cannot recall what was there before.

Se. V-63x: H: + (—) As soon as the shutter went up the first time I went over all the cards and read some of their writing, portions of which I carried in memory during the interval, as well as the images of three cards. When the shutter went up the second time, I looked at some and found them familiar, but when I came to V it gave me the impression of a vacant lot among a group of buildings. It was like something which lacks meaning. If there had been no card there at all,

In his and subsequent readings of the cards the Roman
numbered cards were found to be more familiar than the others.

I don't think the feeling of emptiness produced in me would have been more pronounced.

Frequently the subjects would declare that they had a vague idea of what was there before, but they could not call it up in memory. Yet in some instances they would succeed in recalling N after an interval of a minute or two. Sometimes they would make a conscious effort, at other times they would remain passive, and N would come back of its own accord.

This occurrence led me to believe that perhaps they did not observe the cards as separate entities, but took in the group as a whole; in the same sense that one regards a painting as a composition and not as so many isolated figures or patches of color. Of course, this question once more reverts to our main problem. The fact that deceptive substitutions could be made with cards of 35 or 45% similarity shows that the single cards at least were carried as entities, for if their contents had been carried piece-meal no deception would have been possible at all.

That this was true, to a less extent, of the group as a whole, I found out in the following manner. When a zero substitution had been made, and the observer declared, on the second exposure, that he was positive V was new but could not recall what had been there before, I would remove one of the fillers and put N in its place, still leaving V where it was. Exposing this group once more, the subject would say, "Well, this is the same group you showed me just before, i.e. the second time." Whereupon I would take V out, put N into its original place, and return the filler. Exposing the group again, the observer would exclaim, "Oh yes, there is N. That's the card I could not recall." Or, "The whole group is now the same as the one you showed me on the first exposure. I recognize all the cards now that they are in their original positions."

Occasionally on the third exposure the subject would assert that N, being displaced, is a new card, and only when it was returned to its original place was it recognized as the original card, i.e. as the missing item. The following cases are characteristic of the many that occurred.

Se. III-311
4 (A-B)-O%S

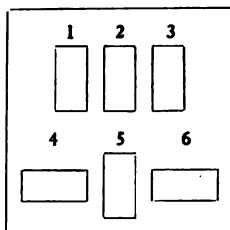


Fig. XI.

I: + (—) Exp. 2:⁸ 4B is new, 100% certain. I don't know what was there before.

Exp. 3. (6A-4B): Well, about four of the cards are the same as in the second exposure. There is one new one—the lower right—6A. I don't remember having seen it before.

Exp. 4. (4A): This is what I saw on the first exposure, and by "this" I mean the whole group. But I also know now that this lower left card, 4 A, is what I saw the very first time. But I could not recognize it when it was put in a different position. I don't know why, perhaps I remembered it in relation to the others.

J: + (—) Exp. 2: 4B is new, certain. I cannot remember what was there before. I don't think I could recognize A.

Exp. 3. (6A-4B): 6A looks like the replaced card, but I am not sure.

Exp. 4. (4A): I should say with more positiveness that 4A is what was there originally. The feeling of recognition is stronger when I see it in this position. There is a synthesis of the feeling of familiarity.

Se. X-67X
3 (A-B)-O%S

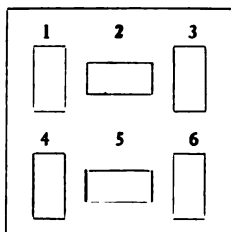


Fig. XI.

E: + (—) Exp. 2: 3 B is new 100% sure. I have not the faintest idea what 3 A was.

Exp. 3: (1 A-3 B): 1 A seems new. I have not seen it to-day.

Exp. 4: (3 A): Oh yes, 3 A is the card I saw on the first exposure. The reason why I recognize it this time and did not when you had it in position 1, is that the cards associate themselves in their respective positions, so that the whole group is like a design to me. Hence, when you put A, which I originally saw in position 3, into position 1, it looked strange; but when you put it back into its original position it seemed to fit in and I got the feeling of familiarity for that arrangement is such. This is true of all cards

that have been seen in the same position in the first exposure, etc.

except the gaudy ones, or those that are distinctive, such as portraits.

I: + (—): Exp. 2: 3 B is new. I am certain. I cannot say what was there before.

Exp. 3: (1 A-3 B): 1 A is new. I remember having seen 1 A some time ago, but when or where I cannot recall.

Exp. 4: Now it is all right. That is what I saw on the first exposure. It did not strike me at all that A was the displaced card when I saw it in position 1, i.e. on the third exposure. But now that I see it in its original position it comes back to me very strongly.

From these representative cases we see that it is the preserved arrangement of the cards which often facilitates recognition. If a card is displaced, it appears strange; if it is put back into its original place it looks familiar. In short, keeping the arrangement intact strengthens recognition. Now this cannot be explained on any other hypothesis but that every visual complex sets up a unified brain process; and if that same brain process is reinstated a second time in its entirety recognition will occur, if not, recognition will not occur.

What is true of the group as a whole is equally true of the single card. If there is no similarity whatever between the two critical cards, then the perception of the second, setting up a totally different brain process from that of the first, naturally destroys all memory of the first. To be sure this phenomenon does not happen every time. According to our figures it occurred in 39% of the cases. This is large enough a percentage, however, to justify the conclusion that, of two totally dissimilar visual impressions successively experienced, the later tends to inhibit the earlier. I say justify, because only a very small amount of similarity is required to produce almost complete recall. In these experiments substitutions of 25% S had a P.K., i.e. recall value of 93%.⁹

⁹ For the metaphysical significance of these facts, see my article on The Fitness of the Environment for the Continuity of Consciousness, *Jour. of Phil. Psy. and Sc. Meth.*, Vol. XI, No. 16.

Methods of Recognition

The method of recognition varied with different individuals as well as with different cards. But objectively it was determined by two general conditions. Either the new card would be recognized as new because it lacked something that was remembered from the old card; or else it would be recognized as new because it had a new element in it. Subjectively, the recognition of the new card arose from the fact that either it produced a jolt—a shock of strangeness, or else it induced a different mood, or it produced a different kinaesthetic feeling, or it did not fit in with the image of the first card. If there was any similarity at all between N and V, the sight of the latter would call up the generalized image of the former. Thus, if N were the picture of a blue bell swinging to the right and V the picture of a violet bell swinging to the left, the sight of V would revive the memory idea of having seen a bell before, but the subject would not be able to say whether that bell was swinging in a different direction or was differently colored.

Certain artifices were resorted to by some of the observers at an early stage of the investigation, namely, they would try to select earmarks—certain distinguishing features from each of the cards presented on the first exposure, and carry these in memory in the hope that if a substitution were made, it would lack the earmark. But they soon gave up in despair, for the cards contained so many different elements that the mere task of deciding which one should be chosen for retention was bewildering. And so they fell back on the general impression. Even the two subjects who continued to the end of the experiments to select what they thought were earmarks, did not do better in the matter of correct recognition than two or three others who relied on their general impression. The reason is that very often the earmark was no earmark at all. Was it a word that they picked upon? then not infrequently the substituted card would contain that same word. Was it a tiny monogram or a little corner of N? then very often

that same monogram would be found in the corresponding corner of V.

The extent to which the process of recognition investigated here was ideational is shown by the amount of intellectualization that went on in the minds of the observers as they looked at the first exposure: Processes of association, suggestion, pleasantness, unpleasantness were set up; some cards impressed them as sentimental, others as ludicrous, some were neat, others gaudy, some were very pretty, others decidedly ugly. In short, a distinct and unique idea was induced by each card, so much so that very often the observer would actively conceptualize the cards by classifying them in internal speech. That is to say, the content of the card, or better still, the impression produced by it would be given a name, a label. Is there anything that can be more representative of the life process? The appearance of one person "strikes" me as neat, i.e. causes me to attach ideationally the label of neatness to him, another person strikes me as careless; one as intelligent, another as stupid; one as having a firm character, another as a rake.

A few cases of introspection will bear this out.

Se. IV-192: B: +: V is new, 100% sure. The cupid is turned in a different direction. I had internal speech saying to myself, "Bridge, wagon." This internal speech comes with cards that are easily named. With other cards it comes only after their meaning has been realized.

E:—: No change, certain. I had inner speech when I first looked at the cards. Thus I had the ideas—and I think I said to myself, "Heart in the wheel-barrow, water wagon, bridge over the Charles River". These things I said to myself more or less completely. I don't think I had any articulation. Inner speech comes only when my attention is very active. Sometimes the inner speech returns during the interval when the image comes up.

Se. VI-106: J +: V is different. It has a different form. I am certain. I had images of all. When I first looked at the cards I had the ideas "Negro, engagement, law-abiding," while the card with the lavender color on it gave me an associa-

tion. In general, whenever I am in the mood, I make comments on the cards—or rather feel myself taking an active part in the ideational processes they set up, just as I do when I am out in the street sight-seeing.

A:?: I take into account the concept of the card, i.e. the story it tells, not merely its lines and colors as bare lines and colors. A change of ideas is very potent with me. I recognize the change when I notice that the new card tells a slightly different story.

But whether or not inner speech and active classification accompanied the act of perceiving, the impression that the observers carried away with them, from the first exposure, was invariably in a conceptualized form.

It was interesting to watch the conflict between the feelings of familiarity and strangeness which were induced by the same card. Some aspects of the card, the observers would say, look familiar, others strange. At such times some of the subjects would speculate as to whether the new element was simply the result of completer perception than was possible on the first exposure or whether it was the distinguishing mark of a new card. The kind of judgment that would be rendered in such cases would be determined by the relative strengths of the two feelings. Thus one subject says for Se. XII-155.

C+: V is new. I am positive. Some parts look familiar, but the feeling of strangeness I get from it is strongest. It occasionally happens that a new element crops out in a card, yet I do not say the card is new for that reason because the other elements in it are so familiar that they exclude such a judgment.

On the other hand, we have such cases as the following:

Se. XII-234: D—: On the first exposure I read the words, "St. Patrick's Day", on the card in the upper right hand corner. There was something else on that card, but I could not make out what it was before the shutter went down. Now, on the second exposure, I see that the words on the card (V) are exactly the same, but I also see that it has two children on it. Therefore I cannot say whether this is a case of the com-

pletion of an apperception or whether it is a new card. I shall say, however, that there is no change.

But there was a change. The two cards contained exactly the same inscription but the figures were entirely different. Now this observer, D, was one of those who relied on earmarks for recognition. He failed in this case because the earmark he selected was no earmark at all inasmuch as it was common to several things.

Sometimes the recognition of a new item would be affected by logical inference. Thus a subject may see card A with phrase B on it, and he later sees exactly the same card with a slightly different phrase, say one word altered. Knowing that the two phrases may function equally well for the content of A, he infers that the latter card is new.

The number of images that an individual carries during the interval bears no relation to his recognitive ability. Indeed, three of the observers would make no endeavor to recall images during the interval, because, as they said, such an attempt always caused mental confusion. Or sometimes the images of two or three items from as many different cards would fuse into one complex. These observers sat in a passive state during the interval. Two of them were among the poorest in ability to recognize, while the third was among the best.

Let us take a typical case. The card in question is Se. IX-235; its similarity is 75%. The standard (N) is a general card with the phrase, "Best wishes" written on top. In the background is a stately farmer's mansion. A hill with its downward slope to the bottom of the card. A stream of water runs down the slope. In the foreground stands a little girl, dressed in white, holding a bouquet of red flowers in each hand. She faces toward the right. The variable is the same as the standard except that the phrase now reads, "Many happy returns of the day." The farmer's house is of a different type. The child is now dressed in red, she faces slightly to the left and holds two bouquets of violets in her hands.

Here is how some of the subjects reacted to the substitution.

A + : I think the child in 3 A was in a different position.

I get a kinaesthetic shock from this card (3 B). I get the same sensation as when one first thinks of the right hand and then of the left. At first, attention was focussed to the right, now I find it focussed to the left. I do not know what other differences there are, though I had images of all. I will say 3 B is new.

C +: 3 B is new, certain. I had three images, but the moment the shutter went up the second time they vanished, yet the sight of each card brought its image back. Also the instant I saw 3 B the image of 3 A flashed into my mind. I notice that this is a different girl, she holds different flowers, and stands in a different position.

F —: No change. I had no images. I tried to think of 1 and 2 during the interval because I thought one of them would be changed, but the more I thought of them the more confused they became, therefore I dropped them from thought altogether.

I —: No change, certain. On the first exposure I got a general impression from them. During the interval I remembered with regard to the lower left card, (3 A), that there was a child, a hill, a bunch of flowers—all this I remembered in a general way; but I had no idea of color. This card (3 B) fits in with everything I remembered from the first exposure. Therefore it is the same.

J ±: It seems to me that the child I saw there before held red flowers in her hands, now she has on a red dress. I had the idea "red" associated with the child, but I do not know whether I substituted "flowers" or "dress" or what. Therefore, I am only 50% sure of a change.

That the function of the image in recognition is only secondary, is manifested by the fact that the subject himself often refuses to rely on it, because he has learned by introspection that it is constantly changing during the interval. Thus one observer says:

"I feel more certain of those recognitions which I make on the basis of the feeling of familiarity than of those which result from a comparison of the image with the impression

This, I think, is due to the fact that the images tend to become confused in my mind. Also they cause the attention to fluctuate and this fluctuation mixes up the details."

Another observer declares on the same occasion: "This flower-pot, (in B), looks a little larger than the one I saw before, (in A). But I do not know whether it really is larger or not. It may be an illusion because I know that sometimes my images grow during the interval and take on new features."

Thus to rely on mental imagery is not altogether a safe road to correct recognition. Nevertheless, the sight of the variable card would frequently call up the image of the normal, and then it was that the difference between the two—between the impression and the image—would fortify the judgment of recognition. But as this would happen only with a few subjects, it shows once more that the process of recognition is not wholly a comparison of two mental contents.

Compare these cases:

F: When I first saw V there was a sense of hesitation. Then the image of N came; I compared the present perception with the image and at once recognized the difference. All doubt vanished.

D: In this case the sight of V called up the image of N, I compared the two mental states and saw that they were different. But I get no difference of feeling on seeing a card of which I had an image and one of which I had none. The primary feeling of familiarity is the same. When, however, I see a card of which I had no image and the image "pops" up suddenly, then I become more certain of my judgment.

There were several states of affairs with regard to imagery: Either all the cards from the first exposure were carried in distinct verbal images; or all the images were distinctly visual with no verbal supplementation; or again, some of them were distinct and others vague; or all of them vague; or they formed a composite image; or else a few verbal images—such as a word or two from each card, were carried; or finally, no images were carried at all.

Some cards were more easily recognized than others—though

of the same degree of similarity—because they were distinctive or individualistic. That is, they were reproductions of famous paintings, or portraits, or of things that were especially familiar to some of the observers. The reason why these cards were easy to recognize is that they made a strong and unique impression on the first exposure. Where such individualistic cards cannot be obtained in sufficient numbers to make up the setting as well as the critical ones, they should be left out of the experiment altogether.

Objective Factors that Determine Recognition

A change of position, as left and right, up and down, was the most easily recognizable factor. But recognizability here depended on the ratio of the area of the transferred item to the total area of the picture post-card. The average size of a picture post-card is 5.5 x 3.5 inches. Hence, if a figure, whose area is 2 x 2 is transferred from one end to the other lengthwise, it will be recognized more readily than if transferred from one side to the other, i.e. breadthwise.

Next to change of position change of direction was most easily recognizable, especially if the pictures were those of human beings. And here recognizability was directly proportionate to the number of degrees through which the variable figure—face or body—was turned. A turn of 45° was less noticeable than one of 90° and that less than one of 180°. ¹⁰

Of colors, red seemed to make the strongest impression, i.e. the replacement of one red figure by another, slightly different, was easier to detect than the displacement of a blue one by another or a pink one by another, other things being equal. An interesting fact was the following: I was fortunate to obtain two pairs of cards, A-B and C-D, which differed as follows: The central figure in A-B was a girl's head wearing a blue bonnet against a gold background; in A the face was turned to the left, in B to the right. The central figure in C-D was the *same* girl's head wearing a red bonnet, of exactly

¹⁰ In this connection see, Dearborn, G. J., Recognition Under Objective Perceptual Conditions. *Psych. Rev.* 1901, 10, 171.

the same pattern as in A-B, also against a gold background. And in C the face was turned to the left, in D to the right. This, then, was all the difference that existed between the two pairs of cards—namely, in the color of the hats. My first surprise came when I noticed that the 15 judges gave the first pair a higher rating in similarity than to the second pair. The ratio was 9:8. My second surprise came when I noticed that the laboratory subjects were less able to detect the difference between A-B than between C-D. Here, then, we had objects that were identical in form and content, differing only in color; and the red color helped discrimination more readily than the blue. It may be, however, that the red color formed a stronger contrast to the rest of the card than the blue, and for that reason the position of the face—the critical part of the card—made a deeper impression in the one pair than in the other.

Human beings are more distinguishable in various attitudes than animals, other things being equal; and the sharper the features of the human beings the deeper the impressions they make. In general, animate objects are more distinguishable than inanimate. This shows that recognition is not dependent so much on mere sense perception and memory as upon the revival of mood and attitude. Else why should it be more difficult to notice that a house formerly turned to the right is now turned to the left than it is to notice the same kind of change with a human figure? Introspection shows that new states of consciousness, new attitudes were aroused by the latter but not by the former. Thus, if a girl's face is turned to the side in one instance and to the front in another, the observer would make some such comment as this: "The first girl did not produce the same feeling that I now have. She seems to be coquetting with me; at first she turns her shoulder to me, and now she stares me straight in the face." Or, "she had a more delicate appearance before than now," and so on.

Errors of Recognition

Owing to their complexity the cards did not afford as good means for the analysis of the errors of recognition as did the

words. We could not, for example, tell with exactness whether the error was due to an incomplete perception of the variable—an error of “is”, or to the obliteration of the normal—an error of “was.”¹¹ By far the majority of the cases point to the latter, which simply means that the sensory-motor force was, or rather is stronger than the ideo-motor force.

In general, the mistakes of recognition were directly due to the degree of similarity; and that means that the elements which the observers fixed upon for discrimination were found in sufficient proportion in the variable card to make this latter acceptable for the normal. This is the fundamental fact of our entire investigation. It receives further proof from an examination of the definite errors themselves, i.e. discarding the doubtful judgments and paying attention to the wrong ones only—those marked (—) in our tables—we find that the ratio of these to the total judgments increases concomitantly with each degree of similarity. The following table shows this.

TABLE 34
Per cent of Absolutely Wrong Judgments

% S =	0	25	35	45	55	65	75	85	100
% (—) =	2	7	12	15	21	34	53	78	2

This fact stands forth regardless of any other elements that may enter in. The reason is that these other elements, disturbing factors, or whatever they may be, would distribute themselves equally over the entire similarity scale, and thus leave the influence of each degree as compared with every other one unhampered.

The disturbing influences mentioned by the observers were so much analogous to those that tentatize the human mind in daily life that we were well pleased to find them operating in our investigation, for, it should be remembered, that we were investigating the recognitive process under conditions comparable to those of daily life.

Thus it would sometimes happen that a card of the milieu could be so attractive that the observer, with the result

that the observer would have his attention absorbed by the former and get only a vague impression of the latter. If he then mistook V for N or felt uncertain whether or not he had seen V before, he had no one to blame but himself. His instructions were to distribute his attention equally, and if he could not do so in the instance just cited, he would very probably still less be able to do so in real life under similar conditions.

Again, the variable card would occasionally be mistaken for the normal because it resembled it in general appearance,—it told the same story. Such cards were disliked by the observers because they had no distinctive features. They had meaning, to be sure, but they meant too much the same thing. Such was the complaint. But here again we have a condition that is duplicated in real life. Do not all infants, for example, look very much alike? Yet they are different. To the uninitiated American all Asiatics look alike, but they are really as distinct from one another as Americans are.

A fourth source of error was that of over-confidence. Some cards would appear to be easy to grasp, the subject would feel that they could not be displaced without his recognizing the change; and so he would only take a glance at them. But here again he was mistaken in his calculations, as most of us are when in a similar frame of mind. Very often such a card would be displaced and the observer would fail to recognize the change.

The final source of error resulted from the impression of familiarity produced by the whole setting. "I have a feeling of familiarity for the group as a whole, and therefore I don't think any were changed," was a remark frequently made. This led me to investigate the influence of the environment on recognition with cards as I did with words. To that we shall now turn.

EXPERIMENTS E

The Influence of Environment on Recognition

The problem concerning the influence of environment on recognition has already been stated. This ph

Will an object be more readily identified if seen in the same setting in which it was originally experienced than if observed in a new setting? Or in other words, does the feeling of familiarity or strangeness, as the case may be, which is aroused by the general environment color consciousness to such a degree as to make a new element appear familiar in the one case and an old element appear new in the other; or is the action one of contrast rather than fusion? If it is contrast, then the exposure of an identical element in a new environment should yield more correct recognitions than its exposure in the old environment. If it is fusion, then the percentage of correct recognitions should be lower for identical objects re-exposed in a new setting. That is, the feeling of newness, being heightened, should swamp the sense of identity and intensify that of dissimilarity.

Two methods for investigating this problem suggested themselves: either we could employ the same observers as in the previous experiments and new material, or new subjects and the same material. Since the results of this set of experiments were to be compared with those of the previous sets, we were anxious to keep the critical conditions as uniform as possible. It was deemed best to use the same material and different subjects.

Hence the series I made use of in this case were Nos. I, II, IV, and IX, whereby I exposed 4 cards for 10 seconds, 5 cards for 10 seconds, 4 cards for 7 seconds and 5 cards for 5 seconds. The method was as follows: The standard card, A was shown in the setting within which it had been exposed in the previous sets of experiments; then, after an interval of 20 seconds the variable card, B, was exposed in a totally different milieu but occupying the same position. The similarity between A and B varied from 0 to 100%. In the case of 0% S, the second exposure would contain nothing whatever that bore the slightest resemblance to anything in the first exposure.

The same apparatus was used. My observers were the same ones who participated in the word experiment.¹² They were already somewhat adjusted to the test and to make more sure,

they were given a week's practice. They came to me once a week, each at an appointed hour and day.

The instructions given to them were practically the same as those reproduced on page 35; only here they were told that on the second exposure there would exist one of three possibilities; either all the cards would be totally different from those of the first exposure, or one of them would be similar or identical to one in the first exposure.

I began with Series I—4 cards exposed for 10 seconds—and after that I turned to Series II. The next two series were exposed together. Obviously I was doing an injury to my theory which says that cognitive ability varies inversely as length of series and directly as length of exposure, for I was introducing the element of practice-improvement into the very series where I expected the theory to be substantiated, by putting them off to the last. But it seems to me that this is the only scientific way of procedure. For if I obtain low results with the shorter exposure-time, as I should, then they would have all the more value, for it would indicate that despite practice-improvement, the subjects are less able to recognize a substitution in a 5-second exposure than in a 7-second one.

The results of this set of experiments are given in the following tables.

Comparative Per Cent Correct Recognition when Variable Card (B) is Shown in New and Old Environment (N.E. and O.E.) Respectively.

TABLE 35
Series I: 4 Cards Exposed 10 Sec.

% S	% C		(N — O) E	No. Judgments	% P.K.
	N.E.	O.E.			
0	100	100	0	12	
35	100	90	10	4	100
46	100	87	13	12	91
56	100	82	18	12	100
65	92	78	14	12	100
74	94	67	27	20	90
91	25	10	15	4	?
100	81	97	—16	24	

TABLE 36
Series II: 5 Cards Exposed 10 Sec.

% S	% C		(N - O) E	No. Judgments	% P.K.
	N.E.	O.E.			
0	100	100	0	12	
25	100	100	0	8	100
46	100	78	22	12	70
50	88	76	12	8	66
64	69	65	4	8	85
74	69	54	15	8	50
100	87	96	-9	12	

TABLE 37
Series IV: 4 Cards Exposed 7 Sec.

% S	% C		(N - O) E	No. Judgments	% P.K.
	N.E.	O.E.			
0	100	100	0	8	
23	100	90	10	12	100
32	100	90	10	8	40
44	92	85	7	12	70
54	92	78	14	12	85
65	87	65	22	20	95
72	62	63	-1	12	100
88	56	20	36	8	
100	70	97	-27	38	

TABLE 38
Series IX: 5 Cards Exposed 5 Sec.

% S	% C		(N - O) E	No. Judgments	% P.K.
	N.E.	O.E.			
0	100	100	0	12	
23	100	79	21	12	60
34	100	74	26	12	66
45	95	67	28	11	40
55	88	61	27	12	60
64	73	35	38	15	40
73	75	40	35	12	66
85	50	20	30	8	
100	54	91	-37	47	

At first sight it would appear that these tables present contradictory results. For we notice that those cards whose value is less than 100% S are discriminated *more* often in the new setting than in the old, whereas the cards of 100% S are identified *less* often in the new environment than in the old. According to the first fact it would seem that the new milieu sharpens discrimination according to the second result.

that of identification. But that is not so. Instead of being contradictory the results are supplementary.

The first important fact that stands out before us is this, namely, it is more difficult to identify the same thing in a new environment than in the old. The falling off in recognitive ability is 16, 9, 27 and 37 per cent respectively in each of the four series employed. Hence it follows that the feelings of familiarity and strangeness do not enhance each other by contrast. For suppose they did. Then not only ought more identifications to be made in the new milieu when an identical item is repeated, but more items whose similarity is greater than 50% ought to be falsely recognized in the new setting than in the old. The reason is that in proportion as the similarity increases it should be enhanced by the surrounding field of newness and thus precipitate the judgment of "the same." But just the opposite is true; cards whose similarity varies between 50 and 100% S are less often mistaken for each other in the new environment than in the old.

There are, therefore, only two possible ways of accounting for this phenomenon. (1) Either the mass of new material, flooding consciousness, obliterates all memory of past perceptions and thus precipitates indiscriminately the judgment "new," whether anything is new or not; or (2) the feeling of strangeness produced by the new environment predisposes the mind to judgments of difference on the principle of suggestibility, said predisposition varying directly as the mass of the new field and inversely as the intensity of the repeated item.

The first theory seems to find substantiation in the fact that the per cent perfect knowledge (% P. K.) is smaller when the environment is new than when it is old. That is to say, after seeing the variable card in the new setting and rating their judgments of change at a 100% certainty, the subjects were not able to state what the corresponding normal was as often as when they saw the same variable in the old setting. The only explanation for this is that the perception of a mass of new material obliterates the memory of the old—just as we saw that a variable of 50% S will wipe out all traces of its mate

If this were the correct explanation, however, then there is no reason why C should vary inversely as S when the environment is new as well as when it is repeated. For the new field that surrounds a card of 72% S, let us say, is just as large for any given series, as that which surrounds a card of 35% S, therefore obliteration in one case ought to be as complete as in the other. Hence if C depended on obliteration alone it ought to have the same value in both cases.

But it has not the same value for all degrees of S. On the contrary, C varies in a uniform manner inversely as S. Moreover, it varies as E (environment). Therefore, other things being equal, we are justified in saying that C is a function of two variables: S and E. But are we not plunging into a difficulty here? It is our desire to establish a definite relation between C and S alone, and heretofore we have treated these factors conjointly as if no third one existed; and now when S appears in a new environment we are trying to say that the environment functions also. Admitting that N.E. tends to inhibit identification by predisposing the mind to say "change"; does not O.E. tend to inhibit discrimination in a similar manner by predisposing the mind to say "the same?" If so, then all the relations that we have thus far established between C and S are invalid because the old environment (O.E.) functioned all the time.

To allay these misgivings it is only necessary to say that this factor (O.E.) has already been taken care of by the first part of our Correction Formula. $(100-S) Y = K'$. That is to say, the extent to which the old environment has a tendency to precipitate the judgment of "the same," or to inhibit discrimination between two unlike things is directly proportionate to the number of false judgments made for 0% S substitutions in O.E., and this value is 2%, (Table 34). Therefore, $O.E. = .02$, or it is almost negligible.

Every time we made use of the correction Formula we took into account the influence of O.E. Hence we are justified also in trying to determine the value of N.E. Now it happens that the per cent of errors made when 0% S is substituted in

O.E. is much less than when 100% S is substituted in N.E. Does that mean that two totally dissimilar cards are much more unlike than two identical cards are identical? Such a thing is possible, i.e. two dissimilar cards may have not zero similarity but minus zero similarity, whereas two identical cards can never have more than absolute identity. I do not think, however, that this argument affects the results. Because in so far as I selected my material from the same universe of discourse—picture post-cards—my fifteen judges had a right to rate all cards that had no resemblance in them at 0% S. Therefore, since the old environment did not cause as many mistakes to be made with 0% S substitutions or even with 35% S substitutions as the new environment causes with substitutions of 100% S, we are justified in concluding that O.E. has not as great a tendency to predispose the mind to judgments of “the same” as N.E. has to predispose the mind to judgments of “different.”

What is the value of N.E? To determine that we simply have to note the extent to which it causes the identical item to escape detection minus the amount of the same kind of error which occurs in O.E. This difference, as given by the four preceding tables is 16%, 9%, 27% and 37%, the average of which is 24%. Therefore, $N.E. = .24$. That is to say, the tendency of a new environment to cause an identical thing to escape detection is 12 times as strong as is the tendency of a repeated setting to cause a totally dissimilar thing to be falsely recognized. Hence it appears that the feelings of familiarity and strangeness tend to neutralize each other by fusion, that one giving the central tendency to thought which is induced by the strongest stimulus.

This is not all, however. The fundamental problem that confronts us at this point is to find the relative values of O.E. and N.E. with respect to the identification of cards of 100% S. This is the only way we can establish an independent relation between C and S.

In the four series under consideration an average of 5 per cent error occurred when all 5 identical cards were repeated,

but it rose to an average of 27 per cent when only 1 card was repeated in a setting of 4 new ones.¹³ The probable per cent of error that fell to the lot of any one particular card in the former case is $5\% \times \frac{1}{5} = 1\%$. Subtracting this from the latter result in order to neutralize the precipitant tendency to say "change," leaves 26% as the amount of error due directly to the influence of N. E. Therefore, under the conditions of our experiment, the probability that a thing will be recognized when seen in the same environment is $\frac{99}{100}$, whereas the probability that it will be recognized when observed in a new setting is only $\frac{74}{100}$.

These figures harmonize so well with those obtained in the experiment with words that they must stand for fact and nothing else.¹⁴

And now let us see if we can account for the fact that discrimination appears to be easier in the new setting than in the old.

The figures given in the second horizontal line of the summary table, (page 109), represents the judgments of "change" or "all new." Now, as a matter of fact, *all* cards were changed objectively in every instance except where $S = 100\%$.¹⁵ And though the observers were able to point out the similar card in the new environment, they were not able to state in more than 75 per cent of the cases how it differed from its analogue. Therefore, inasmuch as the new setting caused the subjects to declare in 27 per cent of the cases that all cards were new when one identical card was repeated, it must also have caused them to make the same kind of a judgment when a card of less than 100% S was substituted irrespective of whether they perceived the difference between this card and its analogue or not. Only we must not assume that the tendency was just as great with each diminishing degree of similarity. For certainly when a card of 65% S is substituted the amount of difference which it would have tendency to elicit the judg-

¹³ Actually the mean of the two groups was 27 per cent for 4 and two groups for 2.

ment "change," and likewise when a card of only 25% S is substituted it would have a still greater tendency of its own to elicit a judgment of difference. And so in general we may say that in proportion as S recedes from 100% in that proportion does the influence of the environment proper diminish. This is the fundamental principle upon which the correction formula was constructed. It was explained, (pp. 36-39) that, inasmuch as there seems to be a simple inverse relation between recognitive ability and degree of similarity, the tendency of the setting to precipitate a judgment of identity with respect to a thing that is different is $(100-S)Y$, which value should be added to the % C corresponding to the S in the expression; whereas the tendency to say "change" when no change exists is $SX \cdot \frac{A}{B}$, which value should be subtracted from % C.

Now this latter is the factor that has to be employed in the present case, inasmuch as the only apparent error is that made with respect to identical cards. Only here $A/B=1$ because only one card was repeated, i.e. the error of 27% was made with respect to one card. If now we multiply each value of C in N. E. by the formula $0.27 \times S$, (since $X = .27$), and subtract the product from the original value, we shall obtain almost exactly the same values that C possesses in O. E. Thus we derive an independent relation between C and S, and incidentally prove that the old environment does not tend to produce false recognitions to the same extent that the new environment tends to produce false discriminations.

The following table contains the corrected results for recognition in the new as compared with the repeated setting.¹⁵

TABLE 39

Comparative Per Cent Correct Recognition when Cards of the Same Degree of Similarity were Substituted in New and Old Environments.

% S	0	25	35	45	55	65	75	85	100
% C in N.E.	100	93	91	83	77	62	55	21	72
% C in O.E.	98	90	85	79	74	61	56	17	99
% C (N — O) E.....	2	3	6	4	3	1	-1	4	-27

¹⁵ For the practical application of these results, see article by the author on The Influence of Environment on the Identification of Persons and Things *Journal of Criminal Law and Criminology*, Vol. V. No. 1.

The different values of C corresponding to the same % S are so close to each other that they prove conclusively that the corrected relations established between C and S in O. E. are independent of the setting. The average difference between % C in N. E. and % C in O. E. for values of S that are greater than O and less than 100% is 3. This may be interpreted as meaning either that N. E. facilitates discrimination to that extent or that O. E. inhibits discrimination by that much. But since we find directly that the average per cent of false recognitions due to O. E. is 2 per cent when substitutions of 0% S are made, it would seem that the latter interpretation is the correct one.

I must caution the reader once more not to assume that the difference in ability to recognize an identical item in the two sorts of environments is due to retardation of perception occasioned by the new setting. No such retardation occurs. Introspection shows univocally that the new surrounding is recognized as new instantly; that the cards which compose it receive the barest glance and are immediately dismissed from consciousness.¹⁶ Hence more time, if anything, is afforded to the observer to dwell on the critical card and to determine whether it is only similar to or identical with something he had seen in the first exposure.

Thus one subject says: "When all cards but one are changed, I readily eliminate the new ones and spend more time on analysing the repeated or similar one. Whereas if all cards but one are the same, I have to study each one before I can determine whether it is identical in all respects with what I saw in its place before."

And again: "The cards constituting the environment do not give me any shock. The more different they are from those I saw in the first exposure, the less effect do they have on me. I 'slide' right over them before they rise to the center of consciousness. But the more similar a card is to one previously seen the more does it disturb me. I have to look at it until I fully determine whether it is new or not."

¹⁶ See also Katzev of *op. cit.* p. 57

Yet it must be acknowledged that not all observers were able to say definitely whether the new setting did in itself produce the feeling of strangeness. It would seem that the acknowledgment of such a state would be necessary in order to substantiate the theory of fusion. With some observers a decided consciousness of the feeling of strangeness comes only when they see a similar card. Thus subject B declares on one occasion, "There was an immediate feeling of strangeness produced by the variable card, but not by the others. I recognized them as new instantly. It is only when I am doubtful that the feeling of strangeness or familiarity makes itself felt." Nevertheless, we may conclude that inability to recognize an identical object in a new environment is due to the suggestible influence of newness or strangeness arising from such environment.

So much for the results that pertain to the material. Let us now turn to those that concern the observers.

Individual Differences

When we come to examine individual differences we find that they are larger in the methods or devices employed in recognition than in the actual results themselves. Some observers thought they did best when they actively clung to their images during the interval—they are C, G and J. Others felt that the mere endeavor to retain the images produced mental confusion, so they remained passive during the interval—they are A, B, F and I. Again, some tried to select an essential element from each card and use it as an earmark—they are D, G, A; while others merely tried to get a general impression of the entire card—they were B, F, I, H and J. Some would go over all the cards rapidly, thus getting several hasty impressions from each; others would first obtain a general impression from the whole group then scrutinize each card carefully; others still would first examine each card carefully and then, if there was any time left, get a general impression of the entire group.

When we examine our subjects with respect to their accomplishments, we find that they fall into three general classes.

(1) Those that are determined in their judgments—not given to much doubt. They are above the average in accuracy, but no better than the average in inaccuracy. (2) Those who are subject to doubt and hesitation—their errors are below the average while their accuracy is the same as that of the former group. (3) Finally, those whose accuracy is below the average, whereas their doubt and inaccuracy are above the average. The following table contains the per cent of accurate (+), doubtful (\pm) and false (—) recognitions made by each of the ten observers who participated in Experiments A to D inclusive.

TABLE 40
Per Cent Distribution of Judgments.

Sub- jects=D	1		2				3			Av.	M.V.
	E	C	A	G	B	F	J	H	I		
+=	64	60	61	63	64	64	53	42	45	58	6.8
\pm =	12	11	15	15	18	18	22	21	11	15	3.5
— =	24	29	24	22	18	18	25	37	44	27	6.1

That two of the observers, H and I, vary so much from the others is to be accounted for by racial differences and early training. Though not of the same race, their early bringing-up was such as to render them unacquainted with the sentiments and ideas expressed in many picture post-cards. Such cards, therefore, did not have the same meaning to them—hence not the same apperceptive values—as to the other eight observers who were all Americans.

It is significant that the average per cent correct recognition which I obtained, 58%, is remarkably close to what Katzaroff got with totally different material and by a different method of experimentation. His value was 50%.¹⁷ This is lower than my average, but that is probably because his material was less meaningful, and Hollingworth has shown that the recognition of an object varies directly as its meaning.¹⁸

There appear to be no differences due to sex.

¹⁷ *Op. cit.*, p. 34.

¹⁸ *Op. cit.*

CONCLUSIONS

If we bring our main results together we arrive at the following conclusions:

(1) Recognitive ability varies inversely as degree of similarity.

(2) When objects of approximately the same complexity as ordinary picture post-cards are exposed in groups varying between 3 and 6 at the rate of 1 per second, the inverse relation between recognitive ability and degree of similarity, expressed in percentage, is as $x : (100-x)$.

(3) The same relation holds for words of two or three syllables exposed in groups varying between 6 and 8 at the rate of 1 per $\frac{1}{2}$ sec.

(4) With such objects as picture post-cards the relation between the number of objects exposed and length of exposure, in terms of seconds, is as $1 : (1 + \frac{1}{2} - x)$, where $x < \frac{2}{3}$ seconds.

(5) The objects must reach at least 25 per cent similarity before they can have a confusing effect on the process of recognition.

(6) The process of recognition is affective and is independent of cognition.

(7) The feelings of familiarity and strangeness inhibit each other by fusion.

(8) It is more difficult to recognize an identical object in a totally different setting from the one in which it was originally seen than in the same setting, the ratio of difficulty being about as 27 : 1.

(9) It is more difficult to recognize an identical item in a new setting than a totally different item in the setting previously experienced, the ratio of difficulty being about as 12 : 1.

(10) Recognitive ability varies inversely as the number of objects perceived—time being constant.

(11) Recognitive ability varies inversely as the number of objects exposed, this being proportionate and not more than one second per object.

(12) Recognitive ability varies directly as the temporal length of perception.

(13) Some of the errors of recognition are due to the incomplete perception of the object when first experienced.

(14) Some cases of false recognition are due to the incomplete perception of the mistaken object, owing to the superiority of the ideo-motor force over the sensory-motor force.

(15) But most cases of false recognition are due to the obliteration from memory of the original impression by the present perception, and this is due to the superiority of the sensory-motor force over the ideo-motor force.

PART IV

THE APPLICATION OF THE LAWS OF RECOGNITION

As a result of our investigation we have come to the conclusion that two such words as "Camphenol" and "Camphenyl" for example, which stand for two absolutely different proprietary medicines, have a psychophysical similarity of 80 per cent. We found that such similarity will produce approximately 80 per cent confusion in discrimination as compared with the ability to discriminate words of less than 20% S and to recognize words of 100% S. It is hardly necessary for us to state under what conditions this holds true. It should be understood, once and for all, that we mean under conditions comparable to those that obtain in daily life. When the chemist says that ammonium hydroxide will precipitate iron, it is understood that he means under ordinary temperature and pressure and out of a neutral solution. So here, too. After careful investigation we have found, for instance, that words which have a certain number of symmetrically arranged letters in common possess thereby a definite amount of similarity. It has been found that it is most convenient to measure this similarity in terms of the mathematical ratio that the number of commonly possessed, symmetrically arranged letters bear to the total number of letters.

Finally, it has been found that similarity and discrimination stand in the same relation as the reciprocals of their per cent.

Let us now see how these facts can help us in the solution of a practical problem.

As I write these lines I have before me the abstract of a report delivered in 1911 by C. A. Mayo, Esq., before the American Pharmaceutical Association on Pharmaceutical Nomenclature. Referring to the confusion which frequently arises with disastrous results from the similarity of drug names, he says:

In Richmond, Va. . . . a death occurred from the dispensing of "lysol" instead of "axol" In Connecticut damages were

recovered from a druggist who sold "calomel" when "columba" was ordered. In Germany a fatal accident was reported in 1908 from dispensing a solution of "chlorinated lime" when "calum chloride" was called for, and in Russia a child was killed through dispensing "saccharin" when "saccharum" was ordered. "The occurrence of these cases which have reached the public notice," says the author, "indicates the probability that many such incidents occur which never become public. *That such incidents do occur with increasing frequency is a matter of common knowledge among druggists everywhere.*"¹

The report contains a rich list of names of high similarity, representing different proprietary medicines, drugs and chemicals. We select only a few for illustrative purposes.

(1) Acetin	(6) Analgesin	(11) Benzine	(16) Cerebine
(2) Aceton	(7) Analgine	(12) Benzene	(17) Cerebene
(3) Acetone	(8) Apinol	(13) Benzoin	(18) Cerberin
(4) Alcotine	(9) Apetol	(14) Camphen	(19) Cerberine
(5) Alcotone	(10) Apicol	(15) Camohene	

This is enough. Now what confusion is apt to arise from such a babel of names as these? Let no one say that the results of our investigation cannot be applied to the above case, because druggists, being supposed to have a first rate acquaintance with such names as the foregoing, would not be confused half as often as the layman may think. To such argument we reply that we were aware of this fact only too well, and for that reason we did not use technical or difficult words on our subjects, but on the contrary, words that were as familiar to them as the above names are to the average druggist.

Nor should it be assumed that because the druggist knows the danger which is apt to result from confusing one name with another, he will therefore be more careful and will make less mistakes than one would infer from my tables. To this argument we need only answer that it is as criminal to tax the mind with unnecessary burdens as the body. The fact that the engineer knows his train will be wrecked if he runs past a danger signal will not be sufficient to save him if it is ill an-

ranged or if he is color blind. But the cases cited by Mr. Mayo, himself a member of the drug profession, constitute a sufficient answer to such objections. If we examine the mathematical similarity of several of the above named words, we find that they possess the following values: (1-2) = 80% S, (2-3) = 85% S, (4-5) = 87% S, (18-19) = 94% S. The probable mistakes in recognition that would result from these words among druggists, i.e. persons acquainted with them, are: 80, 85, 87 and 94 per cent respectively.

Now, it is either for the drug profession or for the law maker to decide what constitutes a dangerous degree of similarity. We have determined how the various degrees operate, just as the dietician ascertains the effects of various food preservatives on the tissue. Personally, I am convinced that objects of more than 60 per cent similarity cannot be discriminated with sufficient ease by the average person, exercising the usual amount of care which we can reasonably expect him to exercise, under the conditions of normal life. This value, or any other which the law makers happen to decide upon, may be too high for some persons and too low for others. The former will claim that it fails to protect them, the latter that it is too arbitrary. But this is the complaint heard about every law—from the national tariff to the speed ordinance of the smallest New England village.

The present investigations indicate that objects of 60 per cent or more similarity will be mistaken for one another at least 50 per cent of the times out of every ten observations. Suppose, then, that we have words, or names, or trade marks, or what not, the similarity of which we wish to discover? The simplest method is to expose one of these items among others of the same category, for such time and under such conditions as the skilled psychologist shall suggest. This exposure should be made to ten or twelve persons of normal eye-sight, and then it should be noted how many of them discover the substitution when the first item is replaced by the second. My own suggestion is that one of the formulas N_4-t_4 , N_5-t_5 , N_6-t_7 should be used with items of some complexity, and that N_8-t_4 with words or names.

What is the advantage of such a method? it will be asked

Is it not the height of absurdity first to build up a scale of similarity by subjective judgments, then to establish a relation between recognition and these judgments, and finally to turn round and declare that subjective judgments of similarity are valueless—only the actual discrimination judgments are indicative of the similarity of the objects? If the comparison judgments of 15 college men are good enough to determine the similarity between two objects, in the construction of the similarity scale, then those of 12 jurymen are equally reliable in suits involving trademark infringement, and there is no reason why the court should burden itself with psychologists and psychological paraphernalia.

Moreover, the mean variations among the psychological judgments are smaller than those of the discrimination judgments, hence the former must be more objective, i.e. more representative of the actual similarity of the objects, than the latter.

I have purposely given this argument at some length in order that we may the more tenaciously cling to the truth when we have arrived at it. How shall we answer this hypothetical challenge? The task is not difficult.

It is the utmost desire of all court proceedings to render a verdict solely based on the evidence of facts—a verdict the justness of which is not open to doubt. But in civil suits involving illegal imitation the presentation of unimpeachable facts is next to impossible.

Infringement of trademarks exists, according to the British Lord Ordinary's definition, "when they are liable to be mistaken by ordinary purchasers, not applying their minds closely to the matter, for the trademark of the goods of the trader who is the proprietor of the mark."² This definition has been closely adhered to by some courts and the unwary purchaser protected. But not so in all cases. As the *New York Times* has remarked editorially, "Usually when a case comes to trial, the trademarks in question are brought together for comparison by the judge and jury, who are thus accorded a privilege denied the unwary purchaser, and the comparison is aided and

emphasized by able counsel. In every way and by suggestion the difference between the two marks is accentuated, and, perhaps unconsciously, the learned judge projects his enlightened mind upon that unwary purchaser, until he endows him with his own sagacity and wisdom." To be sure, the testimony of persons who have actually been misled by the imitation is valuable, but unfortunately "the courts too often set them down as extraordinarily careless or foolish persons, devoid of common sense in comparison with the unwary purchaser of legal fiction."

And so we might say that by the time a decision is rendered, that which originally was a matter of fact, has ultimately become a matter of suggestion, of theory and cogitation, so that neither judge nor jury are in a position to decide with any degree of certainty whether two things are or are not similar enough to constitute an illegal imitation. This is evidenced by the large number of diametrically opposite decisions which have been handed down in almost identical cases of trademark infringement. We shall cite only a few.⁸

(1) In the case of the Sterling Remedy Co. v. Spermine Medical Co., in the Circuit Court of Appeals in the Seventh Circuit, 112 Fed. R., 100, there was involved among other things, the right of the complainant to enjoin the unnecessary imitation of the hexagonal form of the Cascaret tablet. It was contended that complainant's product was to some extent identified by the form of the tablet and the defendant's imitation of this form was unnecessary. The court rendered a verdict for the complainant.

(2) Globe Co. v. Brown, 121, Fed., 90; the imitation of the *form* of a letter file was enjoined.

(3) Yale and Towne v. Adler, 154, Fed., 37; the unnecessary imitation of the form of a padlock was enjoined.

(4) Wrigley v. Larson, 195, Fed., 568; an imitation of Wrigley's Spearmint Gum Package was enjoined.

"There are many other cases from which jurists might safely

⁸ For the cases herein cited I am indebted to Mr. G. M. Besett, Chairman of the Committee of Trademarks for the National Wholesale Druggists Association.

conclude that the unnecessary imitation of a non-functional part of a machine or piece of merchandise was unfair competition and enjoined," we are told. But let us see how the same matter was treated on other occasions.

(5) *Pope Automatic Merchandising Co. v. McCrumb-Howell Co.*, 191, Fed., 979; the Circuit of Appeals of the Seventh Circuit refused to enjoin the defendant from imitating the physical appearance of a hand vacuum cleaner.

(6) *Mississippi Wire Glass Co. v. Continuous Glass Press Co.*, 81 Atl., 374; Vice Chancellor Stevens, of New Jersey held that the complainant could not claim a right in a hexagonal shape of mesh for wire setting inserted in a fire retardant glass, although it was established by a reasonable amount of testimony that complainant's product was identified by means of the appearance contributed by the shape of the mesh. [Compare this with case 1.]

(7) *Rathbone, Lord & Co., v. Champion Steel Range Co.*, 189, Fed., 26; the Court held that so far as concerned the ornamental dress of a stove, the defendant was free to copy. [Compare this with conclusions of cases 1, 2 and 3.]

(8) *Edward Hilker Mop Co., v. United States Mop Co.*, 191 Fed., 613; it was held that the defendant could not be enjoined from imitating the visual appearance of the complainant's mop. [Compare this with case 2 and 3.]

These cases, representative of scores that could be cited, will suffice to show how absolutely impossible it is to decide by present day methods, how much similarity does or does not constitute unfair imitation. Obviously these diverse decisions could not have been rendered on the evidence of facts, because facts are incapable of opposite interpretation.

Now this method of ascertaining the legitimate or illegitimate similarity of two objects is outwardly equivalent to the subjective judgments rendered by our 15 individuals whereby the amount of similarity between pairs of picture post-cards was determined. I say outwardly because there the comparison ends. For the rest, the methods were totally dissimilar with the balance of favor on our side. The reason is this. In the court room

the opinions of judge and jury are swayed if not determined by the persuasive arguments of opposing attorney. Of course all items that are not absolutely identical have some distinctive features about them, and in so far as they are held up side by side, and clever arguments and suggestions are made by an array of able counsel, these features, however small, may loom up very large and vice versa. Under such circumstances a naïve, matter-of-fact sort of judgment can hardly be rendered. Yet that is the kind of judgment that is necessary in cases like these, because the faculties whereby we recognize and discriminate things in the physical world are primarily naïve. In short, recognition and discrimination are primary processes—they do not belong to the cognitive faculty, but are midway between cognition and affection. Therefore, it is fundamentally wrong to try to determine by logic and argumentation whether two things are or are not similar enough to cause confusion in perception, memory and feeling.

In so far, then, as we avoided all forms of suggestion by taking our 15 judges separately into the quiet laboratory, and allowed them to decide the similarity of the cards on the basis of "first impression," we think that their judgments were more representative of objective similarity than any court can reach.

Yet even this method is not wholly reliable. The process of comparison is not the same as that of recognition. In the former case you have the items directly before you. The impression of one can be superimposed on the image of the other; the items may be examined analytically or synthetically, i.e. the observer may fix his attention now on the parts that are identical and now on those that are different, or he may take in the total impression. Recognition involves no such processes as these. Hence, to try to determine recognitive ability by the comparison method is like trying to pass judgment on the quality of symphony by picking out its tones and over-tones by means of resonators.

In all matters of law the merits of the case alone must engage the attention of the court. In supposedly illegal imitation the point at issue is the similarity of impression made by two

objects separated in time and space. Therefore to bring those objects in close proximity to each other and to observe them at the same time, is entirely contrary to the issue involved. It is like attempting to ascertain the amount of adulteration in a food-stuff by its taste or appearance. Nothing but a psychological test on recognition can determine the amount of confusion that two objects possess for the average human consciousness. There is nothing derogatory in the application of scientific methods to court procedure.

The reason why a fair decision on illegal imitation can be reached only by resorting to such a psychological test as we have decided is that there we can determine with mathematical accuracy precisely the amount of confusion produced by the two things under consideration. The results rendered are not open to doubt. The experiment may be carried on either in the presence of the court or in the quiet laboratory of the psychologist. Let us present a concrete illustration.

Suppose it is such a case as that of the Crystalleid Water Co. v. Schultz, 2 Trademark, Rep. 251. Here the court held that there was no unfair competition in the use of a bottle similar in shape, containing a liquid similar in color to that used by another firm provided the label was distinctive. An opinion diametrically opposite to this was handed down in the United States Court of Appeals in the case of the Moxie Co. v. Daniel Daoust. The court's ruling was in part as follows: "Moxie is a well-known beverage put up in a bottle which is of a distinctive shape for many years associated with the beverage Moxie. The defendant makes a beverage quite similar in color and taste. This is put up in bottles, which in color and shape, closely resembles the Moxie bottle. The similarity of the beverages in color and taste together with the similarity of the bottles, facilitates the substitution of the defendant's beverage for Moxie without detection by the customer. There are points of difference in labels blown into glass and affixed thereto, in caps, etc., but they are differences which do not destroy the general similarity of appearance to the ordinary purchaser and therefore are of no consequence, and should not be dealt upon

Both from our own inspection and from the evidence in the records we are satisfied that the resemblance is such as to mislead purchasers." The case was remanded to the district court with directions to enter a decree of injunction.

In both these cases such terms as "distinctive," "similarity in general appearance," "our own impression," etc., are used in a subjective sense. Hence the diverse decisions.

Why should the defendant in the suit of the Crystaleid Water Co. v. Schultz have been allowed to imitate the bottle of the plaintiff, and why should he have been enjoined in this latter case? In one or the other of these instances there has been a miscarriage of justice. The following method might have been used with more equitable results.

(a) The subjects on whom the experiment is to be performed are chosen on the basis of being representative of the average normal individual. (b) The court psychologist constructs his apparatus to meet the situation at hand, but in all instances he must see that it separates observer from experimenter. (c) He must then adjust his experimentation so as to find the ideal formula for the particular object in question. That is, if it happens to be the alleged imitation of a bottle, as in the above case, he must find that combination of the number of bottles (N) and time of exposure (t) which, with N having the highest and t the lowest values possible, two totally dissimilar bottles will be discriminated by the subjects 100 per cent of the times and two identical bottles will be identified 100 per cent of the times when one is replaced by the other at intervals of 20 or 30 seconds. (d) A totally different bottle would be one of different shape and color but approximately of the same volume, for surely the complainant cannot ask that the defendant be enjoined from selling his goods in any bulk he pleases. (e) The apparatus may be so constructed as to expose the items to the ten or twenty observers assembled and have them record their own results. (f) Or the experiment may be carried on with one observer at a time and have him dictate his results either to the court clerk or to the person conducting the investigation. (g) Three exposures should be made to each observer: (1)

One bottle—or whatever the item may be—should be displaced by a totally dissimilar one: This will test the subject's ability to recognize substitutions of zero similarity, (0% S). (2) Within a wholly new field but of the same complexity as in the previous case, the complainant's bottle or item should be exposed and then replaced by defendant's. (3) A totally new setting of the same complexity as in the previous cases should be exposed and nothing displaced on the trial exposure—this will test the observer's ability to identify the same item (100% S). (h) The observers addressed individually or together, according to how the experiment is conducted, should be given the following instructions: N objects (named) will be shown for t seconds, after which a period of p seconds will intervene. During this interval you must hold yourself ready for a second exposure; for at the end of the interval N objects of the same category (named) will be shown to you again for t seconds, and thereafter you must record in writing or dictate to the clerk whether the second exposure differed from the first in the presence of a new item or whether it was identical with the first. And if you think you see a new item (named) in the second exposure you must give its location in the group—or its number if they are numbered—and you must state what it has displaced and how it differs from the displaced object.

Thus we shall obtain a number of judgments in each of the three instances. Suppose there are twelve of them and that they are distributed in the following manner: First test: 11 subjects recognize the substitution definitely and 1 is in doubt. Second test: 3 observers recognize the substitutions positively, 4 are in doubt and 5 fail to recognize it completely. Third test: 10 identify the group as being the same positively, 1 doubts a single item, and 1 is sure that one of the items is new.

Calling the resemblance of the two totally unlike objects 0% S, of those that were not changed, i.e. in the third trial 100% S, and of those whose similarity we are trying to determine x% S, we can arrange the results thus:

% S =	0	x	100
+	11	3	10
±	1	4	1
—		5	1
Therefore % C =	96	42	88

Now apparently either the conditions of the experiment were not ideal or some of the observers were not absolutely normal, else the 0% S substitution would not have been missed 4 per cent of the times nor would the observers have said that a substitution was made 12 per cent of the times when none occurred at all. As a result of the first error the defendants have a right to say: If the observers failed to discriminate the difference between two things of 0% S, 4 per cent of the times than at least that much ought to be subtracted from the amount of confusion produced by our item, or that much should be added to the % C (correct recognition) when our item was substituted. As a result of the second error the complainants might argue: Owing to the nature of the experiment or the idiosyncrasy of two observers there was a tendency of 12 per cent to say "change" when none was actually observed. The probable per cent of this tendency with reference to any one particular item in the group was $12 \cdot \frac{1}{B}$, where B is the number of objects exposed in the group. Hence that much should be subtracted from % C with reference to x, as being due to the precipitant tendency of thought, under such conditions, to express itself in the judgment "change."

To settle these differences we apply our Correction Formula: $(100-S)Y - SX \cdot \frac{A}{B} = \pm K$. Let Y equal per cent error at 0% S end of scale and X equal per cent error at 100% S end of scale; then the amount of correction that should be made for x, the unknown amount of similarity, is $(1.00-.58) \times .04 - .58 \times .12 \cdot \frac{1}{B} = K$ (since S varies inversely as % C when C = change, and since C in this case = 42%). Suppose B=6, then K = .005. Adding this value to % C found with reference to x, yields 42.5%. Hence we say that a substitution of defendant's bottle for complainant's will be recognized as different 42.5 per cent of the times. And since the first law of recognition says that cognitive ability varies inversely as

degree of similarity, the court is justified in declaring, on the basis of the foregoing evidence, that defendant's merchandise, bottle or whatever it may be, is 57.5 per cent similar to the complainant's. If the law makers decide that in such cases as these 25% or 30% or 40% confusion is permissible, then the value found, being above the safe limit, constitutes an illegal imitation. From such judgment there can be no appeal. About the fairness and accuracy of such judgment there can be no doubt.

Needless to say such tests are possible only in the case of portable items. When the items are not portable, or cannot be manipulated with facility in such an experiment, their photographs should be used.

Note. Another kind of similarity that may be cause for litigation is not form but generic similarity. It is the kind of resemblance existing between two items whereby one of them is enabled to inveigle itself into acceptance by virtue of the favorable disposition created in the purchaser's mind by the other. The individual need not mistake one object for the other, he may be perfectly well able to note the difference between them, yet because they have this generic resemblance, he reasons from similarity of name to similarity of origin or quality. Thus there is generic resemblance between Hotel La Touraine and La Touraine Coffee. Knowing that the former is a high class hotel, I infer from the similarity of the names that La Touraine Coffee has some connection with the La Touraine Hotel. Perhaps it is used at that hotel, and if so it must be of an excellent brand.

There is generic resemblance between Royal Baking Powder Co. and Royal Bakery. By the principle of radiation, the sense of quality induced by the former spreads to the latter. Again, there is generic resemblance between Snappy Cheese and Nippy Cheese, though here of course, it being a question of the same item, it is sufficient to consider the form similarity of the names alone. Yet imitation exists even if the customer who is in the habit of buying Snappy Cheese knows perfectly well that Nippy Cheese is not Snappy Cheese when he is confronted with the proposition of buying the former for the first time. The imitation arises from the fact that he thinks that Nippy Cheese is about the same as Snappy Cheese, or he may think that it is a different brand of the same article manufactured by the same firm—because of the similarity of name.

Where generic resemblance alone exists between two things belonging to different categories such as those cited in the first two illustrations, the psychologist cannot be of service to the court in a practical way.

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INTRODUCTION

The problem of the interference of feelings is of the highest importance, both for theoretical psychology and for practical life. The general nature of the problem will be brought to mind by recalling the scene between Tubal and Shylock in "The Merchant of Venice," or the working out of the plot in "Monsieur Perichon." The success of parents in dealing with children by "changing the subject" is due to such skill as can be acquired by experience in this field, while much of polite social intercourse, tact, and even national diplomacy has the same basis. Much depends upon doing unpleasant things in such a delightful way that anger is held in check. Similarly, a large part of the theory of apperception, association, especially the problem of hysterical amnesia, and probably also that of religious conversion (of the sort described in Begbie's "Twice-Born Men") can be much simplified by a study of the mutual influence of feelings.

As yet, however, technical psychology has concerned itself almost entirely with the attempt to discover the nature of feeling, using the methods of impression and expression, backed up by numerous physiological hypotheses. Concerning the behavior of feelings little is to be found beyond brief dogmatic statements, not based upon any general observation, e.g., "Pleasure and Pain are opposites in the strongest form of contrariety; like heat and cold, they destroy or neutralize each other."¹ Exceptions are to be found in those psychologists who have interested themselves in morals and aesthetics.² Much that is suggestive is to be found in works on aesthetics and literary criticism. It is, however, too indefinite to do more than serve as a good introduction to the problem. The work to be reported below was an attempt to extend technical laboratory methods into this almost untouched field. The only extensive research on the subject

¹ Bain *"Emotions and the Will,"* pp. 12-13.

² Cf. Bain. *op. cit.*, Ch. VI, under Will, "The Conflict of Motives"; also pp. 436-439.

previous to the present was that reported by Johnston, "The Combination of Feelings," *Harvard Psychological Studies*, Vol. II, pp. 159-191. His methods were considerably different from ours, but the general problems are much the same. The results should therefore be compared.

In order to indicate the nature of the sources, and also the wide range of our problem, a few of the more striking passages will be cited here.

In the *Philebus* of Plato is to be found the following discussion:

"SOCRATES. . . . When a person is in actual suffering and yet remembers past pleasures which, if they would only return, would relieve him; but as yet he has them not. May we not say of him, that he is in an intermediate state?"

PROTARCHUS. Certainly.

SOC. Would you say that he was wholly pained or wholly pleased?

PRO. Nay, I should say that he has two pains; in his body there is the actual experience of pain, and in his soul longing and expectation.

SOC. What do you mean, Protarchus, by the two pains? May not a man who is empty have at one time a sure hope of being filled, and yet in that he is empty is he not at the same time in pain?

PRO. Certainly.

SOC. Then man and the other animals have at the same time both pleasure and pain?

PRO. I suppose so." (St. 35.)

The analysis of the types of mixed feeling is given in St. 46-50.

Mixed pleasures are morbid, such as those of scratching.

There are some mixtures which are of the body, and only in the body, and others which are of the soul, and only in the soul; while there are other mixtures of pleasures with pains, common both to soul and body, which in their composite state are sometimes pleasures and sometimes pains.

(1) Both are due to sensations, in various parts of the body.
 (2) One factor is sensational, the other ideational. (3) "... anger, fear, desire, sorrow, love, emulation, envy and the like—pains which belong to the soul only." "... shall we not find them also full of the most wonderful pleasures? Need I remind you of the anger

‘Which stirs even a wise man to violence,
 And is sweeter than honey and the honeycomb?’

And you remember how pleasures mingle with pains in lamentation and bereavement? . . . And you remember also how at the sight of tragedies the spectators smile through their tears? . . . And are you aware that even at a comedy the soul experiences a mixed feeling of pain and pleasure?"

Burke ^{2a} may be represented by the following:

"The Effects of Sympathy in the Distress of Others."—To examine this point concerning the effect of tragedy in a proper manner, we must previously consider how we are affected by the feelings of our fellow-creatures in circumstances of real distress. I am convinced we have a degree of delight, and that no small one, in the real misfortunes and pains of others; for let the affection be what it will in appearance, if it does not make us shun such objects, if on the contrary it induces us to approach them, if it makes us dwell upon them, in this case I conceive we must have a delight or pleasure of some species or other in contemplating objects of this kind. Do we not read the authentic histories of scenes of this nature with as much pleasure as romances or poems, where the incidents are fictitious? The prosperity of no empire, nor the grandeur of no king, can so agreeably affect in the reading, as the ruin of the state of Macedon, and the distress of its unhappy prince. Such a catastrophe touches us in history as much as the destruction of Troy does in fable. Our delight, in cases of this kind, is very greatly heightened, if the sufferer be some excellent person who sinks under an unworthy fortune. Scipio and Cato are both virtuous characters; but we are more deeply

^{2a} "On the Sublime and the Beautiful" Part I. Section XXV

affected by the violent death of the one, and the ruin of the great cause he adhered to, than with the deserved triumphs and uninterrupted prosperity of the other; for terror is a passion which always produces delight when it does not press too close; and pity is a passion accompanied with pleasure, because it arises from love and social affection. Whenever we are formed by nature to any active purpose, the passion which animates us to it, is attended with delight, or a pleasure of some kind, let the subject-matter be what it will; and as our Creator has designed we should be united by the bond of sympathy, He has strengthened that bond by a proportionable delight; and there most where our sympathy is most wanted, in the distresses of others. If this passion was simply painful, we would shun with the greatest care all persons and places that could excite such a passion; as some, who are so far gone in indolence as not to endure any strong impression, actually do. But the case is widely different with the greater part of mankind; there is no spectacle we so eagerly pursue, as that of some uncommon and grievous calamity; so that whether the misfortune is before our eyes, or whether they are turned back to it in history, it always touches with delight. This is not an unmixed delight, but blended with no small uneasiness. The delight we have in such things, hinders us from shunning scenes of misery; and the pain we feel, prompts us to relieve ourselves in relieving those who suffer; and all this antecedent to any reasoning, by an instinct that works us to its own purposes without our concurrence."⁸

Hume worked out quite a complete theory. We have already cited one paragraph. The whole passage may be given here.⁴

"Suppose, then, that the object, concerning whose reality we are doubtful, is an object either of desire or aversion, 'tis evident, that, according as the mind turns itself either to the one side or the other, it must feel a momentary impression of joy or sorrow. An object, whose existence we desire, gives satisfac-

⁸ Cf. also Sections III, The Difference between the Removal of Pain and Positive Pleasure, and IV, Of Delight and Pleasure as Opposed to each other

⁴ Treatise Book II Part III Section IX

tion, when we reflect on those causes which produce it; and for the same reason excites grief or uneasiness from the opposite consideration: So that as the understanding, in all probable questions, is divided betwixt the contrary points of view, the affections must in the same manner be divided betwixt opposite emotions.

"Now if we consider the human mind, we shall find, that with regard to the passions, 'tis not of the nature of a wind-instrument of music, which in running over all the notes immediately loses the sound after the breath ceases; but rather resembles a string-instrument, where after each stroke the vibrations still retain some sound, which gradually and insensibly decays. The imagination is extreme quick and agile; but the passions are slow and restive: For which reason, when any object is presented, that affords a variety of views to the one, and emotions to the other; tho' the fancy may change its views with great celerity; each stroke will not produce a clear and distinct note of passion, but the one passion will always be mixt and confounded with the other. According as the probability inclines to good or evil, the passion of joy or sorrow predominates in the composition: Because the nature of probability is to cast a superior number of views or changes on one side; or, which is the same thing, a superior number of returns of one passion; or since the dispers'd passions are collected into one, a superior degree of that passion. That is, in other words, the grief and joy being intermingled with each other by means of the contrary views of the imagination, produce by their union the passions of hope and fear.

"Upon this head there may be started a very curious question concerning that contrariety of passions, which is our present subject. 'Tis observable, that where the objects of contrary passions are presented at once, beside the encrease of the predominant passion (which has been already explain'd, and commonly arises at their first shock or rencounter) it sometimes happens, that both the passions exist successively, and by short intervals; sometimes, that they destroy each other, and neither of them takes place; and sometimes that both of them remain

united in the mind. It may, therefore, be ask'd, by what theory we can explain these variations, and to what general principle we can reduce them.

"When the contrary passions arise from objects entirely different, they take place alternately, the want of relation in the ideas separating the impressions from each other, and preventing their opposition. Thus when a man is afflicted for the loss of a law-suit, and joyful for the birth of a son, the mind running from the agreeable to the calamitous object, with whatever celerity it may perform this motion, can scarcely temper the one affection with the other, and remain betwixt them in a state of indifference.

"It more easily attains that calm situation, when the same event is of a mixt nature, and contains something adverse and something prosperous in its different circumstances. For in that case, both the passions, mingling with each other by means of the relation, become mutually destructive, and leave the mind in perfect tranquility.

"But suppose, in the third place, that the object is not a compound of good or evil, but is consider'd as probable or improbable in any degree; in that case I assert, that the contrary passions will both of them be present at once in the soul, and instead of destroying and tempering each other, will subsist together, and produce a third impression or affection by their union. Contrary passions are not capable of destroying each other, except when their contrary movements exactly rencounter, and are opposite in their direction, as well as in the sensation they produce. This exact rencounter depends upon the relations of those ideas, from which they are deriv'd, and is more or less perfect, according to the degrees of the relation. In the case of probability the contrary chances are so far related, that they determine concerning the existence or non-existence of the same object. But this relation is far from being perfect; since some of the chances lie on the side of existence, and others on that of non-existence; which are objects altogether incompatible. 'Tis impossible by one steady view to survey the opposite chances, and the events dependent on them: but the

necessary, that the imagination should run alternately from the one to the other. Each view of the imagination produces its peculiar passion, which decays away by degrees, and is follow'd by a sensible vibration after the stroke. The incompatibility of the views keeps the passions from shocking in a direct line, if that expression may be allow'd; and yet their relation is sufficient to mingle their fainter emotions. 'Tis after this manner that hope and fear arise from the different mixture of these opposite passions of grief and joy, and from their imperfect union and conjunction.

"Upon the whole, contrary passions succeed each other alternately, when they arise from different objects: They mutually destroy each other, when they proceed from different parts of the same: And they subsist both of them, and mingle together, when they are deriv'd from the contrary and incompatible chances or possibilities, on which any one object depends. The influence of the relations of ideas is plainly seen in this whole affair. If the objects of the contrary passions be totally different, the passions are like two opposite liquors in different bottles, which have no influence on each other. If the objects be intimately connected, the passions are like an *alkali* and an *acid*, which, being mingled, destroy each other. If the relation be more imperfect, and consists in the contradictory views of the same object, the passions are like oil and vinegar, which, however mingled, never perfectly unite and incorporate."

Höfdding considers the problem at some length, as follows:⁶

" . . . feelings . . . arise and bestir themselves more slowly than ideas. It takes longer to convert joy into sorrow than to pass from the idea of something joyful to the idea of something sorrowful. Even in persons of sanguine temperament, thoughts and fancies alter with greater rapidity than the mood. If now consciousness passes from the one of (two) given points of view (ab) to another (ac), the idea *c* will have the tendency to excite a new mood (*v*); but since the mood (*B*) excited by the first idea (*b*) still endures, the two moods will coincide and form a combination. It is like waves striking upon the

⁶ "Outlines of Psychology," pp. 237-238.

shore; the advancing wave absorbs the receding one. In this way a mixed mood arises. . . .

"The moods of hopes and fear appear in innumerable gradations and shades, according to the relation of the possibilities to one another. . . . If the chances are thought equal, and the imagination is therefore attracted with equal force by either stream, the mind feels itself divided. Two different moods strive to expand in consciousness, but neither can gain the mastery. Hence arises the mood of doubt, the chief characteristic of which is a painful restlessness, which may excite so strong a desire to come to a decision that the nature of the decision seems indifferent if only the pain of uncertainty be ended. . . .

"When two conflicting feelings press at once to the fore and try to make themselves felt simultaneously with equal strength, there arises the intolerable sense of division just mentioned. This is, however, a rare case and of short duration. When Shakespeare makes King Claudius describe his mood at his wedding with his brother's widow as an equipoise of joy and sorrow, it is certainly the intention of the great poet-psychologist to exhibit him as a hypocrite, who betrays himself by the unnaturalness of the condition which he attributes to himself. Where the one feeling does not suppress the other or reduce it to a subordinate element, they will succeed one another rhythmically. Plato describes as follows the emotion of the disciples of Socrates during their last interview with their master. He makes Phaedo say: "I found myself in a truly extraordinary state, in an unaccustomed mixture of delight (in the matter of his conversation) and of sorrow when I reflected that he soon must die. And all present were in almost the same frame of mind, now laughing and now crying." Such an alternation is the natural state, when different motives take effect. But this cannot long continue, for the mind seeks equilibrium, and by means of memory converts the successive into the simultaneous; the two feelings are consequently blended into a new feeling sorrow and joy, e.g. into melancholy. .

"Sibbern has with justice therefore drawn a distinction between a mixture, or an alternation, of different or even conflicting states of feeling, and mixed feelings in the proper sense. In a mixed feeling the difference of the constituents is no longer observed, since they go to make up one single total feeling.

"Such mixed feelings comprise elements which, if appearing separately, would bear a character different from the total feeling which they help to form. . . . Here, then, are instances of psychical chemistry in the province of feeling."

Ebbinghaus relates this problem to that of attention.*

" . . . die Gefühle (sind) etwas gegensätzlich Gegliedertes: ihre verschiedenen Stärkegrade bilden zwei Zweige einer eindimensionalen Mannigfaltigkeit, die durch eine Indifferenzzone miteinander in Verbindung stehen und deren Glieder die Kraft haben, sich bei gleichzeitiger Anwesenheit im Bewusstsein wechselseitig aufzuheben. Sie stehen hiermit übrigens nicht allein; bei den Temperaturempfindungen und manchen Organempfindungen (Hunger-Sättigung,-Ermüdung-Frische) finden wir Ähnliches. Ausserdem ist zu beachten, dass es zu einer vollständigen wechselseitigen Aufhebung gegensätzlicher Gefühle durchaus nicht immer zu kommen braucht, ja vielleicht in der Regel nicht kommt. Wie wir gleichzeitig an den Füßen kalt und an den Händen warm haben können, so können wir auch die Lust einer wohlschmeckenden Speise und die Unlust heftigen Kopfschmerzes neben einander erleben. Selbst ein und derselbe Gegenstand oder Vorgang vermag uns gleichzeitig lustvoll und unlustvoll anzusprechen, soweit wir nämlich in unserem Vorstellen verschiedene Seiten oder Beziehungen des Gegenstandes gleichzeitig zu umfassen und doch auseinander zu halten vermögen. Die Gefühlsbegleitung reichhaltiger seelischer Gebilde kann dadurch eine sehr verwickelte werden."

Finally, we may give the following extracts from Sully's Essay on "Laughter."

*Ebbinghaus, *Grundzüge*, I, 540-541.

"A curious fact, not as yet fully studied by the psychologist, is what may be called the inter-diffusion of characters between the several parts of a complex presentation. The figure of a finely dressed lady in a gathering of poor people may either throw the shabby look of the latter into greater relief by contrast, or redeem it from its shabbiness by lending it some of its own glory. The latter effect is favoured by a certain contemplative attitude which disposes us to look at the whole as such, and with the least amount of inspection of details and their relations. When we regard the child in the big hat a semblance of the dignity which lies in the meaning of the latter is transferred to the small head; and the mental seizure of this transferred look of dignity by the spectator is essential to a full enjoyment of the show as a bit of make-believe, of innocent hypocrisy. Similarly, if we are disposed to laugh, a little contemptuously, at the man in the child's hat, it is because the hat throws for half a moment over the heavy and lined face something of the fresh sweet look of infancy" (page 17).

"... among the causes of laughter, a moment's relaxation of strain—muscular, intellectual or emotional tension—is one of the most common, if it be not universal. The delicious sense of relief which the collapse of the strained attitude brings us may no doubt be due to a consciousness of the transition, the escape from pressure of the moment before. At the same time, it is not improbable that the physiological processes of laughter themselves, by securing organic relief and refreshment, contribute a large element to the whole mental state.

"A like remark applies to the element of disagreeable feeling which frequently, at least, makes our laughter a mixed experience:—

Our sincerest laughter,
With some pain is fraught.

Shelley was hardly the person, one suspects, to judge of the quality of men's laughter; yet his couplet contains an element of truth. This mixture of elements is, no doubt, largely due to the initiating perception itself; for . . . the laughable spec-

tacle commonly shows us in the background something regrettable. But it seems reasonable to say that the element of sadness in our hilarity has its organic support in the unpleasant feeling-tones which accompany the effects of all violent and prolonged laughter (page 46-7).

"There is no anomaly here when once we get at the comic point of view. In Molière's plays, the source of laughter lies in this very intrusion of the ill-shapen into a community of well-rounded forms. It is the intruder on whom we fix the eye, for whose unpredictable antics in a world for which he is not made our expectation is set. The serious background is there, but does not take a strong hold of our minds; we are not greatly moved, for example, by the spectacle of the sufferings of the daughters and the wards of testy old gentlemen, or even of the wearing housewifely anxieties of Madame Jourdain. The proper world, into which the absurdly ill-fitted is here pitchforked, is but a background, rendering the valuable service of backgrounds by throwing into relief and so sharply defining the form for which the spectator's eye is accommodated" (page 369).

"In saying that we go to meet comedy in the play-mood, in which our habits of moral approbation and disapprobation, and even of estimation of social values, are lulled to a sleep more or less profound, it is not meant that these serious tendencies in us can be ignored by the writer of comedy. As implied above, they mould our forms of the seemly, unknowingly to us perhaps, even as we look. And more, though inhibited by the play-like mood, they have force; and should the showman go too far, say in the direction of stripping off the veil of decency, they may wake up and make an end of the comic enjoyment. Just as tragic fear and pity may give way to physical revulsion when horror obtrudes itself, so when in comedy the unclean thrusts into view its ugly head, a sort of physical revulsion may silence laughter. The latitude in these matters conceded from time to time to comic art will, it is evident, vary greatly with the particular ratio between the vigours of the mirthful and moral tendencies" (page 377).

"In modern literature, the interesting point to note is the growing interpenetration of the laughing and the serious attitude, and the coalescence of the mirthful spirit with sentiment. The two processes, though distinct, may run on together, as we may see in Shakespeare's plays. The humorous element introduced by the fool in 'Lear' and elsewhere, at once relieves the tragic tension, and gives a moment's play to that disposition towards a lighter laughing criticism which is always active when we survey colossal folly, even though the mental eye is at the moment focussed for its catastrophic effects. The laughter is controlled and kept tenderly humorous and half-sad by a large reflection, which does not lose sight, even at the relieving moment, of the lamentable ruin. It is only another way of combining the "fun" and the "pity" of it when the master brings a genial humour into comedy and makes us, with his faithful follower, Bordolph, half-love and more than half-pity the faulty knight who so merrily entertains us" (page 387).

Further important references are the following:—

- Elsenhans. *Lehrbuch der Psychologie*.
 Kap. III. 34. Die Verbindungen gleichzeitiger Gefühle.
 Kap. III. 39. Die ästhetischen Gefühle.
 Die ästhetischen Mischgefühle.
 (With bibliography)
- Geiger. *Bemerkungen zur Psychologie der Gefühlselemente und Gefühlsverbindungen*. *Archiv f. d. gesamte Psych.* IV (1905) 232 ff.
- Lazarus. *Das Leben der Seele*. (Dritte Auf. 1883) 310 ff.
- Lipps. *Leitfaden der Psychologie*. 297 ff. *Mischgefühle*. *Konstellationsgefühle*.
- Moulton. *Shakespeare as a Dramatic Artist*. 3d ed. The whole book should be read. There is so much that bears upon our topic that it is hard to single out any one part, but the following passages are at least representative: pp. 34; 47-56; 59-61; 69-73; 325-329; 332-335; 340-355.
- Rashdall. *Theory of Good and Evil*. Vol. II, 1, 2 (not a technical discussion, but suggestive.)
- Outlines of Psychology*. pp. 1, 2

- Santayana. *The Sense of Beauty*. Part IV, especially pp. 216-7, 224-5; sections 57, 58; pp. 247-250; 255; 259.
- Titchener. *Feeling and Attention*; especially pp. 46-55.
- Wirth. *Vorstellungs- und Gefühlscontrast*. *Zeit. f. Psych*, 18, p. 49 ff.
- Wundt. *Grundzüge der physiologischen Psychologie*. Zweiter Band. 337-341. *Das Contrastprincip der Gefühle* 341-353. *Verbindungen der einfachen Gefühle*.

REPORT OF EXPERIMENTAL WORK

The experiments now to be reported were carried on in the Harvard Laboratory, during both semesters of 1912-13 and the first semester of 1913-14. In 1912-13 the subjects were five men and three women, one of the men, "M", dropping out at midyears; in 1913-14, six men and one woman. One of the men, "K", worked throughout, the other subjects of the second year being new. All had had some experience in laboratory work.

Our task was to produce feelings as normal as possible under laboratory conditions, and in such a manner as to lead to interference. For materials, picture postcards and pictures cut from magazines, with a few from surgical books, were used. The better reproductions of classical paintings were avoided because of the difficulty of obtaining sufficiently similar, but unpleasant, materials.

Interference was secured by exposing two pictures, generally one pleasant, the other unpleasant, alternately, in a slightly modified form of the Dodge tachistoscope. (R. Dodge, An improved exposure apparatus. *Psych. Bull.* 4: Jan. 1907, pp. 10-13.) The accompanying diagrams indicate roughly the arrangement of the apparatus. The general method rests upon the known tendency of feelings to persist (*cf.* the extract from Höffding in Introduction), and upon the qualitative opposition of feelings. These two opposed tendencies give something definite to be worked out, while variation of the rate of alternation serves as an objective check upon introspection, and may lead to some insight into the rhythm of feelings.

In order to avoid distraction and external sources of feeling as far as possible, the work was carried on in one of the inner rooms of the laboratory, the walls of which are painted a dull black. General illumination was secured by means of a single lamp light, in the middle of the ceiling. As the observer faced

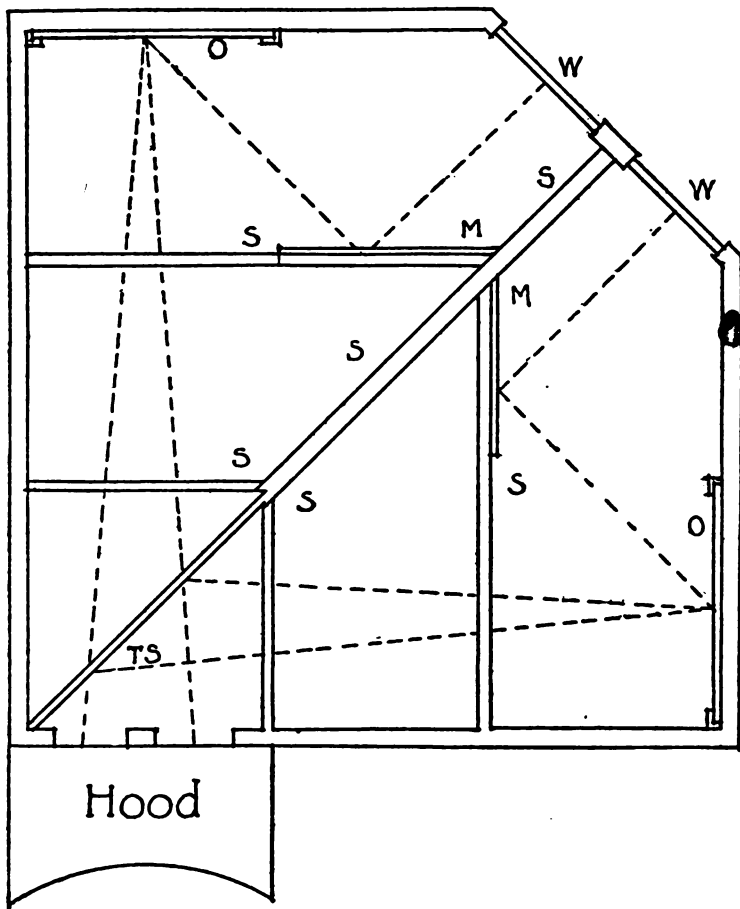
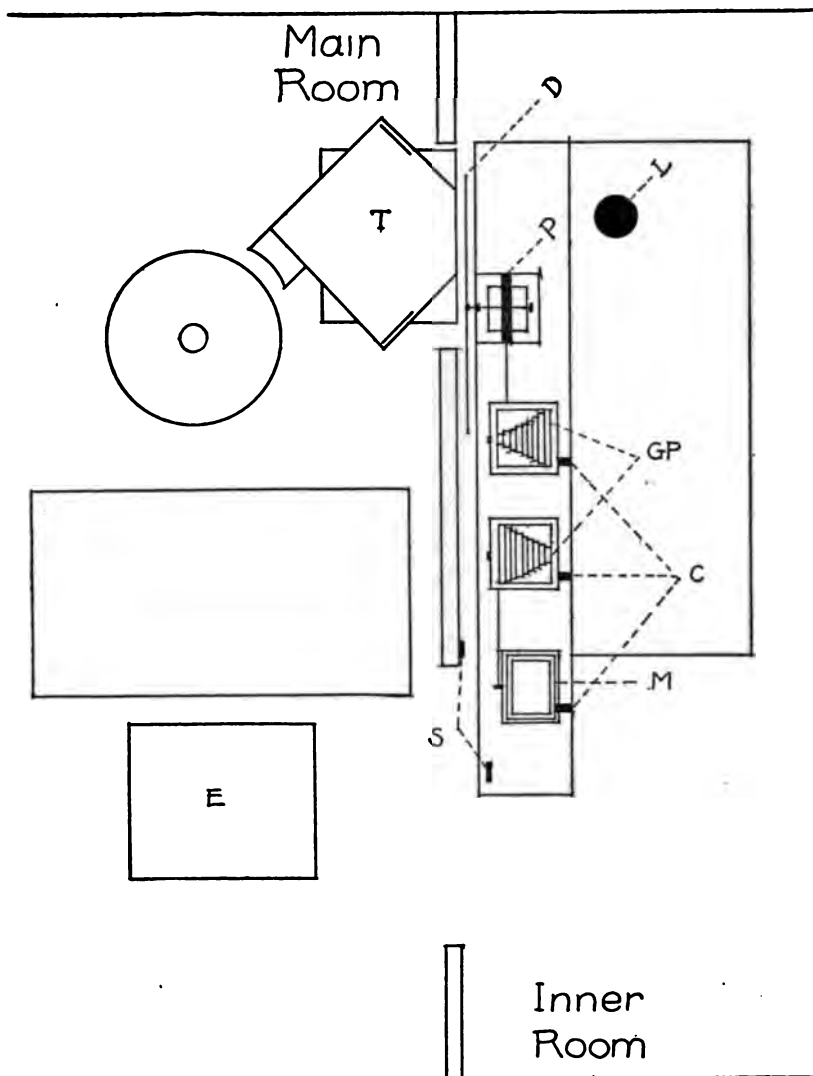


DIAGRAM OF TACHISTOSCOPE, COVER REMOVED

Following Dodge, except that the cards to be exposed are inserted through slots in the cover, instead of in the sides of the box. W W, ground glass windows; M M, mirrors; O O, objects to be exposed; S S S, screens to confine the illumination to its proper object; TS, transparent mirror. The dotted lines indicate the path of two parallel beams of light.



GENERAL ARRANGEMENT OF APPARATUS

T, tachistoscope, placed upon a shelf, with its double window fitting an aperture in the partition; O, observer; E, experimenter. The control apparatus was fastened by means of clamps C to a plank supported by uprights above the table in the inner room. D, revolving disk, with apertures to admit light; L, electric lamp with reflector; P, large pulley. G P, graduated pulleys for regulating speed; M, alternating-current motor; S, switches for L and M. In each trial the motor was started, then the light was turned on for the required time. At the close the switches were operated in reverse order.

the rear of the room, this light did not interfere with the experiments, while the depressing effect of darkness was avoided.

Each subject worked one hour a week, always in the morning. No attempt was made to keep the work rigidly systematic. The number of trials, and the materials used, depended upon the tendencies and success in observation of the various subjects. In the early stages of the work the material was to some extent selected by the subjects, the latter being asked to grade large numbers of pictures roughly, as more or less pleasant or unpleasant. From the material thus grouped, the experimenter chose that to be used in the regular work. When the experimenter became somewhat familiar with the tendencies of the subjects, this method was dropped.

The work was divided into the following sections:

A (1912-13)—rate of alternation in individual trial constant.

1. Preliminary trials.
2. Tracing of the course of the feelings.
3. Description of the feelings.
4. Combination of 2 and 3.

B (1913-14)—rate of alternation varied during the trials.

1. Attention normal.
2. Attention controlled.
3. Experiments on distraction.

(The rates of alternation used ran from ten exposures per minute to about one hundred fifteen).

A summary of the methods and results in each of these, with samples from the introspections, will now be given. (In connection with the quotation of introspection the condition of the subject, unless otherwise noted, is normal.)

A. 1. The preliminary trials were intended to give the observers training in the special type of introspection necessary, and to try out the possibilities of the method. With the latter purpose in mind, no attempt was made to keep these trials systematic. Promising trials were followed out with each subject, until it seemed clear what should be done. A few representative introspections may be given.

In these preliminary trials, the directions were only general. The subjects were requested to give themselves up to the ex-

perience in a perfectly normal way, and report upon the feelings and anything else that seemed of special interest in the experience, associations when these were very noticeable, etc. The importance of this will appear later on.

We will give first a remarkably clear case of feeling-transference.

Subject "F"

Materials, Kitten (sitting on a pedestal) and No. 7, (an unpleasant surgical picture.)

Oct. 3. Trial 1. Speed 10.⁷

"The human figure was sickening, repulsive. The cat picture came as a relief; it looked snug, cozy, contented. The repulsive human figure, however, was stronger and began to lessen the relief derived from the cat. I notice five minutes after observing the two, that the kitten comes unforbidden as an image; the human figure I get by a decided effort."⁸

Trial 2. Speed 19.

"The second observation of the two pictures under more rapid change was more tolerable, the human figure made less impression than in the former exposure, the moments of relief were more prominent than before. Anticipating the human figure created an incipient shrinking attitude, while that preceding the cat created a welcome attitude."

Trial 3. Speed 36.

"The speed during third observation made the size of the human figure more effective; it dominated the field, began by eliminating the effects of the small picture of the cat. Then, too, the cat is not as attractive as in Obs. 1. I have discovered a blemish in her left foreleg which lessens the attractiveness of the picture." (In the picture, there is a very slight roughness in the fur.)

Trial 4. Speed 20.

"The human figure is absorbing that of the cat. It is as if the cat sat on the human figure as a sort of background. I found myself wondering how near the wound are the feet, where would its head come, etc."

⁷The figures for speed here and elsewhere represent pairs of exposures per minute. The length of each trial in this series was about half a minute.
⁸Of Cat. & She. Study in *Nalika* *Journal of Psychology* (1902) or 1-4.

Oct. 17. Trial 2. *a.* (a garden with a path) and Kitten. Speed 10.

"(*a*) pleasant; kitten has lost her pleasantness. Swelled place on leg catches attention. Unpleasant associations still last."

Trial 3. Speed 10. Kitten and Ancient (a caricature of an old maid).

"Kitten not as disagreeable, largely due to background. Other largely due to brightness. Both disagreeable, but kitten less so. Kitten has lost some of the offensiveness, the untidiness."

Nov. 7. Trial 4. Kitten and Two Fishermen. (humorous.) Speed 10. 40 seconds.

"Feeling aroused was a continuous one, humorous. Smiled throughout, the smile persisted during "cat" exposure. The cat, however, lessened, but did not suppress the humorous feeling. The latter may be represented by waves or billows, with the cat in the trough, and being borne up by the pleasant tone thus!"

"F" had forgotten his previous experiences with this kitten. He informed me that he liked cats, and could not account for his dislike in this case. I believe that the whole apperceptive attitude and feeling persisted from the surgical picture, and fixed upon the very slight defect in the picture of the kitten.

The following selections bring out the influence of the various speeds:

Subject "I". Oct. 24

Trial 1. Brook in Middlesex Falls and It (a dude). Speed 10. 20 seconds.

"Pleasant picture produced more of an impression; noticed tendency merely to stare at the other. Very strong feeling tone for the pleasant picture, mildly unpleasant tone for the other. Running water very strong impression, kinaesthetic imagery held over. "It" forgotten as soon as he disappeared. Pleasant feeling lasted a little while. Other picture delayed in coming in; lines just stood there blankly for an instant."

Trial 2. Material same. Speed 19. 20 seconds.

"Very much the same. . . . Great persistence of the picture of the brook, active associations . . . enter into scene. Other regarded more impartially; man does not seem so real, does not take hold so much."

Trial 3. Same. Speed 36. 20 seconds.

"Harsher aspects of man come in more strongly. Pleasant did not have time to develop fully; apparently pleasantness takes longer to come as well as longer to go. Feelings more evenly balanced."

The following trial with the same subject gives a case of fusion.

Trial 4 of same date. "It" and No. 24 (a house) (24 chosen as mildly pleasant by "I". Speed 10. 20 seconds.

"Foolish picture had the advantage. No persistence of imagery, but feeling tone seemed to fuse with the other. "24" is given a slightly disagreeable tone."

Increase of speed in the next trial again favored the unpleasant.

Subject "M" frequently reported parallel feelings, as in the following cases:

Oct. 18. Trial 1. Fabriola and No. 41 "M" sleepy. Speed 19. 45 seconds.

"Two streams of feeling, one serious and the other comic. A sort of continuity. *The streams run along parallel, varying in strength according to the pictures in view. Both pleasant, but in a different way.*"

Trial 2. Same. Speed 10. 40 seconds.

"A little change . . . familiarity. Feeling decreased, but equally. The streams got so thin sometimes that they broke. Otherwise similar to previous."

Trial 3. Same. Speed 29.

"Village schoolmaster more comic, thought of that picture more. The stream of comic feeling was unbroken, not sure about the other."

Oct. 25. Trial 1. "Mona Lisa" and "God Help the Poor Sailors" etc. (a comic drunk) "M" sleepy. Speed 10. 60 seconds.

"One funny, the other pleasant. . . . No noticeable change in course of trial. *Two streams of feeling, emphasised in alternation.*"

Oct. 25. Trial 6. La Vergine Afflitta and Two Fishermen. Speed 10. 40 seconds.

"One agreeable other funny no doubt. Feeling tones lasted throughout."

With pleasant and unpleasant there seems to be more interference.

Oct. 25. Trial 3. Whittier Falls and No. 92. Speed 10. 20 seconds.

"One pleasant all the time, the other disgusting. *Contrast made pleasantness stronger, and the pleasantness seemed to last and be contrasted with the disgust during exposure of "92."* This was not true in the other direction. The pleasant feeling was pure.

Subject "I" was especially sensitive to the effect at transition.

Oct. 3. Nos. 7 (surgical) and 17 (landscape). Speed 24.

"Landscape first . . . seemed all the way through as if it were exposed longer than the man. During the first half of trial, agreeable outweighed disagreeable feeling. Then the disagreeable began to come in stronger. Began to think more about the man. But that lasted only a short time. Then came a return to first.

"Seemed to anticipate the pleasant picture; trees loomed up through the other picture before transition. Landscape more complex. Disagreeableness of other gave way at times to monotony. Transition contrast worked in favor of pleasant. Did not accentuate the other."

Oct. 10. Boy in Red (Le Brun) and No. 12. Speed 10. 20 seconds.

"Two make practically the same amount of impression. At moment of transition always a little discomfort. With the agreeable picture this was immediately followed by a contrast effect, more agreeable than otherwise. With the disagreeable, the feeling accumulates."

Trial 4. No. 49 and Madame Recamier. Speed 10. 20 seconds.

"Madame made stronger impression. Contrast very marked, more so in transition to the unpleasant. Other way, disagreeable feeling seemed to carry over, so in this direction there was subtraction. Seemed to attach less real meaning to the bad one than to the other."

Subject "K" seems occasionally to have objectified the transition effect in a curious way.

Oct. 9. Trial 7. No. 92 and Mona Lisa. Speed 72.

"Change unpleasant for eyes. Spite, competition going on. Indifferent about competition, yet wanted to see pleasant picture, couldn't get enough of it at that rate."

Trial 9. Same. Speed 48.

"Feelings more marked, more time to notice every feature. Two pictures seem to be fighting. Pleasant one is disdainful, admiration for her increased."

Trial 10. Same. Speed 20.

"The fight is over now, and the pictures are separately related to me. Mona looks more serene now,—feeling of pleasantness greater. Other unchanged."

Such results are quite frequent with this subject, and occur occasionally in the work of some of the others.

The above selections are, I think, sufficient to show the nature of the problem, which was the purpose of this section of the work. Accordingly we may leave the preliminary experiments without attempting any more complete report.

A. 2. Tracing Course of Feelings.

The method of this section was suggested partly by the nature of some of the introspections given in the preliminary trials, partly by the familiar criticism, concerning so-called 'mixed feelings,' that these states may be really cases of rapid alternation (*cf. e.g.* Titchener, Textbook, p. 236). It was thought that this question could best be settled by concentration upon the temporal aspect of the experience. In this connection a reply to a common form of the above criticism may be offered.

We often hear of an alternation too rapid to notice as an explanation of mixed feelings, divided attention, etc. Now, while this explanation may possibly serve physiologically,—I do not care to discuss that question here—psychologically it hardly be valid. An alternation too rapid to be detected is a conscious alternation, and it is consciousness with which are concerned at present. Hence, I feel no hesitation in accepting the results of careful introspection, given immediately after a brief exposure of the stimuli. With long trials, I should have some hesitation owing to the liability of confusion in memory. This, however, brings up an interesting problem. Suppose mixed parallel feelings are reported. The suggestion of a confusion in memory, some sort of telescoping of the experience may be valid as against the truth of the statements that

gard to the experience during the trial. But what of the experience at the time when the statements are made? The statements must have some basis. Then do we not face a choice between belief in affective imagery and belief in mixed feelings? I assume that every judgment has some basis in present experience even though its reference is to the past.⁹

In order to understand more easily the reports of my subjects, I occasionally tried an experiment with myself as subject. A few of these were recorded, and I will quote them under my own name, to keep them distinct from the work of the subjects. This is in deference to the opinion of those who hold that the experimenter should not make use of his own introspection.¹⁰

As an introduction to the general method of the present section, I will quote one of these forbidden introspections.

Kellogg

Nov. 1, 1912. Fairly good condition, a bit sleepy. Speed 10. (Length of trial could not be recorded, about a minute, at a guess). No. 3 (surgical) and What are you Laughing At (comic).

"The exposures of 3 seemed to grow longer, of other shorter, as trial went on.

"3 came first, bare apperception, not a trace of feeling. Other slightly humorous. . . .

"Second exposure of 3 very disgusting. No empathy or sympathy noticed in whole trial, just disgust. In one or two scattered exposures the feeling seemed to lose hold somewhat, but in general it increased during the trial.

"At first the feelings were broken off sharply with the shifting. Then the disgust began to get the upper hand, began to hold over, but there was at first no blending. The disgust simply held the humor back. I noticed that I tried to get the humor and get rid of the disgust. This failed; the delay continued. Then the effect changed again. The disgust lasted, and the humor, though delayed a little, began to come in before the

⁹ Cf. Russell, "The Problems of Philosophy," p. 91

¹⁰ Cf. the discussion by Alechsieff, in Section III of his article on "Die Grundformen der Gefühle" *Psych. Studien* 3 (1907), pp. 156-271.

disgust was gone. The disgust began to hold over longer and longer each time, until finally it was continuous. Its strength in the intervals also increased, until at the end the feeling was as strong when the picture 3 was absent as when it was present. At the same time the humor began to come in more nearly at the beginning of the exposures, overlapping more and more. Then there was anticipation of it during the close of the exposure of 3, and finally this feeling too seemed to be continuous. The simultaneity of the two incongruous feelings seemed queer. Then a new feeling of displeasure at the situation came in, and I stopped looking.

"The shifting as such was never other than indifferent. Did not notice any eye-strain.

"The two feelings, when finally simultaneous, seemed to be partially, but only partially, fused. I could not say that there was a thoroughly unified resultant emotion.

"This partially fused emotion was taken in a somewhat impersonal way. My attitude was to a slight extent that of a disinterested spectator, until suddenly the feeling was strongly realized as my own, and the new displeasure mentioned above came at once. . . .

"As the feelings were growing towards fusion, I found myself saying, What is so funny, anyway?

"The visual imagery did not generally hold over. Once, about the middle, imagery of 3 did last into exposure of other picture."

In the work of this section no attempt at rigorous analysis of the feelings was made. They were for the most part merely named as well as might be. The subjects were asked to attend carefully to the time order of the feelings.

In order to get further light on the mutual influence of the feelings, check experiments were often made use of.¹¹ In these, the pictures of the main trial were alternated with a plain white card, or with a period of darkness.

The check trials for the above were made with the following results:—

¹¹ In all that follows, introspections should be read carefully, in groups—check trials with main trials, and trials with different speeds. Much that seems wholly trivial in the rough statements of the notes, is full of meaning when read in this way.

What Are You, etc., and darkness interval. Speed 10, about half a minute.

"Feeling mild humor, continuous. No breaks in intervals. Did not weaken in intervals either, except once, slightly, about middle of trial. General course practically level.

"Intellectual attitude in intervals shifted, sometimes reminiscent, sometimes expectant.

"The coming of the dark intervals was a trifle disagreeable sometimes. At these times, the humor was a little impeded just at the beginning of the dark period, but there was no break.

"No visual imagery in intervals."

No. 3, same way.

"First time, bare apperception. Then in the later exposures a mild dislike came in. Varied in strength. . . . There was a period of indifference in the middle of the trial. Picture seemed a little unreal throughout. Disgust was very mild. Anticipation came into some of the intervals, and there was some unpleasant feeling then. Usually the feeling dropped at once at close of exposure, but occasionally it died away slowly. Sometimes it was cut off sharply by a pleasant feeling which came from the richness of the black. No visual imagery."

(I believe now that the duration of these trials was considerably longer than the estimate quoted from the notes. I may add also that the experience of the main trial made a very vivid impression upon me. At various times for some weeks thereafter I found it possible to go through it in memory with no external aids. Even now, in March, 1914, when writing out the passage referring to the partially fused experience, I had a similar feeling, a complex of pleasure, displeasure, and a marked tension, with faint traces of visual imagery. This being so, I can hardly doubt the validity of the original report.)

Another example will serve to show the need for care, as the displeasure changed to sympathy before the parallelism came in.

Kellogg

Nov. 15, 1912. Excellent condition. Speed 24. Time seemed about a minute. Sunset at Woods Island and Dachshund.

"Dog first, seemed long exposure. Cold recognition. Sunset pleasant as soon as seen, mild feeling. Dog very slightly unpleasant next time. Then for a few exposures attention was

rhythmical. Sunset vivid and pleasantness stronger, but stopped with picture. Exposures of dog almost a blank, but a trifle disagreeable. Then attention suddenly shifted. Dog was the stronger for one or two exposures, but sunset not inhibited to so large a degree as dog had been. Then the process seemed to get mechanized; a pulse of attention (a slight tension noticed) for each picture, not quite at beginning of exposure. I seemed to favor this rhythm. The transition from one feeling to the other was very interesting. Feeling for dog gradually became more sympathetic and very slightly humorous. Then the feelings began to hold over; did not inhibit each other, but overlapped. At end of trial both were continuous, and from merely running parallel they began to fuse. Seemed to form one feeling, regardless of alternation.

"The shifting was not unpleasant as such. Difference in eye accommodation for two pictures seemed to influence attention at first. Then became mechanized and was no longer noticed."

The greater part of the work in this section was done with speeds 10, 24, and 36. Examples will now be given from the results with each subject. In several cases, the same material was used with various subjects, but in any case, consideration of the individual differences is of interest.

Subject "J"

Oct. 31, 1912. A little excited, mild unpleasantness.

Trial 1. Speed 10. 10 seconds. No. 3 and dark interval.

"Two exposures, light grows slowly, attention concerned wholly with light at first. Then picture broke upon consciousness,—a tremendous pulse. Grew to its height just as the light began to fade again. (Strong bodily reaction.) Feeling faded a good deal with the light, but an amount of unpleasantness remained, and this lasted right through the interval. Second exposure merely continued this feeling."

Trial 2. Speed 10. 10 seconds. Georgetown Road (a road at the university), and dark interval.

"Three distinct pulses of consciousness in the first exposure; 1, perception, clear and distinct by itself; 2, 'I've seen that before' in inner speech; 3, mild pleasantness. Feeling persisted through interval, also perseveration of "2."

"Second exposure, associations,—Berlin, etc., but feeling-tone not increased. Afterwards feeling of pleasantness increased markedly

Trial 3. Speed 10. 20 seconds. No. 3 and Georgetown Road.

"3 came first, came with a shock, but not so much feeling.

"(Road), no feeling till towards end, not very strong.

"3 next, indifferent.

"Road, pleasantness came in a little earlier and was stronger.

"3rd exposure of 3 indifferent.

"Road, race of 'peristaltic' feeling. A very vague, fleeting feeling of pleasantness, scarcely above indifferent.

"Two curves entirely discontinuous."

Nov. 14, 1912. Trial 1. Speed 24. 40 seconds. Audrey Burton (from Red Book) and Mosquito (magnified).

"The bug first, just a flash of exposure; then the other, purely intellectual. 2nd exposure of bug cold too. Then girl again, very pleasant. For a while alternation of pleasure and indifference, sharp cut. Then came a desire to see more of the portrait; other attracted attention less. At end imagery of outline of portrait was seen upon the other picture, and pleasantness held over."

Trial 2. Speed 24. 20 seconds. Mosquito and white card.

"Whole time in blooming wonder as to what the thing was. Blank card entirely too bright; tension, strain, all the time, yet a great deal of interest. Desire to find out what was there, mildly pleasant."

Trial 4. Speed 24. 20 seconds. Audrey Burton and white card.

"Pleasant; she's a dear. . . . *When attention is concentrated sharply, feeling-tone disappears.* Tried this with the after image. (Negative after image came on the other card.) *General organic sensations and tonus of body continue, but feeling-tone disappears.* Pleasure continuous, except when attention is strained." (N. B.)

Nov. 21, 1912. Speed 36. 40 seconds. (Heavy organic feeling all over, just slightly unpleasant.) Lovers' Retreat (East Northfield) and No. 38.

"First an effort of adjustment, focusing of attention. Time very short. Both indifferent till very near close. Then dog called up associates, pups in Berlin—feeling of amusement. Tried to get the woods clear, but no feeling for this picture at all. When attention is focused upon one picture the other is just a blur. A good deal of strain in the experience. Sensations localized in solar plexus, in eyes, and in forehead."

Trial 2. Speed 36. 40 seconds. Lovers' Retreat and dark interval.

"Logically pleasant, in a few cases a relief from the general feeling-tone. Connected with empathy. Not positively pleasant, merely inhibited general unpleasantness."

Trial 3. Speed 36. 40 seconds. No. 38 and dark interval.

"Colors clear, outlines difficult to grasp. No feeling except a slight relief from organic sensations. After about half the time turned his attention to transition from picture to interval. Organic sensations disappeared with picture each time and then swept back in interval."

Subject "I"

Oct. 31, 1912. (Fairly pleasant) Speed 10. 10 seconds. No. 3 and dark interval.

"Grew continually more disagreeable; at its height when light was cut off. Image lasts and feeling-tone lasts with it. Image died out first, then feeling faded; but when picture came again the feeling seemed to connect with a process still going on; no break. Feeling for second exposure stronger. Seemed to begin where the other left off.

"At the very beginning, a noticeable interval for apperception. First, just something on paper, then gradually seemed to represent an actual experience of a person."

Trial 2. Georgetown Road and dark interval. Speed 10. 10 seconds.

"Feeling-tone came out very quickly. Almost felt as if running to meet it. It developed continuously, increased to very end. Persistence of image in interval much more marked."

Trial 3. No. 3 and Georgetown Road. Speed 10. 20 seconds.

"Visual image of road as mere sense-impression lasted longer and came more immediately to clear focus at each new exposure.

"Feeling-tone of injured man lasted even during recognition of road. Whatever sympathetic processes were made active by the unpleasant picture were less easily inhibited than those from the pleasant picture which were more easily excited in the first place."

Nov. 14, 1912.

Trial 1. Hazel Troutman (from Red Book) and Ancient. Speed 24. 60 seconds.

"Preference very marked, improved at each exposure. Displeasure as much due to the cutting off of the good picture as to the bad picture itself. Contrast favored pleasant picture. Anticipatory image led to neglect unpleasant picture. Feeling

did not increase as trial went on. Pleasantness did not seem to be entirely cut off by unpleasant."

Trial 2. Ancient and dark interval. Speed 24. 30 seconds.

"Certainly disagreeable enough; gets worse and worse. More activity in intervals toward end than at beginning. Unpleasantness continuous. *During intervals attended more to feelings. This reinforced feeling.*" (N.B.)

Trial 3. Hazel Troutman and dark interval. Speed 24. 30 seconds.

"Development similar. Continuous increase of pleasantness. *Same reinforcement of feeling by attention to it in intervals.*"

Nov. 21, 1912. (Pretty fair.)

Trial 1. Sunset at Woods Island, and God Help the Poor Sailors, etc. Speed 36. 40 seconds.

"Agreeable picture predominated from outset. Persistence of pleasant feeling during exposure of ugly picture. Involuntarily disregarded what was most unpleasant in picture. Fixed on the neutral features. This tendency stronger with each new exposure, while sunset grew more and more agreeable. The ugly picture eventually grew meaningless, except as a rather annoying halting-place between exposures of the other."

Trial 2. Sunset and dark interval. Speed 35. 40 seconds.

"Pleasantness broken by the blackness of the interval, as if there were a dark storm, and the other scene were exposed by a flash of lightning. Interval unpleasant. Feeling almost like fear. Almost destroyed pleasantness towards end."

Trial 3. G.H. the P.S. etc., and dark interval. Speed 36. 40 seconds.

"Imagery lasted. Memory and anticipation. Feeling continuous and increased. Character of feeling changed:—annoyance, contempt, anger."

Trial 4. Sunset and white card (to correct Trial 2). Speed 36. 40 seconds.

"Quite a different effect. Pleasure increasing. More details kept coming in. 1, boat; 2, boat and water; 3, boat, water and sky, etc. Pleasure more extensive.

"Imagery lasted very slightly. Feeling passive, died out rather quickly. Advance in feeling due to adaptation. (Pleasure aroused by these pictures is usually passive, displeasure active.)"

Trial 5. The Lily Pool, and No. 38. Speed 36. 40 seconds.

"Very much like exposure of the other two. Interest more

active in case of pleasant picture. Awoke sympathy. Other picture seemed comparatively insignificant, and as before I disregarded unpleasant features as the pleasant feeling of first picture increased. Marked persistence of both imagery and feeling from lily pool even to extent of interfering with clear apprehension of ugly picture."

Subject "F"

Oct. 31, 1912. (Very well, but has been working with *asafoetida*, and the odor persists in his consciousness.)

Trial 1. Georgetown Road and dark interval. Speed 10. 10 seconds

" . . . A wave of pleasant feeling during first exposure. Feeling in interval comfort, pleasant anticipation. Interval seemed long, but not tedious. Second exposure still more pleasant. Feelings in interval and during exposures were of different sorts."

Trial 2. No. 3 and dark interval. Speed 10. 10 seconds.

"Cringing in short first exposure. Other two were long. In last one the bodily reaction died away. The second exposure was the most disagreeable. . . . Inner speech 'That's one of those horrid wounds'. In last one, curiosity concerning location, so height of feeling was in first long exposure. Feeling of relief at close of exposure in all three cases. The curiosity in third was like whistling to keep up courage. Cringing, etc., came very quickly. After feeling of relief, between exposures, there came a curiosity—'How did I feel?' but unpleasant feeling did not persist. Now after trial, the imagery lasts, but the feeling is gone."¹²

Trial 3. Georgetown Road and No. 3 Speed 10. 20 seconds.

"3 first—expectation. Road—surprise, great relief, feeling of coolness. On return of 3, feverishness. This seemed to be due to influence of road picture,—transference and reversal."¹³
 "Road-relief not so great, intention to enjoy picture as it passed. 3 . . . voluntary aspect of reaction,—attention to rim of wound. Same curiosity effect as before. . . ."

Trial 4. Repetition of previous.

"Feeling-tone less in both cases. Road first. 3 dominant throughout. R. on the defensive against the wound picture.

¹² Cf. Titchener, *Feeling and Attention*, pp. 61-65.

¹³ Cf. the quotation from Darwin, Titchener's Textbook, p. 487; also *Précis de l'anthropologie dans l'expression des émotions*, Rev. Sci. 12, 30, 402-30.

Effort required to enjoy; repulsion comes of itself. Started with a low feeling-tone. Not a strong feeling at the end. Feeling for R. gradually driven out."

Nov. 14, 1912. (Very well.)

Trial 1. Hazel Troutman and Ancient. Speed 24. 60 seconds.

"Feelings sharply opposed. Ancient remained on the same level, attitude did not change. Young lady increasingly attractive. Possibly the unpleasantness was assimilated to the other somewhat and tolerated. Tendency to idealize the good one, read a lot into it. Other just taken as it is."

Check trials were made, but one of them failed, as will be seen.

Trial 2. Ancient and dark interval. Speed 24. 30 seconds.

"Tendency to revive imagery of H.T. in proper places. Seemed to relieve the effect of this. Tried to follow directions by centering attention upon this picture, but could not. Practically a repetition of former experience."

Trial 3. Hazel Troutman, same method.

"On the whole, the feeling rose,—continuous. Other 'girl' butted in once or twice, but did not detract. Came in during exposures, not in intervals."

Trial 4. No. 8 (Niagara) and No. 96 (Bowlegs, comic). Speed 24. 40 seconds.

"Man more or less comical, feeling did not increase. Other—awe, dignity, power. *Pleasant, feeling increased. This feeling continuous; feeling for man broken after each exposure.*

Trial 5. No. 96 and dark interval. Speed 24. 30 seconds.

"Feeling not as humorous, unpleasant aspects come in. Pleasant at first, then unpleasant and stayed so, continuous."

Trial 6. No. 8, same way.

" . . . Intervals seemed to be same as my winking, just as if exposure were continuous. Cool feeling, etc. Bridges in distance noticed. More interest as trial went on. Pleasantness did not increase . . . continuous."

Nov. 21, 1912.

Trial 1. Lovers' Retreat, and God Help the Poor Sailors, etc.

Speed 36. 60 seconds.

"At first two distinct feelings, (a) pleasant, (b) disgust. The pleasurable tone increased on its own account and encroached on the other. This continued until *the pleasurable tone carried over from exposure to exposure, thereby becoming continuous,*

*the disgusting picture coming in as a check or damper on the pleasurable tone.*¹⁴ The pleasurable tone grew in two directions, backward and forward, the former as a pleasant lingering tone, and the latter as anticipatory and of course pleasant."

Trial 2. G.H. etc., and dark interval. Speed 36. 60 seconds.

"Disgust at first, then gave way to incipient sympathy, then gave way to humor. Noticed that everything was in curves. No breaks in feeling during intervals. Intervals hardly noticed."

The other check trial is unsatisfactory, i.e. as such. It has, however, an independent value.

Trial 3. Lovers' Retreat, and dark interval. Speed 36. 60 seconds.

"Intervals came in as an interruption. Blackness seemed to cut off feeling. Scene changed from a warm summer scene to a cold winter scene. Began to shiver. Contrast with the warmth unpleasant. Distinct organic reaction. The shift was rapid, no lingering."

Subject "H"

Nov. 1, 1912.

Trial 1. Georgetown Road and dark interval. Speed 10. 24 seconds.

"Was expecting a steady exposure; very much annoyed by the black interval. This seemed longer than the exposure.

"Feeling first pleasure in the subject of picture. Then seemed to feel myself in that situation. Then the scene began to shift to my own woods. This was cut off by the ending of the exposure.

"Feeling during intervals, annoyance. Wiped out image of picture. Mind seemed to be a blank."

Trial 2. (getting sleepy). No. 3 and dark interval. Speed 10. 24 seconds.

"First two exposures, amazement. Feeling-tone for picture self, sinking, dread. Seemed to get ether smell. In last two exposures feeling-tone entirely the sinking, dread; very unpleasant. Dark intervals very pleasant."

Trial 3. Georgetown Road and No. 3. Speed 10. 24 seconds.

"Pleasure overbalanced the displeasure. No surprise this time: sinking not noticed.

"Pleasant feeling begins the moment the picture appears, just the same as real scenes. Other just slightly unpleasant. Made a conscious effort to endure it. Less unpleasant than the black intervals in case of road alone.

"Pleasant feeling seemed to last during exposures of 3, but under a mist."

Nov. 15, 1912.

Trial 1. Audrey Burton and Ancient. Speed 24. 40 seconds.

"At first amused at both of them. Caricature ridiculous, but lost that character after about three exposures. Audrey a little bit silly-looking at first, but gained in pleasantness. A very decided relief in contrast with other.

Trial 2. A.B. and dark interval. Speed 24. 40 seconds.

"Didn't like her posing, so shifted her all round, and made a pretty little girl of her."

Trial 3. Ancient, same way.

"Terrible. Dreaded it before, and could hardly keep still while looking. No intervals remembered; feelings unbroken."

Nov. 22, 1912. (most indifferent, unpleasantness hangs over from yesterday.)

Trial 1. Hazel Troutman and Ancient. Speed 36. 40 seconds.

"Pretty distinct. Exposures of Ancient seemed much the longer at first and grew more so.¹⁵ About halfway through there was a drop in the attention. Strain of shifting unpleasant. Girl a great relief in contrast with other. Ancient very annoying at first; a person making up faces, not a caricature. After attention came back the pleasant one was much more prominent, its exposures seemed to grow longer. Ancient not blotted out, but effect diminished. Pleasant feeling lasted into exposures of unpleasant, floated out gradually. Unpleasant feeling lasted in opposite direction, but was cut off sharply. Feelings did not seem to run parallel anywhere."

Trial 2. H.T. and dark interval. Speed 36. 40 seconds.

"Intervals much longer than exposures. Girl very pleasant, but as it went on, she came to have a pained expression. . . didn't seem happy. Intervals seemed a little annoying, but a rest from the strain of looking;—felt that it was needed. Strain seemed to influence attitude towards picture, the interpretation of it."

¹⁵ The illusions in perception of time due to different intensity of feelings are worth noting

Trial 3. Ancient, same way.

"Wasn't conscious of any intervals at all until about half way through. Picture very unpleasant from the start. . . . Feeling got worse as time went on. When intervals were first noticed, they were a great relief. Last two times anticipation came in unpleasantly. Shivers ran up arms and back. Intervals seemed very short even when noticed."

The following will serve as a good illustration of the queer things that will sometimes happen.

Subject "L"

Nov. 1, 1912.

Trial 2. Falls of Song. (Ossipee) and dark interval. Speed 10. 45 seconds.

"Got auditory imagery, very tiresome. Intervals, suspense. Seemed to be trying to hold the whole thing up. No change in strength of feeling. Intervals seemed to be long. (Have always had a horror of falling water.)"

Trial 3. Among the Poppies, and dark interval, same way.

"As a whole, feeling quieting, but black hair of one child annoying. Length of intervals seemed to vary. When they were long, felt a strain, tension. No other feeling-tone during intervals. Strength of pleasant feeling did not vary, until black hair was noticed. Then attention wavered between the hair (unpleasant) and the rest of the picture (pleasant)."

Trial 4. Among the Poppies and Falls of Song same way.

"Poppy feeling tiring, children inquisitive about flower—empathy. Water quieting, pool at bottom pleasant. No auditory imagery. Feelings coextensive with the exposures. No changes noticed."

Such generalizations as that in Trial 2 do not seem to be of great value. In addition to Trial 4 above, consider the following:

Nov. 22, 1912.

Trial 6. Lily Pool and Whittier Falls. Speed 10. 20 seconds.

"Auditory imagery of falls. Pictures distinct. Falls more vivacious, both pleasant. Familiarity of pool lessened feeling."

Subject "M"

Nov. 15, 1912. (Sleepy.)

Trial 1. Sunset, Woods Island and Dachshund. Speed 24.
40 seconds.

"Dog funny, other slightly agreeable, so feeling on the whole agreeable, very slightly. Seemed like one continuous mildly pleasant feeling."

Trial 2. Sunset and dark interval. Speed 24. 20 seconds.

"Agreeable, very slight trace of feeling, none in intervals."

Trial 3. Dachshund, same way.

"Funny all through; feeling not broken by intervals."

Nov. 22, 1912.

Trial 1. Among the Poppies and No. 7. Speed 36. 40 seconds.

"One was positively disgusting, the other quite agreeable. Transition from pleasant to unpleasant a shock, uneasiness. Transition in opposite direction a relief. Feeling of disgust the stronger. Increased during each exposure. Both feelings increased as trial went on, disgust more so than pleasure."

Trial 2. Among the Poppies and dark interval. Speed 36.
30 seconds.

"Agreeable throughout. Increased after the first few exposures. Intervals, agreeable expectancy."

Trial 3. No. 7 and interval, same way.

"Exposures as disagreeable as can be. Intervals came as a sort of relief, then apprehension came in."

In the following case this subject was asked to pay particular attention to the transitions:

Nov. 15, 1912.

Trial 5. Among the Poppies and "It". Speed 24. 40 seconds.

"Agreeable and unpleasant. Transition a shock in both directions; tension, aversion, and expectancy, but both slight. Heart beats noticed at transition; beat stronger just before good picture; before the other seemed to slow up. This was noticed only a few times. At increase of speed in pulse, a little feeling of excitement, elation. Pleasantness comes as soon as picture is seen. After the good picture there is a break in the feeling. Uneasiness comes with the decrease of pulse and the unpleasant feeling joins this feeling when the picture appears."

Trial 6. Among the Poppies and dark interval. Speed 24.
30 seconds.

"Quickening of heart beat before picture, lasts through exposure, dies out slowly afterwards. Feeling of expectancy. Pleasantness seemed to be correlated with pulse."

Trial 7. "It" same way.

"Same effect reversed. Feeling is stronger when the pictures are alone than when they were together."

Subject "C"

Nov. 13, 1912. (A little sleepy.)

Trial 1. Lily Pool, and What Are You Laughing At? Speed 10. 40 seconds.

" . . . Old duffer silly and smirky. . . . Pleasantness increased, for children. The unpleasantness also increased. About equally divided."

Trial 2. *A*, and fly's head (much magnified). Speed 10. 40 seconds.

"Road came first, pleasant; later, only mildly so. Fly—first, thought it was a diseased one, creepy. Then a fly's foot. Then a plant; interesting, displeasure faded."

Trial 3. *A* and a white card. Speed 10. 20 seconds.

"Really very nice, had no idea that it was so pleasant. Grew pleasanter all the time. Intervals seemed fearfully long, wanted to get back to it. Depends more on associations than on artistic character."

Trial 4. Fly's head, same way.

"First just interested, then began to think of germs, fly-killing contests, etc. Felt flies crawling in hands; feeling grew worse and worse all the time. Don't remember any intervals."

Subject "K"

Nov. 6, 1912. (More cheerful than usual, a little sleepy.)

Trial 2. Among the Poppies and white card. Speed 10. 24 seconds.

"Feeling at its maximum at the very beginning of the whole exposure. Bright, cheerful, intervals unpleasant. Feeling steady during separate exposures, but decreased from beginning to end of trial as a whole."

(This seems a rather unusual mode of behavior of feelings. Subject "H" reported a somewhat similar effect with this same picture in a check trial Nov. 1. The "feeling-tone increased up to the sixth (exposure), then diminished. Intervals passive. Feeling level during single exposures.")

Trial 3. No. 3, same way.

"Revulsion increased during period as a whole, also during separate exposures. Intervals more indifferent than in previous trial."

Trial 4. Among the Poppies and No. 3, same way.

"Feeling less in both cases. Unpleasantness strongest just a little after beginning. Pleasantness best almost at the beginning. At shifts just vacancy. . . ."

Nov. 20, 1912.

Trial 1. Audrey Burton, and God Help the Poor Sailors.

Speed 24. 60 seconds.

"Wasn't any sharp contrast. Girl more pleasant at beginning than afterwards. Didn't pay as much attention to the man. One only a picture, the other a scene, so effect not exactly comparable. Feeling for drunk picture sometimes humorous, sometimes indifferent. . . . General effect of girl at first glance pleasant. Details later not so good."

Nov. 27, 1912.

Trial 1. Elsie Ferguson and No. 41 (comic, wolf with hat, etc.)

Speed 36. 60 seconds.

"Pleasant remained the same, while the unpleasant one grew worse. Transitions just blanks. At times pleasant one became indifferent, when attention wavered."

Trial 2. E.F. and white card, same way.

"Feeling-tone increased, intervals blanks. Brought up the image once in an interval . . . pleasant but faint."

Trial 3. No. 41. same way.

"Grew more unpleasant, feelings a relief. Both feelings stronger when alone."

In addition to the regular method, in which there was hardly any break between the exposures of the two pictures used, another method was developed from that of the check trials. Two pictures were used, in alternation, but with half the former exposure-time replaced by an interval, thus, a, interval, b, interval, etc. I hoped the results might prove more directly comparable with those of the check trials. The method was, however, so difficult, owing chiefly to the clumsiness of the apparatus, that it was used only at one speed. The main trials were at speed

10, and the checks at approximately double, 19+, so that a, interval, b, interval, etc., is to be compared with a, interval, a, interval, etc. It will therefore be sufficient to record materials and length of trial. The interval is always darkness.

Subject "J." Slight unpleasant tone

Nov. 7, 1912.

Trial 1. God Help the Poor Sailors, etc. 20 seconds.

" . . . Picture comical for a while, then feeling altered; went down below the normal unpleasant tone. Due to strain in watching, crooked lines, etc."

Trial 2. Hazel Troutman. 20 seconds.

" . . . Mildly pleasant tone, continuous, right through intervals and all. Began with a flash at very first of exposure. . . "

Trial 3. G.H. etc. and H.T. 20 seconds.

"Both distinct in their type, but both pleasant. H.T. very pleasant. G.H. a joke, not drunk but making believe. O. watching him.¹⁶ Difference between the two feelings very distinct. Seem to arouse different complexes of organic sensation. G.H., standing off, head back, hands on hips, etc.; other quite different. *No breaks in either feeling, but a sinking in the curve.* Feeling for H.T. much the stronger. Quickening of mental activity during each exposure."

Subject "I"

Nov. 7, 1912.

Trial 1. Audrey Burton and G. H. etc. 40 seconds.

"During first few exposures the unpleasant picture showed a somewhat greater increase in intensity of impression. After it had reached a certain stage it suddenly seemed to become uninteresting and there followed a sudden shift to the pleasant face; the attention was thereafter almost completely centered here and the feeling-tone was increasingly more pleasant.

"Feeling-tone from ugly picture was not completely inhibited by presence of pleasant image with its accompanying feeling-tone, but gave rise to a composite feeling-tone. The agreeable feeling seemed more easily interrupted, and so far as its effect

¹⁶In taking down introspection it was occasionally put into the third person, as O. (observer) was used. *The subject's phraseology was not otherwise altered.* In some cases, especially those which involved mixed feelings the subject was requested to write out his own introspection in full.

could be observed during the exposure of the disagreeable picture, it heightened the unpleasant effect."

Trial 2. Audrey Burton. 20 seconds.

"Seemed about seven exposures. During first four there seemed to be no persistence of feeling-tone during interval between exposures. Each new exposure, however, had a more immediate effect than preceding one. This was reinforced during last exposures by feeling-tone holding over. Interest was much higher at end than at beginning."

Trial 3. G.H. etc. 20 seconds.

"Seemed about eight exposures. Feeling-tone held over from very first exposure heightening effect of each succeeding one. Picture was beginning to lose interest on last exposure."

Subject "F"

Nov. 7, 1912. (Pretty well, but a little tired.)

Trial 1. Fly (magnified). 20 seconds.

"At first there was a disgust, shrinking, growing in intensity, first two exposures. Then curiosity came in; new attitude, feeling disappeared, became neutral, interest. Eye movements noticed; effort to make out object."

Trial 2. Eugen Ysaye. 20 seconds.

"At first dislike. Second exposure looked at name, then thought he was wasting time. Third, forehead looked good. Fourth, nose and hand good also; finally thought he wouldn't mind having him as a neighbor. Pleasant at end. Imagery agreeable now."

Trial 3. Ysaye and Fly. 40 seconds.

"Attitude towards fly belongs to the unpleasant side. No curiosity, no fine lines about it. Poor aesthetically, in marked contrast to lines of face. Pleasantness grew in other case, also interest. Two feelings divergent, with the pleasantness at a greater angle. . . . Transitions marked, like shoot-the-chutes. During intervals, expectancy always. Attitude preparatory, brought feeling for the comic picture."

Subject "L"

Nov. 8, 1912.

Trial 5. Hazel Troutman and Ancient. 60 seconds.

"Striking contrast, strong feeling. Repulsive, snarling feeling; crude, vulgar in every way. Other very pleasant, refined,

etc. Antithesis of tension and relaxation. Vulgar one, unpleasant tension; actress pleasant relaxation. Also a certain pleasant tension in case of actress and unpleasant relaxation in other. During first interval after unpleasant picture, the feeling was unpleasant. During all other intervals, the feeling was pleasant, concerned only with picture of actress. At beginning, feelings about equal. Pleasantness grew as trial went on, unpleasantness grew less and less."

Subject "M"

Nov. 8, 1912.

Trial 1. Hazel Troutman. 30 seconds.

"No feeling-tone at all."

Trial 2. Fly. 30 seconds.

"Disgusting all through, intervals and all, and grew worse as trial went on."

Trial 3. Hazel Troutman and Fly. 60 seconds.

"Likes H.T. fairly well. Other as disgusting as ever, perhaps a little more so. During intervals after fly, expectancy for other picture. Intervals after the girl not remembered. Pleasantness grew towards the end; no difference of degree noticed in the disgust."

With this contrast effect it will be of interest to compare the following trials. The method is the more general one, with an interval substituted for one of the pictures in the check trials.

Trial 7. Drawings, faces 1.a and 7 (artist unknown).

Speed 19. 30 seconds.

"Both disagreeable, one less so than the other. Almost got on his nerves."

Trial 8. 1.a and white card. Speed 19. 30 seconds.

"Disagreeable, but not so much so as before."

Trial 9. 7, same way.

"As disgusting as he can conceive. Felt like closing his eyes."

"1.a. sketchy, clear-out, almost stony. 7, brutal." (Cf. Charles Rann Kennedy's, "John Heron goes and sits down on the keyboard of the piano. In the bass. *The cacophony relieves him.*" The Necessary Evil, p. 60.)

The method seems to have justified itself. Our results show considerable variety, ranging all the way from inhibition through partial and complete parallelism, with occasional fusion, to in-

crease in cases of contrast. Feeling may increase gradually, decrease gradually, change intensity suddenly, with a sort of staircase effect, or stay fairly constant for a considerable length of time. McDougall, in summarizing, for the purposes of his argument on the psychophysical problem, the laws of the behavior of feelings, says, "When several sensations of pleasurable tendency are present together, their tendencies re-enforce one another; and when sensations of opposed tendency are present together, the opposed tendencies partially or completely neutralize one another. Or, if the pleasurable feeling tendencies be regarded as of positive sign, and the disagreeable tendencies as of negative sign, we may express the facts by saying that the feeling-tendencies of the various sensations simultaneously present to consciousness are algebraically summed, and, according as the resultant is of positive or negative, the feeling-tone of consciousness is pleasurable or disagreeable, or in other words, the individual feels pleasure or displeasure. But the sensations are only one class of occasions of pleasure and displeasure. Every form of mental activity tends to affect the feeling-tone of consciousness positively or negatively, and the stronger or the more intense the activity, the stronger is its feeling tendency. In general terms it may be said that the smooth flow of mental process towards its proper end tends to pleasure; the baffling or hindering of it by any obstruction, conflict of tendencies, or difficulty of any kind, tends to displeasure. And of all such feeling-tendencies the law of algebraic summation holds good, perhaps not absolutely, but in the main and in general. The feeling-tone of consciousness at any moment is, then, the reaction of the subject as a whole upon all the many feeling-tendencies simultaneously influencing it."¹⁷

In view of the results we have quoted, it seems that such statements as the above hardly fit the facts. With simple sensations, the case may be different. We shall have to consider that, to some extent, later. With material appealing to what may be called instincts, the results are much more complex than

¹⁷ "Body and Mind," p. 313. ¹⁸ *cf.* McDougall's "Introduction to Social Psychology," pp. 54-7.

has been generally realized. Simple algebraic summation seems to be unusual, rather than the general rule. We may leave our results to speak for themselves on this point.

As regards mixed feelings, the results seem to be positive, but we must admit that the experiences are decidedly complex, and somewhat difficult to introspect. The language used by the observers seems to indicate something different from that index of the general condition of the whole organism which is commonly spoken of. We have discovered certain possibilities in the behavior of feelings. There seems to be need of a more careful analysis of the feelings which do thus behave. Some attempt at this analysis was made in the next series of experiments.

A.3. As just indicated, the work of this section was the description, and at least a rough analysis, of the feelings brought out by the pictures. The introspection was not restricted entirely to the feelings, however. The subjects were asked, towards the close of the series, to report on the intellectual attitude towards the experience. They tended to do so from the first, and some of the results seemed so suggestive that I decided to make the best of the situation and see what could be gained. The results have, I think, justified this course, objectionable as it might seem to add to a task already difficult. In this case, the introspections throw a great deal of light on the problems of the relation of feeling to apperception, and the nature of apperception itself. Accordingly, I shall quote, for its general value, some introspection that does not bear directly upon our special problem.

In the earlier portion of this section, the check trials were conducted with the same method as in the previous section. Later, series of trials, using one picture with various others, were used. Two such series were used with all, or nearly all, the subjects. These will be given in full.

All trials in this section were at speed 24. i.e., the single exposures lasted $1\frac{1}{4}$ seconds.

First Series: To avoid repetition, the following abbreviations will be used (a) stands for "There was a race back to Albion"

(a picture from a magazine), a man driving over a very rocky road, followed by another in the distance; and "Mrs. Aaron Ward Roses." (b) represents the first of the above and the drenched cat, "I am for Prohibition." (c) represents "a" (the garden with the path), and the roses of (a). The method was not the same for all subjects, but the changes were only slight, and will not interfere with comparison.

Subject "F"

Dec. 19, 1912. (Tired.)

- (a) "The two feelings are quite distinct. Flowers, quietness, calmness, at-home-ness. Fellow lashing horses, puzzled, frustrated feeling, curiosity as to meaning of picture. Noticed many details of picture, hard, strained, stony feeling. Flowers a rest, decided relaxation."
- (b) "The cat ridiculous. Solved the picture for him. The man was tight, and taking it out on his horses. All one general feeling of humor. The two mutually supported each other."
- (c) "Fine; quiet restful feeling. Caught himself taking a long breath when the woodland scene came. Made out the roses. Couldn't have told what they were before. Now they are real roses. Is sure the woodland scene brought out the full realization of, and feeling for, the roses. (The team seemed to inhibit it. Before was a reserved calm; now full relaxation.)"

Race to Albion and a

"They certainly did contradict each other; in direct opposition. Couldn't make out which was getting the upper hand. Feelings tended to neutralize each other. Not so strong this time. Runaway not nearly so roistering, so blustery, as when cat was there. Woods calm, peaceful, but not the distance, the far-away feeling that was there before. With the roses there was the feeling of Indian summer,—dreaming. That was taken out of it by the old man."

Subject "M"

Dec. 20, 1912.

- (a) "One was agreeable, the other was funny. Neither very strong, just a little above indifference. Attitude not active. Pictures coming to meet him. Feelings continuous, two thin parallel lines."

- (b) "Cat disagreeable. Other at first indifferent, then grew to be disagreeable. Feeling-tone of new picture seemed to be projected into the old one. Attitude towards the race picture now active. Felt as if he had something to do with it, and he didn't want to.
 "Cat disagreeable from the first. Has an instinctive dislike for cats. Felt a desire to drive it away; wanted to strike it."
- (c) "Liked both of them. The picture of the road was slightly more so. Felt himself walking along the road. Other more passive; more agreeable; feeling more pronounced than before.
 "The two feelings were continuous, and partially fused."
 "Was looking at the roses. More real, and nearer than before, but he was not active, just looking.
 "Was walking along the road, and enjoying the scenery."

Subject "L"

Dec. 20, 1912.

- (a) "Motor exhibition very interesting. Heard all the noise; could feel himself on the seat; liked it. Roses rather tame."
- (b) "Cat more positively unpleasant than the other was interesting, wet cold, made her shudder a little. Wanted to read printing but it is reversed."
 "Rose picture. . . quieter, more a spectator."
- (c) "Roses unpleasant; looked too much like specimens."
 Just a short exposure of both first, then roses, full exposure. Seems to lead to over-estimation of exposure of roses all through.
 "Garden pleasant, more so than formerly. Seems to be due to the contrast effect."
- "a" and I am for Prohibition.
 "Cat is awful. "a" spoiled by it; seemed gray and uninteresting."

Subject "H"

Dec. 20, 1912. (Eyes tired; sleepy; didn't sleep well last night.)

- (a) "Queer; didn't understand the picture of the man with the horses; occupied with the attempt to see what it was all about. Other picture did not hold attention. No feeling of pleasure; only curiosity."
- (b) "Both very, very unpleasant; rivalled each other pretty .

closely. Hated to see one coming, and then hated to see it go because the other was coming. Cat seemed to interpret the other. Whole thing was tipsy. Both bad as they could be."

- (c) "Very pleasant, both equally pleasant; helped each other very much. Could get the odor of roses; could see them on bushes; made a scene of this picture somewhat like the other. Got the odor of the woods, too. Two experiences distinct though similar. The rose scene had a path too."

"a" and I am for Prohibition.

"Cat very unpleasant; increased as it went on. Other not as pleasant as when with roses but became more pleasant during trial. Unpleasantness the stronger."

Subject "I"

Dec. 19, 1912. (Went to a dance last night.)

- (a) "Scarcely any development of feelings; seem to inhibit each other more thoroughly than any other he has had. Seemed as if there ought to be feelings, but they were cut off before they could develop. Almost a case of sensation without feelings. The incompleteness, inhibition, caused a sort of organic feeling, somewhat akin to the sort of strain in dreams when one tries to move and cannot.

"Roses were something pleasure-giving; but these seemed to lack reality of appeal; not fresh roses.

"Other very 'bill-board-y.'"

- (c) "A good deal of reinforcement . . . the walk more prominent, suggests activity, very real. Seemed to prepare the way for roses.

"The first picture seen was the roses; same as before. . . but after the walk had come, they were helped in genuineness of appearance, got more dewy-looking. Walk picture gave a general attitude towards vegetation, which included the roses as well.

"Feelings for roses closely related to touch, something wearable.

"Walk very suggestive of all the agreeable feelings of outdoor life."

- (b) "Cat is an awful looking creature.

"Other very, very rocky.

"Two more separate than the two pleasant.

"Cat unpleasant to touch.

"In other case purely a spectator, watching for a calamity.

"Each one stronger each time it appeared."

Subject "J"

Jan. 9, 1913.

- (a) "First thing just a physical shock; reaction to sensation. (Race picture) Nothing made out. Then flowers . . . indifferent too. Both pictures practically indifferent most of the time, but once with each picture memories came in, and there was a momentary pleasure. "Most of the time there was curiosity (attendant strain sensations), also organic sensations—feeling of going out towards pictures. Self-activity prominent at times."
- (b) "Still puzzled about Race. "Three attitudes towards cat: first surprise, amusing, characteristic organic reactions; then looked at cat's eyes, took the cat as a man who had been thrown into a mud puddle, amusement; then the cat was personified, saying, Look what you've done to me. Amusing again. Then it looked dirty. Wanted to kick it away; then wanted to wash it off. Unpleasant. "Other still puzzling. More and more strain in connection with the desire to find out what it is."

Race, and God Help the Poor Sailors.

- "The new one called up some of his old experiences in connection with the picture; empathy. . . . The fellow is not drunk, but just cutting up.
- "Other just the same as before.
- "Increasing strain, a sudden shock in head towards end, like striking of heels on sidewalk, etc.
- (c) "Very strong curiosity in "Vorperiode." Seems to be localized in forehead. "Roses came first. Rather mild liking; then a much stronger. Liked it because he could walk there and pick flowers, or with a lady friend. "Then a sudden flood of consciousness of whole organism, with the thought that he was lying out there in the sunshine with nothing on; very definite pleasantness. The bunch of roses only secondary. "During moments of strain, as in previous trials, the organic sensations noticed were disappear."

Subject "K"

Dec. 18, 1912.

Trials 1 minute.

Race to Albion, and Man with Sargent Roses.

"Pictures seemed to be on different levels; one in garden static; other called up situation . . . being at a fair,—a little romantic. . . . Man unable to control his horses . . . no pronounced feeling, i.e. neither pleasant nor unpleasant."

Mrs. Aaron Ward Roses, and Man with Sargent Roses.

"Just at times the new flowers seemed real. Thought how fine it would be to smell them. Flowers by themselves better than the other. Man seems to have the flowers and O. is out of it."

- (b) "Didn't like the cat, but not decidedly unpleasant. Other somewhat romantic. Liked to look at it, like being in some Western city or something of the sort; a little less pleasant than at first, because he is used to it."

Subject "C"

Jan. 8, 1913.

- (a) "Enjoyed it very much. At first just wanted to know what was going on there. Thought those things were gardenias, then they turned to roses. Taken symbolically. Other not noticed at first, except it was a generally exciting affair; then taken as an elopement. This due to the roses; decidedly emotional experience, rapid breathing, etc."
- (b) "Liked cat; reminded her of a time when she dumped an angora cat off a float. Other didn't get her attention. Most of the time was wondering what the letters were under the cat."
- (c) "Very pleasant, much more so than before; taken in the same general way, but became serious this time.
"At first, path only an interval, very first time annoying; wanted to look at roses. Towards end was taken into association with roses.
"Felt herself breathing so hard there didn't seem to be half enough of her to breathe as deeply as she wanted to."

In the second series "The Lily Pool" was used throughout, alternating with six others, as follows: (a) No. 88 (a little

church on a hillside); (b) I am for Prohibition; (c) All I Did was Growl; (d) Sunset at Wood's Island; (e) The Garden Wall; (f) Brook in Middlesex Fells.

The length of each trial was 30 seconds.

Subject "F"

Jan. 9, 1913.

- (a) "The two were immediately associated. The children were playing at the pool below the house. The house added to the beauty of the picture; gave protection, a homey feeling. House not aesthetic, but children decidedly so. One general attitude, parts of the same thing."
- (b) "The pond-lily picture lacked the artistic finish, children were not as well groomed; pond not fit for them to play in; the cat had crawled out of it. Grew worse and worse.
 "Not a story that he tried to make up. Just came with no effort. Tried to keep his former attitude, but couldn't. The two struggled, and the bad won."
- (c) "They fit in fine. Hard to describe, but seemed appropriate. Dog belong to children. Pond with lilies . . . green leaves against their original beauty . . . etc. . . a beautiful scene.
 "Not as with house. Then coziness, safety; now fitness. Mutually support each other aesthetically. Feelings parallel."
- (d) "Didn't go together at all; two distinct fields. Couldn't pull them together if he wanted to. Feelings not parallel, but broken.
 "Children not enjoyed so fully as before. Water picture cold, like northern scenery, ice, slush, etc."
- (e) "Here there was a togetherness, a coherence not there before. Four children, two on the wall, looking down at the others. Children seemed to be gathering lilies with more zest because they were watched.
 "One feeling—the two were simply parts of a whole, much closer than ever with the house."
- f) "Both pleasant, very pleasant, but they don't fit in together. Two distinct pictures, not so far apart as the "moon-night" scene and the children. Woods too wild for the children, though.
 Feelings not broken . . . in along parallel; both perfectly pleasant."

Subject "M"

Jan. 10, 1913.

- (a) "Both agreeable, but different. The transition a shock; organic sensations, a little like beating of heart but not sure.
 "Attitudes . . . children seemed to be related to him, and he was just standing and watching them play.
 "House. . . just seen as he was taking a walk in the early dawn . . . general haziness. House itself very insignificant. Situation as a whole pleasant.
 "The two experiences entirely separate, remained the same throughout."
- (b) "Cat picture positively disagreeable; other agreeable. In the same way. There was a sort of jerk at transition.
 "Children's picture more agreeable this time than last time. Contrast seemed to cause this.
 "Wanted to kill it (the cat). Attitude towards children same as before.
 "General field of appeal is that of empathy; nearness prominent. Would like to carry the children on his shoulders. Nearness to cat offensive."
- (c) "Children's picture very agreeable; dog not exactly agreeable; ridiculous or pitiable; shifted a little. Felt as if he would like to show the children that dog.
 "No shock at transition; just as if he were turning his head a little.
 "Feelings separate and broken."
- (d) "Both were agreeable; a sort of jerk from one to the other.
 "Children's picture more agreeable; growing more so.
 (Sunset) . . . "seemed to be on the bank of the river; much like the river in his native village.
 "Two experiences wholly unrelated."
- (e) "Both agreeable; just one feeling; no break at all. Difference only in that the new children are strangers not related to him, but he likes them.
 "No transition shock at all."
- (f) "Both agreeable, but there was a distinct transition, and in a way there was a sort of contrast.
 "Wood,—walking alone . . . a sort of poetical loneliness. The children were with him, good company."

Subject "L"

Jan. 10, 1913.

- (a) "Breathes in with the Lily Pool and out with the church. Lily Pool seems to demand expansion; reverse with the church. Like looking into the two ends of a funnel. Church interesting, but there is no life in it, no motor interest. . . ."
- (b) "Rather shocked to see the cat again.
"Breathing effect reversed, but it was too fast for her this time.
"Just a very short exposure of kitten, disturbed her a good deal.
"Getting tired of children . . . familiarity. Whole experience a chilly one."
- (c) "Breathing in with the Pool and out with the puppy; brought back the feeling she had when she came in.
"Children seemed to be in a class with the puppy.
"General warmth in whole experience."
- (d) "Sunset reminded her of an experience in the Golden Gate, interesting. Other just an unpleasant interruption; took her time from the new one."
- (e) "The Garden Wall unpleasant because the child's legs look too long and stretched out.
"Old one pleasant."
- (f) "Auditory image of stream. Could feel the woods.
"Likes the Pool better this time. Liked the idea of life. Woods a little subtle and uncanny."

Subject "H"

Jan. 17, 1913.

- (a) "Lily Pool familiar and tiresome. Other a decided relief, pleasant each time it came; exposures seemed longer than of children.
"House interesting; a sort of entering into the atmosphere of the place; felt the coolness, imagined the family, etc.
"Other annoying, irritating."
- (b) "The children became very pleasant. Rather felt that the interest was forced by the contrast; no imagery or associations.
"Cat annoying, unpleasant to look at.
"Delighted when the trial stopped."
"Pictures didn't seem to enter her consciousness; just things she had to look at."

- (c) "Dog pleasanter than children; sort of quaint humor about it; seemed to put herself in the position of the dog and feel that the mask was too much punishment for a little growl.
 "Children just very slightly on the favorable side; not real, but mere figures. Likes the overhanging tree.
 "Transition neutral.
 "Pleasure in dog a continuous feeling, only weakened a little when children were there. Children gave a slight boredom, which did not hold over."
- (d) "Water very, very pleasant. Children remained neutral; their exposure seemed very much shorter.
 "Water scene a beautiful picture; not just a picture, a real view."
- (e) "Made all the children real, i.e. just beyond the border, wouldn't have expected them to speak. Seemed to be the same two children, in spite of the difference in color of hair. Wouldn't have cared enough about them to speak to them.
 "New picture seemed to redeem the older one. Whole experience very slightly pleasant. Wouldn't care to repeat it."
- (f) "Interesting. Children first . . . bored feeling.
 "Stream very pleasant; auditory imagery, water rippling over the stones.
 "The transition from stream to children was filled with imagery of the other water scene; almost wiped out picture of children. Didn't notice them at all, just conscious that they were shown.
 "Picture of children leaves very little impression; has had to stop and think each time to realize that the pool was shown."

Subject "I"

Jan. 16, 1913.

- (a) "Accentuated each other to a very marked degree, feeling-tone grew almost continually more pleasant. Quite distinct, no fusion. Difference in light seemed to relieve the eyes. Very restful to get to the church; very interesting to go back to the girls.
 "Entered more actively into experience with the church; walking down the slope.
 "Other independent of him, but attractive."

- (b) "Cat unpleasant, seemed to spoil color of pool. Green not so saturated. Unpleasant feeling held over. Half way through, comic aspect of cat came up and made him laugh. After that, seemed to pay a different kind of attention to the pool; colors seemed to be truer. Seems queer, but there does seem to be a difference in the objective appearance in the two parts of trial.
"Feeling, alternation of nausea and well-being."
- (c) "Aided each other rather than interfered. Two appropriate to each other. Didn't go as far as actual ownership of dog by children, but they went together well.
"Passive observation of pool. Dog taken actively. Thought of patting him, tumbling him round, and maybe taking the mask off him.
"Both pleasant. Seemed to be something carried over from one to the other. A general feeling of internal comfort. In addition there were elements peculiar to each picture; one aesthetic, other social."
- (d) "Pretty hard to find himself, to settle down to any definite state of mind.
"Children first, same as before.
"Other picture altogether different, seemed dangerous. For a while it shut out the pleasure in pool. Latter just marking time, while he was trying to look at water scene and understand what it was. After he decided that it was only cloudy weather and not really dangerous both were slightly pleasant, but totally incompatible.
"An effort to get any setting for boat picture, unstable . . . outing, practical utility.
"Other constant, rich with color and meaning.
"Seemed to try to hit a mean that would suffice for both, but couldn't quite make it. Was straining to look at the children, when this is not needed, and when looking at other picture, there seemed to be traces of his attitude to pool. Imagery did not hold over."
- (e) "About as nearly like a single picture as could be.
"Feeling-tone augmented.
"Stronger lighting makes pool a little more real. Pool is the halting place. Children on fence are on their way to the pool. Sort of a story,—not four children, but two. Pool the prominent one for feeling. Aside from this the feeling was just one throughout."

- (f) "Rather a curious effect, two feelings in connection with brook. Transition from color to lack of it essentially unpleasant. At the same time, the appeal to general organism was very pleasant. Called out a feeling of its own.
 "Other a bit monotonous, but noticed the childlike character of the hands of child holding flower. Coloring also pleasant.
 "Two pictures distinct in feeling, call forth different attitudes. Pool passive, other active."

Subject "J"

- (a) "Both pictures very pleasant, but completely separate. Towards end, thought, if they are both pleasant, why in the dickens don't they run together. Then they did with a sort of shock.
 Church picture . . . associations . . . touched off erotic complexes . . . social experiences.
 "Lily Pool did not call up such associations until towards end.
 "A good deal of empathy. Varied between objective and subjective.
 "Organic sensations . . . in throat and upper chest, connected with panting, etc., i.e. those organs. Attention very lively; passive all the time."
- (b) "An entirely different experience. Was expecting something he had never seen before. There was a shock of recognition, "Oh, it's you". . . Cat indifferent so far as pleasantness-unpleasantness was concerned. Next time it came, was a little amusing. Seemed to take away from the other at first. Later the pool became dominant, but not so strong as before. At the very end, attitude of "clever(?)" practical joker came in. Was setting the cat on the middle of the pool. Possibly due to his disappointment at the cat. Fiendish delight in tormenting the little girls by putting the cat there.
 "Trial cut off before the experience had completely developed."
- (c) "More like the first one in the amount of association called up. Girls and pool getting more or less indifferent.
 "Other came as something entirely new. Thought in-

stantly as the dog of the girls, but he seemed too forlorn. Separate for a while. Then the thought came that he was inside, and they were out. A certain sympathy because he is muzzled; no tendency to help him.

"Feeling-tones of the whole thing rather indifferent, but border on pleasantness. (But the rough skin of the dog's hindfoot looked like a rhinocerus's hide . . . a certain slight horror, no organic reaction).

"Organic sensation not focal during the experience."

- (d) "Girls first, full exposure, seemed unusually long, mildly pleasant.

"Wondered what the next was going to be. When it came, more or less of an organic shock going downwards in his body, . . . pleasant.

"Pleasure lasted all through.

"Associations came rather slowly. None for the pool. Other . . . (1) out boating, (2) an early morning on the Elbe.

"Whole experience very sluggish."

- (e) "New picture came first. Brought a very pleasant feeling-tone instantly. At the same time there came a thought of the other picture, with some imagery, not completely. Thought the whole experience was going to be girls. At first a little distinct. Then girls on wall were looking down at the other. But a few incongruities; no fence in pool picture. Girls on wall a little too high to see in the other.

"Two thought together all the time. Looking at one with the other at the background. Then the reverse.

"Association widened in scope. Children in general; the children in the streets of Berlin; then some young relatives of his, etc."

- (f) "New one came first. Relaxation (organic sensations in chest). Associations of experience in woods.

"Other brought back association of first trial. Basin in botanical garden.

"Feeling-tone of pictures runs together, very pleasant. The prominent thing was the feeling-tone. Pictures intellectually distinct."

Subject "K"

Jan. 17, 1913.

(Rather sleepy, hasn't been up long; not thoroughly awake yet).

- (a) "Likes both of them slightly. Difficulty in making out church scene, because lighting is not so bright.
"Both pictures remote. . . ."
- (b) "Cat unpleasant, aversion, just slight. Other slightly pleasant,—light, brightness, coloring, summer, etc. No special interest in the children."
- (c) Seemed to associate the two pictures. Really one to him. Dog in one corner looking at children in the other. Playfully sullen.
"Somewhat pleasant. Seemed nearer than before. No break at transition."
- (d) "New one the pleasanter. Seemed to be sometimes transposed to the scene and watching it; could see all the surroundings. Could localize the picture of children too, playing on a little hill.
"Transition a gap."
- (e) "These two are connected too. Two children on fence are looking down at others. Liked the ones on the fence better, because he could see their facial expression.
"Not localized in space; only saw the picture.
"Both pictures slightly pleasant. Just the same except that those on fence are more "active." They are watching the others, taking it all in. He likes them better."
- (f) "New one seemed to be in the evening; other morning or afternoon. "Road" pleasanter when it got him into the mood of being alone. Other getting to be pretty commonplace, indifferent."

The above results seem to point to a very close relation between the feelings and what are commonly called intellectual processes. The mutual influence of the feelings depends largely upon the similarity of the presentations, i.e. similarity in meaning, in appeal. When the two feelings are of like intensity, if the total reactions are widely dissimilar, the result is inhibition. Inhibition diminishes with increasing similarity, until in some cases, there is a tendency to parallelism, and rarely, fusion.

When the fields of appeal become nearly identical, the feelings either become one, or increase by contrast, in alternation, according as they are of like or opposite value. When the feelings are of unequal intensity, inhibition continues farther up the scale of similarity. Further, there are individual differences, which seem to be related to the scope of attention. The above statements represent the hypothesis which resulted from all the work so far. The remainder of the investigation is an attempt to test its validity.

A 4. In this section the tasks of the two previous were combined; the subjects were asked to trace the course of the feelings and describe them as well. The speed ranged from 10 to 72, the same pictures being used in series of trials at various speeds. The first two series began with the lowest speed, i.e. the longest exposures, while the later series started from the opposite direction. Excepting a few odd trials towards the close of the season, the work was the same for all the subjects. The check method of the preceding section was adopted, a pleasant picture being used with another pleasant one and with an unpleasant one, etc., while some series were concerned with other pairs of opposites, such as excitement and calm. The materials were entirely new, so that familiarity is not a disturbing factor. A brief summary of the results with several series will be given with such examples of the introspection as seem important.

In the first series only two pictures were used: (1), a crude 'comic' entitled *Who Moved That Keyhole*, being an intoxicated man in evening dress with battered hat, trying to unlock a door; (2), a little boy in red jacket and cap, from *Nordmark*. The speeds used were 10, 12, 14, 16, 17, 19, 23, 24, 29 and 36, in the order stated. The length of the first trial was thirty seconds, of the rest, twenty.

Subject "L" found 'neither picture very striking.' They began on the 'same level of interest,' and 'blended very easily.' 'The man excited curiosity more than the little boy.' (Speed 10). 'The man is just dressed up for the occasion.' (Speed 12). As speed of exposure is increased, 'motor effect comes in more.' 'feels more as if she were the people' 'Man's and

getting tiresome, felt as if she were holding the key.' (Speed 16). All feeling except interest in experiment disappeared after Speed 17. At that speed, there was 'much more interest in the little boy.' Speed 12 seemed slower than 10, and 14 the same as 12. A change is noticed with 16 and again with 17.

"J" found the man 'less real,' 'tendency to comic'—'not a real drunk, but a man acting.' The boy is serious. 'Pictures do not interest at all. Feeling-tone not noticeable after first exposures; no fusion.' (10). The interest in the pictures increased, they seemed brighter, and 'the whole experience more active and vital' with increase of speed. (19). At (23) the pictures ran together; he 'saw the little boy standing laughing at the man.' 'Mildly pleasant throughout.' This unification disappeared and the pictures became indifferent at the highest speeds.

With "C" and "H" there was a good deal of feeling, chiefly displeasure. With "C" the maximum is at (19), the pleasure being completely inhibited then. There is no feeling at (36). With "H", the highest point is at (23), and at (36) it is still 'very unpleasant. Haven't reached the point of tolerance. Still struggling against it.' "H" did not notice the difference in speed until (36).

With "K" the two were well contrasted at first, opposed morally, 'about equally balanced,' 'feelings distinct.' The contempt for the man was stronger at (14) and (16), and the boy 'not so pleasant as he might be, . . . pampered and overfed,' 'being spoiled and might get where the other is.' At (19) and the higher speeds, the two were put together into a situation, and the 'slight antipathy for the kid is gone.' The feeling is practically nil after (23).

"F" found that the pictures 'opposed each other, no fusion, radically apart.' 'Man the dominant picture.' (10). At (16) the 'man has a sort of impudent look, the little boy looks more friendly. (Before, he looked shy.) They seem not so far apart—in same neighborhood. (Before, they were far apart). Think it is due to speed.' Then the man became humorous (17), and finally 'stagey.' 'Is really tipsy, but it is partly put on.' The boy gets more human and real, and becomes the dominant picture. The spatial relation disappears. The pleasure increases and inhibits the displeasure completely.

With "I" the feelings were at first slight (10), then increased, 'more contrast' (12); then he seemed to interpret the two as phases of a single thing (14). The interpretation held throughout the rest of the series. The pleasure was dominant at (14) and (16), then gradually gave way, until the displeasure came to be dominant at (36). The feelings alternated completely.

The general effect of the shortening of the single exposures is increased inhibition, as might be expected. The tendency to assimilation in meaning and to a synthesis of the two presentations is, I think, worth following up. It reappears constantly.

In Series II, a picture of a road through a pine grove at Concord was used, first with the rear view of the Paul Revere house, and then with a picture of a path in the New England Gardens. The trials lasted 45 seconds each, and were made with speeds 10, 23 and 36.

"K" found all three pleasant, the trees less so when alternating with house than with flowers. The feeling diminished with the increase of speed, but was not inhibited entirely. Speed (10) was the best.

"F" found the first trial 'a numbing experience,' could hardly say anything afterward. 'Neither attractive.' In the same trial, the 'trees were not at all as before.' 'Would hardly believe the woods scene was the same picture. Two not parts of same scene, or anything of that sort, but similarity helped. A quickening, exciting experience.' The feelings increased in the trials at higher speed. 'More active interest.'

"L" found the house 'close, stuffy,' the trees 'cool and green.' There 'seemed to be a difference in the air she was breathing.' The flowers were 'a great success,' the 'trees gray and uninteresting in comparison, not so pleasant as before.' At (23) the feelings are less marked, and at (36), could see the pictures all right, but not time for feeling to develop.'

With "H" the 'walk between the trees' was 'a great relief every time.' . . . 'Glad to get away from the house.' The flowers were 'a pleasant surprise, afraid she would dislike the lane, but there was a peacefulness and restfulness about it that was charming.' At (23), the lane is less effective in both cases, while at (36) the pleasure in the lane was completely inhibited by the unpleasantness of the house, while the trees gained the day over the flowers. In each case the dominant picture seemed to be exposed very much longer.

"C"'s results have to be discarded on account of the influence of associations.

"J"'s results in the first trials are almost exactly the same as those of "F". The feeling was less at the higher speeds, trial 5 being indifferent, and trial 6 mildly pleasant.

"I" 's introspection will be given in full:

Speed 10. 1.

"Quite a strong contrast. Ugly one looked uglier each time. Not sure that other one was improved, because of the alternation's being unpleasant. Pleasant, but there was unpleasant anticipation of the other.

"After about six exposures of house, felt like doing something, demolishing house, or clamoring out against having to look at it.

"Feelings during exposure of woods mixed—pleasant, but worried. Two parallel, not alternating.

"While looking at ugly picture, his feeling was for that alone."

2.

"Fine. They reinforced each other a great deal. Easy to imagine them parts of one scene. Just one pleasant feeling increasing throughout."

Speed 23. 1.

"A queer thing happened. Woods picture seemed to hold there for quite a while once, yet the other picture was much more present to his mind. And in the duration of the woods picture was just using it as a sort of brace to help him ward off the other. Same mixed feeling with the unpleasant anticipation the more prominent factor."

2.

"General effect just the same as before; just one feeling all through, nearly apprehended as one picture. . . ."

Speed 36. 1.

"In order to get anything out of it has to concentrate on the house; woods almost a blank. Still succeeded in giving the house some meaning, but not very vivid. Rather unpleasant, but not so much as before. Woods indifferent."

2.

"Got a clear interpretation of it all the time; kept its feeling-tone pretty well. Seemed to be riding through at high speed. Woods altogether different from in (36).1."

The familiarity gained during the early exposures, when these are at low speed, seemed to influence sometimes the results at high speed. Hence the rest of the series in this section were taken in the opposite order. This method brings out the selective and assimilative features of the apperceptive process still more clearly

Series III will be given complete.¹⁸ (a) represents speed 36; (b), 29; (c), 24; (d), 16.

In trials-1, an old lady with her knitting, from Leksand, was used in alternation with a frightful 'comic,' a caricature of an old woman, with a dog, the latter being humorous. In trials-2, Luise von Preussen was substituted for the caricature. Subject "K"'s results are dropped, owing to a mistake in the order of trials. The length of each trial was 30 seconds.

Subject "L"

Feb. 28, 1913.

a-1. "Faces stood out strongly. 'Old woman' etc. worse than the other was good. Other had a slightly vicious expression . . . a little cross-eyed, etc. A motor response; wanted to do something about it, but doesn't know what."

a-2. "That was the niece or granddaughter of the other. Old lady looking at her,—more intellectually alert than the girl. Girl rather pathetic,—but more a symbol than a living object. Old lady was living—much better than in (1), more human, didn't have the witch-like cackle you might have expected before.

b-1. "Leksand old lady looking worse again; back to her

¹⁸ The task of the present section was so difficult that the subjects, even those of long experience tended largely towards objective description instead of strict analysis in subjective terms. They described the pictures instead of their own feelings. However, in view of the interesting way in which these descriptions vary with the method of exposure, it seemed best to allow the statements to be made in the more natural form, rather than to run the risk of falsifying the results, or getting none at all by strict insistence upon the technical form of report. The variations in the descriptions may be interpreted as indicating changes in the response of the subject, with some risk of error, it must be admitted. Mere verbal associations may creep into the objective statements and it may be difficult to sift them out from the genuine response objectified.* Still, with all due respect for laboratory tradition, I feel that in such a case as the present, where the facts of inhibition, parallelism, etc., and not the analysis of the single feelings, are the object of investigation, it is wiser not to insist upon one form of statement rather than another, but to put as much stress as possible upon accuracy of the statements made. Some introspections which cannot be wholly justified even by the above views have been included in order to keep the series complete.

* Cf. Santayana, "The Sense of Beauty."

original condition; also too idle; ought to be tending to her work. The other creature was making more noise this time, worse than before. (Two entirely separate.)

b-2 "Leksand old lady back to her pleasant expression again. Young woman improving. Feelings separate—transition."

c-1. "The frantic specimen has turned into an object of abnormal psychology. Heard the dog saying, "My dear woman, you're making a great mistake." Better than before, not unpleasant.

"Other old lady just a placid spectator of specimen; lacks initiative."

c-2 "Take on now a more special attitude. Old lady Swedish, girl German type. Both very pleasant."

d-1 "Leksand . . . had a dish-cloth about her neck; stripes in dress noticed. Doesn't vary in pleasantness from before.

"In other, dog gained in vivacity. Specimen just the same as before."

d-2 "Doesn't like either of them, Too immobile."

Subject "F"

Feb. 28, 1913.

a-1 "Good-natured, motherly woman seemed to be exposed much longer. Attracted to her; shrank from the other. Both faces grew in distinctness and clearness as trial went on. Appreciation increased too. Kindly feeling towards good-natured woman. She grew more so as trial went on, while the cross one grew crosser, so the two seemed to be farther apart at end.

"Feelings entirely distinct. Strain at transition, a sort of screwing up of face."

a-2 "Thought the old lady was clear before, but this time much clearer; clothing brilliant; figures stood out. Young lady very pretty. Two mutually supported each other. His social attitude towards them is a good deal the same. Did not associate them personally.

"No strain at transition.

"In (1) the old lady seemed to want to make the best of everything. In (2), she was just bubbling over with good nature; was really good in herself."

b-1 "Pictures farther apart. Strain is greater. Both clearer, but good-looking old lady more so. Details of her dress come out more. Feelings stronger this time. Good old lady seems to be wanting people to overlook the weakness of others, to make the best of things. O. wanted to ignore the bad picture. She seemed farther away this time."

b-2 "The old lady is more brilliant, vivid now, as if a spot-light were turned on her. Before she was more prominent than young lady, but now the emphasis of attention goes to the young lady

"Relaxation instead of strain as in (1).

"More pleasant than in a-2, a greater contentment."

c-1. " 'That old witch and I had it out that time.' She was the more prominent in consciousness. A kind of a cussed feeling about her. Made out the dog this time. First time he has seen him."

c-2. "Old woman a little tense and anxious in comparison with the calm composure of the young lady. Both pleasant, but there is this difference. One a little tense, other relaxation. Does not think of old lady as so benevolent now."

d-1 "Wise old dog has come to his rescue again. Old lady no better, but he doesn't take her so seriously. Longer time gives him a chance to grasp them more fully. Attitude seems much more complex than before. Other old lady still a pleasant relief, but the emphasis is upon the bad one. Still entirely distinct."

d-2 "The young woman is more beautiful. Set of head and the graceful lines come in more.

"Old lady looks more foxy. A little glint in her left eye. Her character hurt a little."

a-1 "Not the same as the first time, but the Leksand woman is a relief from the other; a strong personality; does not take her as so good as before. But the emphasis of attention goes to her again."

a-2 "More as pictures; not so real as at lower speed. (As speed decreased, he began to feel acquainted with them). Doesn't feel as if he knew them so well. Old lady still a little crafty."

Subject "H"

Mar. 4, 1913.

a-1 "Very nice. Trial about half through before she noticed the unpleasant one,—and it didn't come in other much. Seemed a very short exposure."

a-2 "Granny lost by comparison. Likes the girl better,—a darker picture. Both pleasant."

b-1 "Very nice. Unpleasant one not unbearable because it helps to make the other pleasant. Old lady very charming just as at first. Exposures seemed equal."

b-2 "About the same as a-2. Girl pleasant; old lady less so (than with other specimen).

"Difference in speed does not seem to make any difference in feeling."

c-1 "A little less pleasant than before. Unpleasant picture seems to occupy three-fourths of the time, and the unpleasantness seems to lessen the pleasure in the old lady."

c-2 "Doesn't respond to the old lady's smile now. Girl pleasant, cool and calm, slightly unresponsive, but she likes that."

d-1 "The next step in the same direction. Unpleasant exposure longer; takes away from the pleasantness of the other. Not *positively* unpleasant."

d-2 "A distinct pleasure each time the girl comes. Old lady much less pleasant. Once she thought the girl was exposed longer."

Subject "C"

Mar. 12, 1913. Sleepy.

a-1 "Hurt her eyes so much she had to stop."

a-2 "Too fast to get any feeling, but she did look for the whole 30 seconds."

"Luise self-satisfied looking."

b-1 "Just mildly bored. Hurts her eyes; feels cross-eyed at every shift."

b-2 "Sure the old lady hasn't any teeth, and the other lady looks flat; and she isn't a bit surprised by either of them."

c-1 "Rather a boring experience. Very slight feelings. Pleasant and unpleasant alternating."

c-2. "Pleasant one in the dark. Old lady still lacks teeth."

d-2. "Pictures in other positions. Both much better, old lady not so sharp; girl better.

"Trial seemed very short, not more than a quarter as long as the others."

d-1 "Pictures reversed. Caricature not real to her at all. Old lady just a normal old lady, mildly pleasant, not so much so as with girl. Not interesting, but her attention is on old lady, not on caricature."

Subject "J"

Mar. 6, 1913.

a-1 "First time he saw the witch, just recognition. Other old lady slightly pleasant. Next thought of Shakespeare's Macbeth, but said, the witch of Endor. Noticed her feet, then noticed old lady's glasses, didn't like them. Both real. Old lady (sewing) venerated for her character, etc. Then attention shifted from the pictures themselves to the effort in seeing."

a-2 "Both continuously pleasant. Practically fused. Parts of one situation. Old lady looking at the damsel. Strong organic reaction. A sort of anticipation of the shifting, no effort. Imagery carried through as well as feeling."

b-1 "That time those two pictures fused. Old witch had come to try and hypnotize old lady. A very amusing situation."

b-2 "Young lady very pleasant. Thought of her as a real leader, a genius. Two pictures separated now. Old lady just off to one side,—indifferent. When she came, was thought of in the situation of b-1."

c-1 "Discovered the dog this time. Couldn't make him out before. Amusing. Old lady knitting; didn't know that before. Experience indifferent except for dog. Didn't think of a single thing outside the pictures. Quite real."

c-2 "Rather a mixed-up affair. Both of the previous situations tried to get back, but did not succeed. Pictures taken separately. Associations came in. Young lady still slightly pleasant; old lady indifferent. Young lady not so well balanced as before. Thought of a story he had read. This girl taken as an American who had married a German."

d-1 "Everything except the dog absolutely indifferent. Same situation as before. Dog is an on-looker enjoying the situation. Empathy. Old lady is getting nothing done. Wants to see her do something."

d-2 "Brightly lighted old lady butts in. Adjustment rather painful.

"Young lady pleasant, still real. Not thought of as a picture.

"Old lady herself indifferent. Bright light unpleasant. No carrying over of pleasantness. Has to begin again each time. Absolutely no anticipation."

Subject "I"

Mar. 20, 1913.

a-1 "Distinctly grew in disagreeableness from beginning to end. The worse was so bad that the other hardly came in at all; so bad that it held over and was anticipated. Stronger reaction to it every time. Unreal, but horrible, like waking up from nightmare. Fascinated, yet repelled."

a-2 "Pretty girl preponderated, but the old lady was an annoyance. As if he were looking right at her without really seeing her, but not so much as he would have liked. No illusion about time. Feeling-tone ran in two layers. Girl distinctly pleasant, but there was an undercurrent of annoyance. Relative strength of impression about $2\frac{1}{2}$ to 1. Before about 5 to 1.

b-1. "6 or 7 to 1 this time. More reality about old woman; not a mere piece of pasteboard, more alive, an unfortunate creature. Doesn't think he saw the other more than twice. A little pity comes in this time. Can be taken as a fact, not a fancy now."

b-2 "3 to 1 this time. Noticed a little impatient catching of his breath when the unpleasant one was there. Noticed that he started focusing eyes for the pleasant one while other was still there. Once . . . seemed to hold too long. Due to his focusing too soon for the good one.

"Feelings parallel again all the time. Wavered in strength, but never broken. A sort of lemonade effect, the sour and the sweet."

c-1 "Getting angry with the woman. Dog came in more, silly, rather pleasant. Made him laugh. About 8 to 1 in strength. Unreal again towards end. No time illusions."

c-2. "Unpleasant one a little stronger again. Stronger desire than ever before to shut her out. Wanted to shut his eyes. Girl quite attractive; enjoyed her more and more to the very end. Afterwards his memory brings up the unpleasant one. Unpleasant one is associated with the other awful one. Fairly ground into him.

"Feelings more fused this time. Bitter-sweet with alternating emphasis. Never a complete absence of either. Almost equal."

Series IV was undertaken in order to test the effect of excitement and calm, for comparison with the results with pleasure and displeasure. This series also will be given in full.

In trials-1, the pictures are Riksdagshuset (a parade, with a large building in the background), and the court-yard of the Boston Public Library. In trials-2, a view of a portico at New England Gardens takes the place of the parade. The speeds are: (a) 36; (b) 29; (c) 24; (d) 23.

Subject "L"

Mar. 7, 1913.

a-1 "Too fast; couldn't get any ideas before it was gone . . . with each. One looked like B. P. L. Other rattled her. Unsettled, uneasy. Pictures indifferent."

a-2 "Both pleasant. New one much more so. Feeling of repose, quiet; helped by buzz of machine. B. P. L. a little too stiff. Nearer apperception time in this trial."

b-1 "Parade one associated with inauguration; very attractive, sunlight. Other moldy in comparison. Parade seemed German; pleasant. Just shut her senses to B. P. L. B. P. L. shorter exposures than other.

"Feels much more comfortable; breathing slower and easier."

b-2 "Still too fast for comfort. Worse than b-1. Feels choked. Pictures pleasant. Garden one more so."

c-1 "Very much more comfortable.

"Emphasis is on the parade; French or German. Other just fills in time; not positively pleasant."

c-2 "Garden gaining in pleasantness; other losing."

d-1 "Got into rhythm of motor. Took away a little of her feeling for pictures.

"Gaining interest in library again, i.e. relatively. Other lost."

d-2 "Heard water splash. Added interest. Both gaining a little in pleasantness on that account.

"Transition is more easy in (2) than (1)

Subject "F"

Mar 7, 1913.

a-1 "Incongruity between the two pictures; and fellows marching spoil the picture. Uniforms look black. At this speed they hardly look like human beings. Irregular, jerky, in contrast to lines of the buildings. (Procession came first, then "mansion.") Wanted to get rid of band."

a-2. "So beautiful he can't describe it. Affected his breathing. Felt as if he wanted to say something, but didn't know what to say. The two mutually strengthened each other. (B. P. L.) a little dim. Wanted it to be as well lighted as the other. Both beautiful."

b-1. "Other speed was too fast. This helps the parade. The men seemed to be really marching this time. The beautiful picture was not so dim. Both improved."

b-2. "Seems to understand the (B. P. L.) picture; seemed to be an inner court; seemed to be the Jefferson Hotel in Richmond. Pleasanter because he appreciated it better. Other not surprising of course, but beautiful. Had more opportunity to grasp details."

c-1. "Couldn't see much change. More empathy for the marching.

"(B. P. L. is a different building; a government building of some sort.)" (Association).

c-2. "Both hotels this time. Gets more details now. Noticed the pottery this time and the pond of lilies, fountain in inner court, etc. Both pleasant. Just as beautiful as before."

d-1. Didn't enjoy the soldiers marching so much. Other, if anything, more beautiful. Noticed lines of columns, etc. No response to rhythm.

"Both government buildings."

d-2. "Both again in detail and in appreciation. Both hotels. Enjoyed more than before. Two are more on a level. (B. P. L.) not so dim, but imposing. It will wear well, not get tiresome.

"Transition in (1) very abrupt; in (2) hardly noticeable."

Subject "J"

Mar. 13, 1913.

a-1. "First mood called up—Berlin—wouldn't quite fit in. Other B. P. L. But two were taken together. Grandeur of archi-

ecture in B. P. L. harmonizing with order of soldiers. Speed a little too great for satisfaction."

a-2. "Mood of same quality as before, but more pleasant. Both fused into one whole, just passing from one part to another. Satisfied pleasure. B. P. L. a little barren in contrast with other. Changing didn't seem so rapid as in a-1; could catch details more clearly."

b-1. "Trying to get details, but each time the other comes flip. Dissatisfaction with each picture. Got no feeling tone for either picture itself. Experience as a whole . . . effort to get details the prominent thing."

b-2. Mildly pleasant both of them; courts of the same building or something of that sort. A slight preference for the more complex one."

c-1. "A German scene and B. P. L. Mildly pleasant . . . inactive. Scenes real. Separate now. Can't recognize the building, but takes it as in Berlin. Speed more satisfactory; going slower. Pleasure aesthetic."

c-2. "Mildly pleasant, both of them. Not the library now. Two are parts of same palace. Thought how nice it would be to walk along by the water. Pleasure erratic. Organic reactions. Remembers looking at flowers, grass, water, etc. Bust started associations, one of the museums in Paris."

d-1. "B. P. L. again. Mood of public building. Mild admiration for architecture. He is a spectator. No immediate connection, i.e. local. They are both public places. No contrast in mood; no associations."

d-2. "Four distinct moods, perhaps 5. 1st library; wouldn't work. 2, a palace kept up for show. 3, Palace one in which old-time kings live. 4, Modern palace, modern kings living there. 5, His own house. Mild erotic feeling; increased towards end. Two pictures fused throughout."

Subject "H"

Mar. 18, 1913.

a-1. "Very pleasant . . . watch in Berlin? . . . enjoyed it very much. Other pleasant too. Reminded her of the B. P. L., but not sure. (Asked me; I told her.) Liked speed; seemed to be doing things."

a-2. "Too fast for the new picture; couldn't get anything out

of it. Library still dignified and cool, but the two little awnings were very bad."

b-1. "Speed unpleasant; made her dizzy. The marching. . . . They were getting too close. Library all right. But men are more real than before. Going too fast; wanted to look at them longer."

b-2. "Doesn't like the shifting. Feels familiar with the library, . . . but wants to see the other, can't wait. Library seems to be exposed ever so much the longer."

c-1 "Soldiers going to the band concert in the square, very pleasant. Didn't see the library at all; actually didn't know it was there."

c-2. "Didn't see the library this time either, although she had intended to look at it. (Gardens) very pleasant now."

d-1. "Noticed the jars in the library picture. Otherwise it was all the "Berlin" picture. Very pleasant associations. They come in more now,—people that she knew, etc."

d-2. "Library much the longer time. Noticed a lot of details. Mildly pleasant, but not interesting. Wanted the other. Other not there long enough."

Subject "C"

Mar. 19, 1913.

a-1. "Too speedy. Doesn't like it all. Hurts her eyes. Knows what the pictures are, but that's all."

a-2. "Very pretty pictures. Liked to look at them, but towards end, eyes got cross-eyed and hurt again."

b-1. "Liked the garden one, but the people came in too much of a hurry. Contrast favors (B. P. L.)."

b-2. "Very pleasant; go well together, but (B. P. L.) is lots better, dignified. Other ostentatious."

c-1. "Likes the nice, calm, placid one. Other distresses her. Doesn't like to see them hurrying. (Empathy.)"

c-2. "Thinks she is turning socialist. Wants to smash the statue in (Gardens). Much more concerned with the pleasant. Both feelings alternated. $\begin{smallmatrix} + & > & - \end{smallmatrix}$ "

d-1. "Heard the band; wanted to be there. Other just losing. Lots of things finer than just satisfying one's senses. Wanted to be doing something."

d-2. Didn't fit in with her aroused mood at all. Wanted to upset something.—Both unpleasant. (Gardens) the worse."

Subject "K"

Mar 19, 1913.

a-1. "Both were pleasant of course, but the palace made him feel big. In the other he was just one of the crowd. In the palace he was intimate with the people of the place. Attitudes alternated."

a-2. "Wasn't that difference. "Palace" got more commonplace. B. P. L. recognized. Other the approach to a very magnificent house. He was a spectator admiring, 'inquisitive,' more interest in it. Aesthetically admired the pillars in B. P. L. more. Association hurt B. P. L. He thinks of it as just an ordinary building."

b-1. "This time interest was more for the large building in the background. Other too isolated and simple a place.

"The big building seemed far away. A good many rooms; worth seeing.

"Still admired the pillars and the magnificence of the square, but it was too simple.

"No interest in soldiers."

b-2. "More curiosity now for the house. Pleasant. Other not so much as before; too isolated. Might have been a jail. House in the open, considered the surroundings."

c-1. "Liked the picture with the soldiers much better than the other; more life in it. Other got monotonous.

"Soldier picture better than before."

c-2. "No difference from b-2. Liked the approach to the house better."

d-1. Soldiers seem more pleasant than before. Enjoyed the march of the soldiers . . . and the music; and wanted to be in the big building.

"Other more monotonous, commonplace."

d-2. "This time thought of the house as the approach to the courtyard. (I told him to pay attention to the transition). Not so pleasant as the picture on the street (soldiers).

"Transition not difficult in either case. Contrast helps his feelings."

Subject "I"

April 5, 1913.

a-1. "Feeling-tone . . . as long as he remained passive, the two reinforced each other steadily. When he tried to grasp details of the street scene it was rather painful, detracted from the feeling-tone. When he just took it easy and let them come both were very pleasant, and steadily reinforced each other. Feeling for the two much the same. Same bodily processes. And the different aspects of the soldier picture—the feeling for the glamour of it persisted during exposure of other. Reverse not true. (Speed difficult.)"

a-2. "The familiar one is more pleasant. Speed too great for the new one. Picture itself pleasant, but its disappearance so soon very harrowing. Felt eager to get more details. Over-effort to accommodate the eyes; haste, a sort of diffusion of energy generally. Tension in the back of neck, etc. An effort to prolong the image seems to proceed from inside the head. Strange tension.

"Relaxation was added to the enjoyment of the other picture. Yet there was a desire to get back to the other and solve it."

b-1. "A tendency to concentrate attention on soldiers. Other almost passed out of sight in several cases. Very marked increase in the feeling-tone for soldier picture. Could understand it much better. Doesn't have to begin again each time. Pleasant, feeling varied, mingled with curiosity."

b-2. "Much the same change in appreciation of (Gardens), but it didn't interfere with the appreciation of the palace (B. P. L.) Transition almost instantaneous and practically unnoticeable. Whole process of apprehension and enjoyment practically the same in the two. House part of (Gardens); became quite familiar."

c-1. "Associations begin to come in. He "recognizes" the pictures. Could go from one picture to the other easily this time. Palais Royal and Pont-Alexandre, facing Louvre. More interested in architecture than in the soldiers."

c-2. "Nothing to be added to b-2. Vines noticed this time. Feeling identical throughout."

Series V dealt with the relations of humor and aesthetic and religious sentiment. In trials-1, Katarina Kyrka in Stockholm was used with a humorous picture of two dogs and a bottle

of champagne. In trials-2, the church is replaced by another dog picture, two pups tumbling out of the basket of a balloon as it strikes earth. In trials-3, Stephans-kapellet is used with the other church. The speeds were (a) 36; (b) 24; and with one subject, (c) 12. The results were very much the same for all subjects. Those of "F" will be taken as an illustration. Each trial lasts 30 seconds.

Subject "F"

Mar. 21, 1913.

a-1. "Couldn't get much out of it. One humorous, other beautiful. So disparate that they did not seem to influence. Lines of "school building" pleasing; dogs humorous. Pleasant all through. A slight break at transition. Didn't belong together at all."

a-2. (Was laughing to himself all the time.)

"Reinforced each other. Not in the same place. Dogs falling out of the basket are enjoying it, but he couldn't make out the basket, who was pulling the strings. Whole thing frolicsome. Transition much less marked; hardly noticed."

a-3. ("Oh, that's fine"; spoken at beginning.)

"That's beautiful. Big brick church magnificent. Not just sure what the other is, school, court-house, or house. Supported each other, just as the two dog pictures did. Grandeur of architecture, especially in church; lines of other not so clear because of the trees, etc.

"Transition very slight."

b-1. "More incongruous. Transition at first startling. Couldn't take the dogs as humorous as before. Beauty of building didn't strike him as it did before. Dog situation is rather trifling. Dog not as funny as he wants to be."

b-2. (Laughing again.) "They are funny; you can't help reacting that way. Discovers now that there was a good deal of tension in b-1. Relieved now. Both pictures funny."

b-3. About the same as before. Of course some of the newness is gone, but the same relationship. They mutually support each other.

"Immensity of space prominent; all out-doors. He notices difference in his breathing.

("Dogs rather cooped up—in a yard, or something of the sort.")

In Series VI, I selected two winter scenes and two summer, and used first one of each, then the two winter scenes and the two summer scenes. This was repeated at speeds 36, 29, and 24. I wished to study the interference of the complex attitudes towards the two kinds of scenery. With all the subjects except "I", there was an interference,—'discord,' 'loss of bearings,' 'a slight conflict,' 'vehement,' 'senseless,' 'hard to go from one to the other,' etc.

I believe the above will be a sufficient indication of the methods and results in this section. The important points seem to be these: The frequent apperceptive synthesis depends upon the objective similarity of the pictures, and is of only minor importance for us here. The tendency to increase 'by contrast' comes when the general field of appeal is the same. With complex presentations there is a marked tendency to selection or assimilation, working towards identity or direct opposition. Mixed or parallel feelings seem to occur when the two separate attitudes differ slightly in meaning or field of appeal. They may be either of the same, or of opposite, value. They occur (with the materials used) only with certain subjects, and seem to depend upon the scope of attention, though only roughly so. They occur only when the feelings are well-marked. The parallelism is often one-sided, so to speak. A strong feeling may persist without inhibiting the weak feeling, while the latter is, as a rule, broken. The most frequent results are contrast or inhibition.

The effects of the different speeds need no discussion at this point. They come out more clearly in the use of the method of continuous variation. Beginning with Series VII the work of this section was mostly at high speed, beginning with 72 and working downwards. As still higher speeds were used in the later sections, there is no harm in omitting the remainder of the present one.

We now come to the experiments with variation of speed within the single trial. The apparatus was rather crude, so that the results are valuable chiefly in a qualitative way, though they give some indication of the possibilities of quantitative work.¹⁹

¹⁹ The disk was revolved, during this season, by means of a direct-current motor, controlled by a rheostat.

After a few preliminary trials with speed constant, the work falls into the following divisions: *b 1*, trials starting at about 25 pairs of exposures per minute and gradually increasing to about 50. In this section no special directions were given as to the attitude to be assumed towards the experience. The task was the same as in A. 4, with the greater emphasis upon the tracing of the course of the feelings; *b 2*, consists of trials in which the subjects were asked either to try to get the feelings or to be passive; *b. 3*, the concluding section, consists of two series of trials with distraction, in the form of mental arithmetic. These trials were expected to throw some light on that part of our hypothesis (conclusion of A. 3) which refers to the scope of attention. In the last two sections, high speeds were used.

The materials were those of the first season. All the subjects except "K" were new, and I wished to extend the results with the same pictures.

Selections from the results of *b.1* and *b.2*, and the two series of *b.3* will now be given.

b. 1. Method as stated above, trials lasting about 45 seconds.

Subject "A"

Oct. 8, 1913.

Trial 1. Riksdagshuset & Courtyard (B. P. L.)

"Picture with columns very pleasant. Other about indifferent. Suggestion of militarism once or twice, somewhat disagreeable, but pleasure did not disappear even then. Was occupied almost altogether with the column one.

"A constant background of pleasure, at times tinged with unpleasantness."

Trial 2. N. E. Gardens (portico) & B. P. L.

"My! That's great. Both quite pleasant. The new one is the better of the two. As the speed increased, tended to dwell on the new one, even when the other came, and the other became less pleasant than before.

"The feelings differed, but both pleasant. Cannot describe the difference.

"At first, the two feelings alternated, distinct. Then the feeling for the new one became a constant background"

Trial 3. Rear View Paul Revere House, & Arbor (N. E. Gardens.)

"Arbor—happy, quiet.

"House—poverty,—squalid.

"As trial went on, attention was monopolized by the arbor. At first feelings alternated, then the happiness became constant, and was momentarily tinged with sadness,—a sort of sad happiness. The happiness during exposure of arbor was unalloyed."

Trial 4. Road to Sleepy Hollow & Arbor.

"There is a difference in the feelings; the old one secluded; nice place to walk with a girl; other open. Marked change in the attitudes at transition; alternated distinct until very last, when tended to dwell a little on the old one, but there was no holding over of one feeling into other this time. + all the time, in varying degrees, i.e. the things themselves.

"A little unpleasantness came in at transition, due to the change. Wanted to dwell on the arbor."

Oct. 22, 1913.

Trial 1. Kitten on pedestal & I am for Prohibition.

"The wet cat seemed funny. This feeling waned.

"Other pleasant, too, but different. Wanted to pat it.

"The feelings alternated. Then humor waned, and the other feeling tended to hold over. The feelings overlapped."

Trial 4. Elsie Ferguson & Ancient.

"One attractive; other horrid, ugly, didn't want to see it at all, but couldn't shut it out.

"Feelings alternated at first, then towards end the pleasure seemed to be combined with the repulsion.

"There was some notion of assimilating the two in perception, which made the feeling all the more horrible.

"He has no doubt whatever about the reality of this "awful loveliness."

Trial 5. Elsie Ferguson & Audrey Burton.

"Both pleasant; at first not much different, but later whenever the girl with the hat came, he felt like saying, Oh you! This feeling more sexual, other dignified. These feelings alternated all through. No blendings.

"No tendency to assimilate the two."

Subject "B"

Oct. 7, 1913.

Trial 1. Paul Revere House & Road to Sleepy Hollow.

"At first old house was unpleasant, but got adapted to it.

Then when the speed increased, the effort at adaptation was unpleasant, chiefly due to eye sensations. No longer any time to enjoy trees. The feeling for the pictures themselves disappeared."

Trial 2. N. E. Gardens (Arbor) & Road to Sleepy Hollow.

"Noticed a tendency to breathe in rhythm with the two. Didn't like the high speed. Couldn't enjoy either or get olfactory imagery.

"2 much the more pleasant trial.

"In 1 pleasure added to trees, due to contrast. In 2 trees not so distinctly pleasant as before, because the apple blossoms were better."

Oct. 14, 1913.

Trial 1. Luise von Preussen & Leksand.

"Very pleasant until about halfway through; then (as speed increased) the pleasure diminished. Liked the girl a little better."

Trial 2. Leksand & Old woman of Gloucester.

"This time liked the high speed better, except the very highest.

"The old lady (caricature) was very disagreeable. Good one a decided relief.

"High speed cut out unpleasant details.

"Feelings entirely distinct; simple alternation.

"Leksand better in second trial than in first."

Oct. 28, 1913.

Trial 2. Madame Recamier & Dude with Bundles.

"At first increase of speed increasingly pleasant, then faded again.

"Female fairly pleasant, but her dress was unsuitable. A little revulsion; this held over sometimes and was then with the humor. No holding over the other way."

Trial 3. Madame Recamier & La Cruche Cassée.

"At first pleasant, but as soon as he noticed details of dress, whole thing was unpleasant.

"The former one not as bad as the new. The old was a little good by contrast. The worse feeling held over into the exposure a little."

Trial 4. Two old Salts & La Cruche Cassée.

"The pleasant feeling held over all the time. Occasionally

there was a slight ill-defined unpleasantness *along with the pleasure*, during exposure of girl. Once there was a break in the pleasure."

Trial 5. My Chauffeur & Remembered by St. Valentine.

"Whole experience pleasant; less so towards the end. Old lady more pleasant, humorous. The humor was held over a good deal, sometimes through entire exposure of girl; at others crowded out by a quieter feeling.

"Sometimes the girl seemed to be smiling, but he doesn't think she really was. Seemed to be a carrying over of his own *Einfühlung*."

Nov. 4, 1913.

Trial 1. Mona Lisa & Old woman of Gloucester.

"The rate seemed to be pretty much indifferent; a little too fast, but increase didn't seem to make any difference. Mona Lisa slightly pleasant; other only slightly unpleasant.

"The pleasantness of Mona Lisa held over an instant, appearing localized near the centre of the field; the unpleasantness from the wench seemed to 'wobble' in from the lower left corner, and when partly in, the pleasantness from the other 'exploded' and the unpleasantness spread over the whole picture."

Trial 2. Österåker & La Vergine Afflita.

"The higher speed was slightly unpleasant.

"The nun slightly sad and very slightly unpleasant; feeling of depression; the Dutch girl rather pleasant. In passing from the girl to the nun there was abrupt change of feeling. The pleasant coy feeling became suddenly sad, distinct empathy. The slightly unpleasant feeling of the nun held over nearly every time (during slower rate) into the other picture, and the pleasantness of the Dutch girl seemed to come from underneath and push up through, and shove the sad feeling aside."

"Trial 3. Hallowe'en Greeting & El Divino Pastor.

"Faster rate got unpleasant, as before. Not time enough to see them.

"Jack o' Lantern pleasant, quite pleasant. Other less pleasant; looked sad.

"Alternated distinctly; two degrees of pleasantness.

"Suggestion of the lesser degree of pleasantness of the shepherd holding into the higher degree of the jack o' lantern, which immediately crowded it out. No tendency in the opposite direction I'm sure."

Subject "N"

Oct. 31, 1913.

Trial 1. Mona Lisa & Old woman of Gloucester.

"Slight unpleasantness in both.

"Doesn't like the smirk in Mona Lisa. The other counter-acted that and had a slight pleasantness on that account. On the other hand, the coloring in Mona Lisa is pleasant, and the other bad by contrast. Mona Lisa was always the main thing, and the other a foil for it, either good or bad. Could not say just how the feelings behaved."

Trial 2. Repetition.

"Once with Mona Lisa there was a mixed feeling, unpleasantness dominant, but with a slight pleasantness due to the colors. Mona Lisa became on the whole more unpleasant and the other more pleasant. Feelings more definite this time.

"Complete alternation. As speed increased, the feelings now and then sank to indifference."

Trial 3. Duchess of Devonshire & A servant girl down in North Bingham.

"At beginning, there was mixed feeling in each. Each was partly pleasant and partly unpleasant. Then for a while the painting was pleasant and the other unpleasant. Then as speed increased there was a transition period after which the feeling was reversed, painting unpleasant and other pleasant. In transition period whole experience was unpleasant. As speed increased, painting got simperish and sickish; other a solid, workaday thing then. At very end, everything became indifferent. Complete alternation."

Trial 4. No. 7 & What are you Laughing At.

"At first it was awful. Man sitting on bench first. Mixed pleasantness and unpleasantness. When the other came it was a great shock; result a muddled state of pleasantness and unpleasantness. The whole soon became indifferent."

Trial 5. Elsie Ferguson & Ancient.

"Pretty girl first. Change to other an awful shock.

"Each had its own feeling tone; each tended to hold over a little while the other was developing; so at transition there was a slight mixture. Towards the end the rapid change seemed to color the whole experience with unpleasant, yet at the same time the girl was pleasant and the other unpleasant. The spots of pleasantness were shorter than those of unpleasantness."

Trial 6. Elsie Ferguson & Mrs. Aaron Ward Roses.

"The first change from girl to roses was luscious, but later the same unpleasantness at transition came in, i.e. the same sort of thing, but not so strong. Both were pleasant. The unpleasantness partly overlaps the other feelings,—not continuous as before."

Subject "K"

Oct. 16, 1913.

Trial 1. Vigée Lebrun & Daughter, & Snakes.

"Snakes first; no feeling at first, just studied it.

"Then a great gush of pleasantness, when the woman and girl appeared; seemed nearer, more alive.

"A tendency to ignore the unpleasant one. This tendency increased with the increase of speed. Unpleasantness was diminished, did not last as long as pleasure. The two feelings seemed separate in their actuality, but there was a continuous residue of knowledge that the pleasant one existed."

Oct. 23, 1913.

Trial 1. Winter sports, and dark interval.

"Very pleasant."

Trial 2. Winter Sports & God Help the Poor Sailors.

"Very good contrast, both slipping, etc. Girls pleasant, and man pitiful. Tried to ignore him.

"Winter sports a little more pleasant the first time than this.

"Not a direct contrast, but one between sympathy and aesthetic enjoyment.

"Pure alternation *throughout*."

Oct. 30, 1913.

Trial 1. Courtyard, B. P. L. & Oh Gee!

Not very much feeling.

"(B. P. L.) pleasant, other somewhat unpleasant, but there wasn't very much about either."

Trial 2. Luise von Preussen & Oh Gee!

"A good deal of feeling. The new one seemed to be nearer, more alive; pleasant. Man slightly unpleasant, about the same as before. The whole mood seemed to be pleasant. He was more charitable towards man. The man seemed less significant. The unpleasant feeling seemed merely to push the pleasure a

little aside, so to speak. The pleasure was continuous, as a background. The displeasure was only superficial."

Trial 3. Mona Lisa & Old woman of Gloucester.

"Pretty great contrast; a tendency to bring the pleasant one closer.

"Complete alternation throughout. + feeling far stronger.

"They seemed to be in two different worlds. The contrast was so sharp that they could hardly be compared.

"Seemed to him that Mona could not know of the existence of the old hag.

"The two seemed opposite in every way; one attractive, other repulsive; one seemed close, other distant.

"Feelings weakened as speed increased."

Subject "D"

Oct. 27, 1913.

Trial 1. Girl learning cello & 38.

With girl, he wanted to draw the bow across (Einfühlung).

"Other gave him the feeling of toothache; rather unpleasant. The unpleasant feeling seemed to last longer, to be carried over into exposure of other picture.

"*No mixed feeling.*

"When the speed increased, there was no feeling whatever."

Trial 2. My Chauffeur & Remembered by St. Valentine.

"The young dame was very pleasant. Other at first mildly entertaining, later on got monotonous; then conflicted with the other. The thought came that ever the 'belle dame' might become the other one. That was unpleasant. The feelings were quite distinct.

"The shifting of the light is unpleasant. This persists even during exposure of pleasant card. The displeasure of anticipation of removal is present simultaneously with pleasure in the picture."

Trial 3. N (young woman in evening dress sitting on the back of a chair, & Ks (children playing).

"A great deal of curiosity to find out what the girl was doing. She became very unpleasant. The other mildly pleasant at first, became more pleasant as other grew unpleasant. The unpleasantness seemed to be stronger, seemed to survive even when the other card came."

Trial 4. M (young lady) and Ks.

"Children still pleasant, but not so much so as before.

"Didn't like girl. The feeling held over into exposure of children.

"Once there was a trace of parallelism."

Subject "E"

Oct. 15, 1913.

Trial 6. Hazel Troutman & Ancient.

"Contrast greater as speed increased, but not very much so. Actual reaction to unpleasant picture fades out into difficulty of seeing it.

"The feelings delay each other. Can feel one crowding out the other. They do not seem to actually overlap. Hard to describe."

Oct. 22, 1913.

Trial 1. Kitten & I am for Prohibition.

"The little one was agreeable; other unpleasant.

"Contrast in both sympathy and touch imagery.

"Simple alternation throughout."

Trial 2. We Used to be Pals, & I am for Prohibition.

"Dog comic. Feeling was carried over a bit. Interpretation of cat was altered. A little more inclined to smile at cat."

Trial 4. The Garden Wall & Among the Poppies.

"The one on the wall was more pleasant, easier to see, but both pleasant,—and a little discomfort at the difficulty was carried over into the exposure of the better one, but not referred to it. Seemed to interfere with the perception of the better one.

"Pictures similar in feeling, but a slight break between."

Trial 5. The Garden Wall & Oh Gee! The World belongs to me.

"Interesting. The prominence of the man's feet called her attention to the undue prominence of the child's legs and feet.

"The pleasure in the subject (children) as great as before, but alternated with displeasure at the poor drawing.

"Picture of children was refreshing by contrast with the other. The other suggested the cynical attitude. No redeeming features about it.

"The feelings alternated throughout, with the exception of the influence on interpretation spoken of above."

Nov. 5, 1913.

Trial 1. Österäker & La Vergine Afflita.

"Interesting; posed so much alike that it was very easy. Both

pleasing. Peasant girl more natural; other—massing of color was good.

"At low speed peasant girl more pleasant; at high speed, other.

"The two seemed to help each other. The Madonna seemed lacking in expression, owing to contrast with other."

Trial 2. 98 & La Vergine Afflita.

"Madonna looked quite natural; other amusing at first, but before he disappeared he was quite painful. Madonna still aesthetically better, too.

"Distorted figure produces a sort of tension, empathy. Could feel her own face smoothing out when she looked at the Madonna. Even the pose of the hands seemed natural this time. Seems as if it were a better copy."

Nov. 5, 1913.

Trial 3. Österåker & Lily Pool.

"Girl looked rather artificial in comparison with children. At high speed the girl was more pleasant, however.

"Effort to see details in picture of children a little unpleasant. In seeing this picture the unpleasantness is there with the pleasure.

"No blending of the feelings for the separate pictures."

Trial 4. Mona Lisa & Old woman of Gloucester.

"Old hag very unpleasant. Mona Lisa pleasant by contrast. The first time she ever saw her to like her.

"As trial went on, details came out.

"There was a marked contrast between the repose and the distortion. Both vague empathy.

"—increased.

"—diminished; became more an intellectual distaste.

"The repose became the dominant.

"The pleasure crowded out the displeasure. No blending."

Subject "G"

Oct. 14, 1913.

Trial 1. Luise von Preussen & Leksand.

"Chief thing was annoyance at the flicker.

"Old lady was rather amusing. Girl indifferent."

Trial 2. Leksand & Old woman of Gloucester.

"Both unpleasant; increasingly so.

" . . . Leksand seemed different this time. Instead of **amusing**, her expression was unpleasant. Bad features were **brought out** by similarity with other."

Trial 3. God Help the Poor Sailors, & Dude with Bundles.
 "More or less amusing. Increase of speed seemed to make no difference."

Trial 4. In the Garden & Among the Poppies.
 "At first almost indifferent, peaceful. As speed increased, a little unpleasant."

Trial 5. God Help the Poor Sailors, & Among the Poppies.
 "One amusing, the other pleasant, increased as speed increased, even to the highest speed.
 "Contrast made the children seem sort of cute."

The individual difference in the tendency to parallel feelings should be noted for comparison with the later results. The tendency is quite strong with "A" and "B," somewhat less so with "N," still less with "K" and "D," and absent with "E" and "G."

b.2. In this section the method of taking the introspection was somewhat altered. The subjects were asked to note the points at which changes in the nature of the experience occurred. As a general rule, this was done simply by the word *now*, spoken at the time of the change, when the speed was recorded by the experimenter. Occasionally a very brief statement was made. Immediately after the exposure, the introspection corresponding to the previous signals was taken down.²⁰

At first each trial started with the lowest speed, increased to the highest, and then decreased again. The section began with a few experiments in which the task was the following:—
 (a) to note the best rate; (b) to find the point where the feelings are gone; (c) to find the point where the feelings begin to go, and describe the effect.

A trial with Subject "D" will serve to illustrate the method.

Nov. 10, 1913.

Trial 1. Good Bye, & God Help the Poor Sailors.

- (a) "Strongest at lowest speed". (43)
- (b) "All gone at 57."

* The points of the rheostat were numbered, and these numbers were recorded at the signal word. At the close of the hour, the number of exposures per minute for each point recorded was taken. As the apparatus was generally pretty constant during any single hour, this method was fairly satisfactory for such a preliminary study of the method.

(c) "At 54 on way up, 53 on way back. Feeling at this point compared to other was like pointing a cap pistol at a man compared with a revolver; reality gone. Dog picture sad, other ludicrous, but not enough to make him laugh."

It became evident that the results were much influenced by the attitude taken, passive or active. A few trials were made to compare the results in the two cases. In general the active attitude, the attempt to get the feelings, was successful. The feelings came at much higher speeds than when no effort was made. With "E" the reverse is true; e.g. on December 3d, in trial 2, passive, the feeling disappeared at 93; in trial 3, active, same materials, it disappeared at 74. In another pair of trials, with new materials, similarly, in trial 3, active, the feeling 'became rather intellectual, picture recognized as the sort of thing that gives pleasure', at 70; in trial 4, passive, same materials, this effect did not appear until 83. With "A", there is little if any difference in the two cases.

The greater part of the work was done with the passive attitude. The results bring out very clearly the influence of the change of speed upon the feelings,—the gradual sifting out of portions of a complex reaction and the variations in dominance of one feeling or the other. The individual differences in the time required for development of feeling are very marked.

The following will serve as illustrations:

Subject "B" (eyes tired)

Nov. 18, 1913.

The first two trials run from speed 25 to 45 and return, the next two from 42 to 87 and return. (Method, passive).

Trial 1. Rear View, Paul Revere House & New England Gardens, Arbor.

'At first and last apple blossoms quite pleasant, house slightly unpleasant. At 41 on way up house getting more unpleasant, apple blossoms less pleasant. At 41 on way down two about equal and opposite. At 28, apple blossoms getting pleasant again, at 26, house unpleasant again.'

Trial 2. Georgetown Road & N. E. Gardens.

"Best at lowest speed. At 29 on way up woods the more pleasant. At 29 on way down equal. At 38 on way down boat had

weakened a little. At 34, both got stronger. At 29, woods again the more pleasant.

"A little more empathy in the woods. Noticed that sometimes he inhaled when he looked at woods, just as when looking at flowers."

Trial 3 (pictures of trial 1).

"At first unpleasant stronger than pleasant. This increased at 49. At 56, about equal. At 74, unpleasant disappeared; at 87, pleasant. On way down, pleasant came in again at 70; unpleasant at 56. At 48, unpleasant stronger than pleasant. At end tending towards equality."

Trial 4 (pictures of trial 2).

"At 44, road more pleasant. At 49, two equal. At 74, flowers gone. At 87, all gone. On way down, at 70, road slightly pleasant; at 57, blossoms slightly pleasant. At 49, two equal. At 45, road more pleasant.

"Different kinds of pleasantness, Road, a sort of outgoing feeling, open, etc. Tendency to extend himself. Other, olfactory imagery pleasant. The road attitude held over a little into other as rate became slower again."

Comparison of the results of the increasing part of the trial with the decreasing serves as a check upon the effect of familiarity, fatigue, etc. When the results are fairly symmetrical, they may be taken as indicating the effect of the rate. When this is not the case, the results are of doubtful value. The constant method, especially as in a.1, could also be used as a check.

Concerning the effect of high speed, "N" says (Dec. 5, Trial 2) alternation 'so fast that the complete feelings do not come, so conflict is gone, and the experience is pleasant.' He does not 'sink himself into the two alternately as before.' "G" (Dec. 2) speaks repeatedly of a 'ghost-like effect', 'unreal', 'not time for the meaning to develop, no context.'

Other similar phrases may be found scattered all through the work of this year, and in A.4. Unreality seems to result from (a) lack of motor response, *Einfühlung*; (b) lack of associations. I believe that such results with normal persons may throw some light on the problem of the abnormal experience of unreality, as discussed by Packard.²¹

²¹ The Feeling of Unreality, *Journal of Abnormal Psychology*, V. 1, pp. 69-82.

The latter part of the work of this section began at the highest speed and went to the lowest. It was intended to bring out the individual differences in quickness of response. It will not be necessary to quote any of the introspection. Suffice it to say that there is very great variation, some getting feeling quite often above 100 (single exposures less than $3/10$ second) while others required much longer exposures. More adequate apparatus must be devised before accurate results can be obtained. The subjects, arranged in the order of quickness of response, from the slowest up, are as follows:—"D," "E," "K," "N" and "G," "A" and "B."

These results, so far as they go, indicate a positive correlation between quickness of response and the tendency to parallel or mixed feelings.

b.3. This final section, as mentioned before, consisted of two series of trials with distraction. This took the form of mental arithmetic. The results throw some light on the relation of the tendency to mixed feelings and the scope of attention. They are not very satisfactory, however, owing to the fact that the adding soon gets mechanized, and also that some of the subjects add visually, so that interference of an unwelcome sort came in. Still, though two series are hardly a sufficient basis for generalization, the results seem to indicate that distraction has less inhibitory effect with those subjects who have the stronger tendency to mixed feelings.

The method was as follows:—There were four trials in each series. In series 1, the pictures were No. 38 ("comic") and Bridge in Franklin Park. The trials began at medium speed, and the speed was increased to the highest. In trial 1, the subjects were directed to be passive, to make no effort to get the feelings. They were asked to begin with 13, and add 17 repeatedly as long as there was any feeling, or in case there was no feeling at the start, to continue adding until the light was turned off. In trial 2, the method was passive, without distraction. Trial 3 was active, with distraction, to begin with 9, and add 17 repeatedly, and try to get the feelings. Trial

4 was active, without distraction. The point at which the adding was stopped indicating complete inhibition of the feelings, was recorded. In the trials without distraction, the introspection was taken as in the previous section.

"D" and "E" had no feeling in either trial 1 or trial 3. "G", in trial 3, 'succeeded in getting a feeling of pleasantness from bridge once about halfway up'. Apperception also is inhibited; there is only 'a sort of recognition of the presence of certain colors,' etc. "K"'s and "N"'s results are quite similar. In trial 1, they both tended to attend either to the adding or to the pictures, and got feelings when they attended to the pictures. In trial 3, both had considerable feeling, though less than in trials 2 and 4. "K" had parallel feelings at high speed in trial 2. "A" and "B" had feelings in all four trials, less in the trials with distraction. In trial 1, "A" had feeling only for the dog, 'a mixed feeling, humorous, but not very aesthetic.' "B" found 38 indifferent, the landscape pleasant. In trial 3, both "A" and "B" had feeling for both pictures, in complete alternation. "A" had mixed feeling in trials 2 and 4; in 2, humor and pleasure; in 4, pleasure and displeasure.

In series 2, the pictures were the Arbor (N. E. Gardens) and Paul Revere House. The method of the trials was in reverse order; 1, active; 2, active with distraction, begin with 19 and add 13 repeatedly; 3, passive; 4, passive with distraction, begin with 7 and add 13 repeatedly.

The subjects fall into the following order, when arranged according to the intensity of feeling in the distraction trials:—"D," "E," "G," "K," "A," "N," "B".

The results of these two series point towards a positive correlation of scope of attention with the tendency to mixed feelings, when compared with the results of b.1. Combining the results of the two series, the subjects rank as follows:—"D," "E," "G," "K," "N," "A," "B."

The ranks in b.2 were: "D," "E," "K," "N" and "G." "A" and "B."

In B.1 they were "E" and "G," "D," "K," "A," and "B."

The results are not sufficient to warrant any distinction between those paired in the ranking.

Using Spearman's "Foot-rule," R is as follows:

(1) Tendency to parallel or mixed feelings and quickness in response,—.562. . . .

(2) Tendency to parallel or mixed feelings and scope of attention,—.687. . . .

(3) Quickness in response and scope of attention (possibly scope of response would be better)—.75.

$$P. E. R = \frac{0.43}{\sqrt{7}} = .161 \dots$$

Converting R -values into r -values (using Table 6 in Whipple's Manual, rev. ed. Vol. I), we get the following:

$$\begin{array}{ll} 1) .77 \dots P. E. r = 0.6745 \frac{1-r^2}{\sqrt{7}} = .101 & \\ 2) .88 & .056 \\ 3) .93 & .033 \end{array}$$

Our hypothesis, stated in A.3, seems to have been confirmed so far, and is therefore worth further testing.

It remains now to sum up our results and consider their bearing on the theory of feeling.

SUMMARY AND CONCLUSIONS

The 'law of algebraic summation' of feelings is quite inadequate to account for the results obtained in the present investigation. Simple algebraic summation is only occasional. Displeasure may inhibit displeasure, rather than add to it. (*Cf.*, *e.g.*, the last of the results quoted in A.2.) The resultant feeling depends very closely upon the relations of the whole apperceptive attitudes towards the two separate stimuli, and this, moreover, in a manner that suggests a much closer relation of feeling and intellectual processes than has usually been taught. 'Feeling' and 'meaning' often seem well-nigh identical.

The most important of the results may be summarized as follows: When the two feeling-tendencies are of like intensity, if the total reactions, the apperceptive attitudes, are widely dissimilar, the result is inhibition, whether the feeling-tendencies are of the same or of unlike sign. Inhibition diminishes with increasing similarity, until, with some subjects, there is a tendency to parallelism, and, rarely, fusion. The latter seems to occur usually along with an apperceptive synthesis of the stimuli, and to depend largely upon the objective similarity of the drawings, etc. When the fields of appeal become nearly identical, the feelings either become one, or 'increase by contrast,' in alternation, according as they are of like or opposite value. Parallelism does not occur with feelings of unlike sign when the general attitudes are the same. When the feelings are of unequal intensity, inhibition continues farther up the scale of similarity. In some cases a strong feeling persists without inhibiting a weak feeling, during the exposure of the stimulus which gives rise to the latter. The weak feeling is, as a rule, broken off with the appearance of the other stimulus.

Inhibition becomes more marked the more rapid the alternation of the stimuli, the whole experience becoming 'unreal', 'cold', 'purely intellectual.' The point at which this occurs varies greatly from subject to subject.

Transition, in cases of inhibition, is marked by a kinaesthetic readjustment, usually unpleasant, described by such phrases as 'loss of bearings,' 'a slight conflict,' 'hard to go from one to the other,' etc. This unpleasantness often overlaps the feeling due to the separate stimuli.

The individual differences are of great importance, the occurrence of parallel or mixed feelings having been shown to be correlated with (1) readiness of response—the occurrence of feeling even with rapid alternation—and (2) with scope of response—the occurrence of feeling from the stimuli offered, despite the distraction of a task in mental arithmetic. The indices of correlation are:—(1) .77, P.E. .101; (2) .88, P.E. .056.

Cases have occurred in which feelings were reported increased by attention to them, in the intervals between exposures of the stimuli, as well as cases in which feelings were inhibited by attention to them. Cf. the introspections marked N.B. in A.2.

Experiments on excitement and calm, humor and solemnity, etc., gave results closely similar to those with pleasure and displeasure. Parallelism is a little more frequent with these other feelings.

We may now discuss briefly the value of these results. Titchener's criteria for affection as an independent element seem to have failed. Qualitative antagonism is generally found, but mixed feelings do occur, under certain conditions, which seem to have been hit upon in our work. Besides, inhibition occurs with excitement and calm, etc., as well as with pleasure and displeasure. It seems on the whole quite likely that the special pair of feelings known as 'affection' should be taken along with other attitudes, viewed as responses of the organism, and studied, like the others, with due regard to individual differences. It may quite well be that in this case, as in that of the thought processes, what is simple for one observer may be complex for another, while introspection may interfere with, or intensify, the process. We have found such differences in the present investigation. Titchener's discussion in *Feeling and Attention*, Lecture IV, from page 155 on, should be read with this possi-

bility of different types of organization in mind. I do not intend to attempt any defense of the tridimensional theory, but will only mention the fact that Titchener fails to take account of Alechsieff's critical discussions.

I have but little to add to Titchener's summary of the previous experimental work.²² I should, however, insist that the negative results of Orth and Alechsieff are hardly conclusive. Consider:—In section VIII, 4, of his article, Alechsieff gives two sample introspections in which the feelings alternated, and says, "Aus diesen und 27 ähnlichen Versuchen kamen wir zu dem Schlusse, dass Lust und Unlust nicht gleichzeitig in unserem Bewusstsein existieren können, sie können nicht nebeneinander, sondern immer nur nacheinander von uns erlebt werden" (p. 262). In our work, there were several hundred trials which gave this same result, but there were also a large number in which mixed feeling was reported. A perusal of the two introspections given by Alechsieff will show that not only the feelings, but the awareness of the two stimuli, alternated, and it should be borne in mind that Alechsieff was purposely working with weak stimuli, '(um) jeden Übergang der Gefühle in Affekte (zu) vermeiden.' (page 169). As our results show mixed feelings only in certain of those trials in which the awareness of the two stimuli was also simultaneous, Alechsieff's results only go to confirm our own.

I suggest for consideration the following interpretation of our results:—

Biologically viewed, the fundamental features of life consist of certain periodic functions,²³ hunger, breathing, circulation, sex, hibernation, sleep, etc. As psychologists, we do not need to beg the question as to whether 'spontaneity' is a resultant of these functions or whether the latter form the primary conditions under which a certain psychical factor—(Royce's restlessness)—must work. In either case, behavior does develop, and sensori-motor coördinations are formed. The most important of these latter are the favoring and avoiding reactions,

²² "Feeling and Attention," pp. 46-55.

²³ Cf. Münsterberg. "Psychology and Social Sanity" Chapter I.

which in the course of development become very intricate and complex. The conscious correlate of the manifestation of these coördinations or attitudes is feeling.

The above view harmonizes completely with the customary evolutionary theories concerning feeling, but seems less mysterious. It also serves as a good basis to account for the behavior of the feelings.

Any presentation will have a certain field of appeal, *i.e.* the organism, with its periodic functions and acquired tendencies in a definite state of readiness, will take a certain attitude, a motor readjustment. Attitudes may conflict and result in complete or partial inhibition. If they are wholly unrelated, inhibition is the usual result. If the attitudes are similar and in the same direction, summation, or at least increase, is the result.²⁴ If they are similar and in opposite directions, the result is commonly a sort of pendulum effect, ('increase by contrast'), unless the two are very different in intensity. When the fields of appeal differ somewhat, the differing features sometimes disappear, ('assimilation'), sometimes survive. In the latter case the result is 'parallelism' or 'mixed feeling'. 'Mixed feeling' usually goes with apperceptive synthesis. The two become parts of one situation.

'Two streams of feeling, emphasized in alternation', is now intelligible. The common part of the two responses is continuous and stays at the same level of intensity; the differing parts do not die out, but first one, then the other, is the stronger, as the stimuli alternate. When the two responses are in opposite directions, the common part alternates in direction. The differing parts are more often lost, but in a good many cases do persist. Such is the view which I favor at present.²⁵

It must be admitted that our results throw the way open,

²⁴For an interesting quantitative study of inhibition and summation cf. South's, "The Mutual Influence of Feelings." *Harvard Psychological Studies*, II pp. 141-157.

²⁵The following, from the conclusion of Geiger's *Bemerkungen*, may be of interest in this connection:—

„Ich habe mich hieraus die folgenden Gefühlsverbindungen abgeleitet. Die Verbin-

to some extent, for such notions as 'zentrale Mitempfindungen', 'Gefühlsempfindungen, etc. The question as to the relation between feeling and emotions is all-important here. In view of the similarity of our results and those of Johnston, as well as on grounds of comprehensibility, I am inclined to accept Titchener's criticisms of such theories.

A genetic distinction may of course be drawn between feelings that owe their origin to the direct effect of the stimuli and those which develop indirectly, on the basis of some periodic function, e.g., the attitude towards pumpkin pie. It seems unlikely that any sharp line should be drawn between the feelings of one class and the other, so far as their own nature is concerned. With this I leave the general theory.

Careful study of the behavior of feelings may have not only theoretical interest, but practical value. Certain applications are apparent from the passages cited in the Introduction. 'Dramatic

dungsgefühle sind, als nur durch die systematische Uebersicht erfordert, eingeklammert.

I. Verbindungen von Affektgefühlen.

A. Verbindungen gegensätzlicher Gefühle.

1. Gefühlsverschmelzung (Mitleid).
2. Mehrdeutige Gefühlsverflechtungen.
 - a. Gefühlsverdrängung (unangenehme Speise bei Hunger)
 - b. Mehrdeutige Gefühlsverwebung (Sehnsucht)
3. Eindeutige Gefühlsverflechtungen.
 - a. Eindeutige Gefühlsvereinheitlichung. (überwundene Anstrengung)
 - b. Eindeutige Gefühlsverwebung (Entrüstung)
4. Zwischenverbindung zwischen Gefühlsverbindung und Verbindungsgefühlen.
 - a. Gefühlssubordination (Rache, Neid)
- (5. Verbindungsgefühl: Vertiefungsgefühl)

B. Verbindungen verschiedenartiger Gefühle.

1. Gefühlsverdichtung (Überraschung)
2. " —durchdringung (Kraft)
3. " —coordination (leuchtendes Rot)
4. " —überhöhung (Schreck)
5. " —verknüpfung (freudige Überraschung)

II. Verbindungen von logischen Gefühlen.

A. Verbindungen gegensätzlicher Gefühle

- [1. Verschmelzungsgefühl (Möglichkeit)]
2. Gefühlsentgegensetzung (Zweifel)

B. Verbindungen verschiedenartiger Gefühle.

1. Gefühlsnebeneinander (neue Möglichkeit)

III. Verbindungen logischer Gefühle mit Affektgefühlen.

1. Affektiv-logische Gefühlsdurchdringung (Gewissheit)
2. Logisch-affektives Gefühlsnebeneinander. (unangenehme Gewissheit)

hedging', 'mixture of tones', and 'character-contrast', as discussed by Moulton, correspond closely to the three types of results in our work. Sully's discussion of the play-mood and the limits of the decorous shows that it is well for the dramatist to understand the behavior of feelings. Other practical problems are not far to seek.²⁶ Is it not of value to know that a feeling cannot be inhibited by its direct opposite, provided the original stimulus continues, since the result will be a return with increased intensity,—('increase by contrast'), whereas a feeling can be completely inhibited by another which is sufficiently different? The order of situations in plays and stories, the arrangement of pictures in art galleries, the order of slides in illustrated lectures, even the grouping of advertisements, whether in periodicals or street cars, to be effective must follow the laws of feeling. There is, then, work worth while to be done. The present investigation lays no claim to anything more than an indication of certain possibilities for future work.

The first requirement is adequate apparatus. The tachistoscope devised by Dodge gives a mirror reversal of one of the pictures. This is often very disturbing. Some apparatus must be found which shall eliminate this difficulty, and also permit of continuous alternation either with constant or gradually varying rapidity, as is desired. I have been unable as yet to work out anything satisfactory. I believe, however, that the most effective method would be that of the stereopticon, if a control could be devised.

When we have a workable apparatus, the many part-problems can be taken up carefully and thoroughly, e.g. the study of the effect of various lengths of trial upon the behavior of the feelings, upon inhibition, assimilation, etc., the problem of voluntary control of the behavior of the feelings, not merely the active and passive methods as we tried them, but attempts at fusion etc. etc., the effect of fatigue upon the feelings, especially upon the control of the feelings, etc., etc.

In the present state of the field has been indicated, I now turn to the discussion

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THE PSYCHO-PHYSIOLOGICAL EFFECT OF THE ELEMENTS OF SPEECH IN RELATION TO POETRY

The purpose of this investigation is to determine by means of the expressive method the effects produced by the speech elements in poetry upon both the motor and introspective consciousness.

Lying on the borderland between Esthetics and Psychology, the investigation aims to throw light (1) upon the so-called "tonal theory of poetry," by measuring the emotional value of the sounds in poetic language without reference to alliterative or grammatical devices, and (2) to discover what auditory, kinaesthetic and organic sensations are aroused by the play of vocal functions in meaningful as well as meaningless collocations of the elements of language.

The material is presented in the following order:

1. A statistical determination of the frequency of the various speech elements in English poetry,
2. Experiments upon the psycho-physiological effect of such elements combined in simple relations,
3. Transmogrifications of English poetry to determine reaction to the bare tonal elements, and
4. The esthetic and psycho-physiological question: Is the psycho-physiological value of the poetic sum equal to the sum of the psycho-physiological values of the separately contributing phonetic elements?

I. SOUND FREQUENCY IN ENGLISH POETRY

Prior to undertaking the experimental work in the laboratory, an elaborate statistical record was made by the writer of the percentage of frequency of the various letter sounds in the leading English poets from Sydney to Rossetti. The basis for this work was the observation of very striking differences in the acoustic and kinaesthetic sensations aroused by the audible

reading of different poets. Especially was the motor pattern and the tonal display of such poets as Byron and Keats noticed, and upon analysis it was found that sound frequency was one element likely to contribute to the differences between the effects which they produced. A previous study of vocal music to provide an increased sensitivity to tonal effects, and of phonography to hasten the sensori-motor reactions necessary in making the tabulations were found to have been invaluable psychological instruments in this preliminary work. This analysis required over four years and involved the recording of over 540,000 tonal elements; 46 phonetic rubrics were employed, and where doubt was entertained over the classification of the elements under the various rubrics, recourse was had to general poetic usage as well as to the special idiosyncracies of the individual poets. All the poets were analysed upon the basis of the current English speech, the Standard Dictionary being used as the criterion for pronunciation. The "foot- and -quantity" system was employed to determine the accentuation, and both accented and unaccented sounds were registered in the tabulations. Approximately 1,000 lines of the maturest and most melodious verse of each of the poets were examined. The result of this work is here summarized:

I. English poets usually employ about 10 accented to 8 unaccented sounds. Shelley, Browning and Swinburne are the notable exceptions, each of which gives the ratio of nearly 10 to 10. But their rhythmic patterns determine much of this.

II. Greater variations, general and individual, are noticed in the use of the accented than of the unaccented sounds.

III. Tennyson and Swinburne deviate most from the average use of the sounds; Milton is nearest the average of all the poets examined.

IV. The greatest individual variations are found among the most used accented consonants which occur in the following descending order of frequency: R, N, L, T, S, D, M, etc. But the smallest individual variations in the use of the unaccented sounds occur among those of the greatest frequency of use, namely: ũ, ĭ, ă.

Only a few double consonantal rubrics were employed besides

the familiar Ch, Wh (Hw), Ng, Sh, and Zh; such double sounds as Bl, Cr, etc., were split and tabulated as two sounds. While the work was in progress Josselyn's investigations (see Scripture, "Elements of Experimental Phonetics," p. 501) came to hand, in which it was shown that a double consonant was simply a single one strengthened and lengthened, in so far as the time estimation of rhythmic syllables was concerned; but whether the double or triple consonantal combinations were felt as fusions or as additions did not enter in as a standardizing agent in the above work of tabulation.

Perhaps the most striking thing in the whole work was the constant observation of the modifying influence of R. Besides being the most used sound in English poetry, it is the one most frequently observed as modifying the quality of juxtaposed vowels and consonants, and when so found, its local signature is absorbed by them.

2. THE PSYCHO-PHYSIOLOGICAL EFFECT OF SIMPLE SOUNDS

The experimental work was carried on in the Psychological Laboratory of Harvard University from 1911 to 1914. It began with the audible recitation of groups of five iambs, such as la-mo, la-bo, and la-do. The O was long, and the A was given as the 'Italian' A. On account of the neglect it received in the arsus, however, it became the neutral vowel. The time of the recitation was taken and the chief results were: the appearance of a caesura, the feeling of satisfyingness at the fifth iambic, the changes in feeling-tone, sensations and imagery, as the different combinations were presented. Only the above three iambs were employed this way; in order to obtain a record that was valuable for the correlation of so motor a function as speech, some graphic record had to be employed.

HISTORICAL

Brücke¹ had obtained kymograph records of ictus and arsis by means of a quill marker while various kinds of verse were

¹ Brücke, "Die physiologischen Grundlagen der neuhochdeutschen Kunst."

recited.² Lip movements were also recorded by means of a lever passing from the lips to the kymograph. His findings were that the time taken to tap various kinds of poetic feet were almost equal; at least the arses recurred at equal periods of time, and the abruptness of the departure of the kymographic tracings from the abscissa line was found to vary considerably for different poetic metres.

Bourdon³ had traced the neck vibrations at a particularly mobile place, while certain sounds were uttered, and found very great amplitudes for the vowel I, less great for O, and lesser still for A. It appears, however, that the average amplitudes for combinations of consonants with O was greater than those with I and A; the I-combinations being quite the lowest.

In regard to what one might expect with reference to the general permeability of the psycho-motor organism to stirrings of various sorts, Angell⁴ held that "only those sensations breaking in upon a state of relative quiet disturbed the psycho-physical mechanism enough to make any peripheral difference."

Fere's opinion⁵ on this matter is that a momentary intellectual activity is accompanied by a momentary increase in power of the voluntary muscles. He also found that both under the influence of an intellectual effort and of other things (*e.g.*, speech and odors) certain excitations of the muscular sense were aroused. His studies showed that during the exercise of speech the movements of the right hand were influenced, *i.e.*, augmented, just as one works a treadle with the foot and finds the synchronous hand movements augmented as much as a 6th or a 5th; it is even stated that the right hand in gesture plays a veritable "esthesiogenic" rôle. But the correlation of the amount of energy expended with pleasurable or unpleasurable states, Fere does not report in the case of speech; in connection with odors and the

² The study was not of the pure iambic or other line; inversions of feet occurred, as usual.

³ Bourdon, P., "L'application de la methode graphique a l'etude de l'intensité de la voix," *L'Année Psychologique*, 1897.

⁴ Angell, J. R., "Organic processes and consciousness," *Psych. Rev.* 890.

⁵ Fere Ch. "Sensation et Mouvement," 1900, esp. Chap. 3.

like, the greater energy seems to be aroused in a state of pleasure. But the final generalization is in these terms: "La sensation de plaisir se resont donc dans une sensation de puissance; la sensation de déplaisir dans une sensation d'impuissance."

The development of the expressive method itself is a fitting corollary to the "modern tendency to understand all consciousness in motor terms," and to connect it with the "motor rather than the sensory side of the organism."⁶ Professor Münsterberg's action theory allies itself with the same tendency. Such writers as Dearborn,⁷ Pillsbury,⁸ Alexander,⁹ and others give a large place to the psycho-motor side of the neural arc, though treating the consciously volitional side of consciousness with varying degrees of prominence.

In their experiments upon the "Time relations of poetic metres,"¹⁰ Hurst and Mackay appear to have justified somewhat the method of poetic analysis used herein (only indirectly, however), and while their subjects only scanned silently or tapped empty rhythms, they found that the iambic foot was really short-long and thus that the tapping of the metrical unit laid a stress upon the so-called accented syllable. Inasmuch as greater differences appear among the poets (as found by the tables previously mentioned) in the use of accented than of unaccented sounds, the above results are pertinent to the present investigation. Further comparisons of method and results are hardly possible; they used the iambic foot only in octosyllabic lines, (*e.g.*, Scott's poems), and even then the iambus was frequently exchanged for other kinds of feet.

Scripture's subjects¹¹ read rather than scanned poetry. Triplet and Sandford¹² found that the explosive consonants were

⁶ Kostyleff, quoted from E. B. Delabarre, "Volition and motor consciousness-theory," *Psych. Bull.*, 1912.

⁷ Dearborn, G. V. N., "The relation of muscular activity to the mental process," *Am. Ed. Rev.*, 1909 (14) 18.

⁸ "The place of movement in consciousness," *Psych. Rev.*, 1911, (18) 83-99.

⁹ *Brit. Jour. of Psych.*, 1911 (4) pp. 239-67.

¹⁰ Univ. of Toronto Studies, No. 3, 1899.

¹¹ Scripture, E. W., *Yale Psych. Stud.*, Vol. VII, 1899.

¹² "Studies of rhythm and meter," *Am. Jour. of Psych.*, XII, 1901.

more nearly tapped and spoken at identical times than were the others. S was almost always syncoped. But in the general, they found that finger stress indicates quite well the vocal stress, though minuter correlations are not indicated. To the above results we ally those of Meumann¹³ which state that the time limit of syncope is but 0.02 seconds. Miyake¹⁴ found that the beat of the finger came before the beginning of the vowel when it stood alone, when it had a glottal catch, when it was short or long, followed by a final consonant, or when it was short or long between two consonants. Also, except in the case of B, D, and G, the beat as tapped came before the vowel following these consonants.

With regard to the matter of correlating qualitative consciousness states with the motor consciousness, there is to be mentioned Dressler's work¹⁵ where increased central activity seemed to favor increased rapidity in voluntary movements; also the work of Drozynski¹⁶ which does not crystallize into any specific positive correlation, but shows apparently that the unpleasant stimuli gave the more noticeable arousals. But by 'unpleasant' we must understand here the many meanings of the term in the sense of Wundt's tridimensional theory. This writer used no iambs.¹⁷

So much for a general account of some of the more important and resultful experimentations upon the motor and introspective phases of an expressive method in psychological esthetics (especially *in re.* poetry). But to come down to the particular elements of our own research, especially the form of the rhythmical presentation and the apparatus used; and first the rhythm form.

¹³ "Untersuchungen zur Psych. und Aesth. der Rhythmus," *Wundt's Studien*, X, 1894, p. 419.

¹⁴ See Scripture, "Elements of Experimental Phonetics," esp. Chap. 37.

¹⁵ "Excitement and tapping rates," *Am. Jour. of Psych.*, 1891, IV, p. 523.

¹⁶ "Atmungs und Pulssymptome rhythmischer Gefühle," *Wundt's Psych. Stud.*, Vol. 7, pp. 83-140.

¹⁷ See also for the effects of pleasant and unpleasant music, F. Rehwoaldt, "Ueber respiratorische Affectssymptome," *Wundt's Stud.*, Vol. 3, pp. 149-192.

THE CHOICE OF THE IAMBIC FOOT

It was shown above that iambus is a sufficiently characteristic form to be used as a vehicle for sounds (*op. cit.* Hurst and Mackay). There is another justification, however. From the writer's own experience in the field of literature, the standard, as well as the most dignified line of English poetry is the iambic, decasyllabic line; the comic finds a place much more readily in the octosyllabic (and trochaic) line, or in still smaller forms. No longer line than this has succeeded for great lengths of verse, and most of the sustained work of any considerable length (barring of course Coleridge's "Lyrical Ballads") is written in it, and even lighter works such as sonnets are not rendered over heavy by its use. The iambic foot was chosen because it appeared to be the standard foot in English poetry, not because exceedingly frequent inversions of it did not occur, nor because dactylic and anapestic innovations were not part of the very body of even the heavier epics, nor because runover lines did not frequently render the iambic-trochaic mêle of feet difficult of interpretation in favor of one or the other kinds as the predominant foot,—but because the stress of the accented syllable of the iambus seemed to bring more into prominence the sounds meant to be stressed than did that of the trochee. Hurst and Mackay (*op. cit.*) found indeed that the iambus detained its ictus in the motor consciousness twice as long as its arsis, while the time relation of ictus and arsis in the trochee was only $3/2$ to 1.

The experiments carried on by Stetson¹⁸ and Bingham¹⁹ had effectually shown the advisability of employing some simple voluntary process as a basis for psycho-motor correlation. The method herein employed is practically the same as theirs. The voluntary process used was the tapping movement of the right index finger. This movement is exceedingly simple and natural, and soon tends toward automatism, leaving one's attention entirely free to be directed upon the stimulus. Very rarely did the finger movement return to consciousness after it had become automatic; when it did so, introspection showed a very unpleasant

¹⁸ "Rhythm and Rhyme," *Harvard Psych. Stud.*, Vol. I.

¹⁹ "Studies in Melody," *Harvard Psych. Stud.*, Vol. II.

and turgid state of consciousness; which state seems to be present usually when any chain of habitual responses is broken.

THE APPARATUS

The form of the apparatus was as follows: Upon two tables placed about two metres apart, revolving brass drums were fastened; over these drums passed a smoked paper belt; the driving mechanism was at one end, the record-taking device was at the other. The subject sat comfortably at the side of the table and laid his right arm on a flat wooden rest having a notch sufficiently long to avoid all possible interference with the index finger, which was left free to move throughout its entire natural range of flexion or extension. To have had the finger strike against some resisting surface would have prevented our detecting any slight variations which the stimuli produced and inasmuch, also, as the characteristic departures of the tapped strokes from the abscissa line were of great importance for most of the subjects, the lack of objective controls in the tapping was an obvious advantage.

The periodic movement of the finger was recorded as follows: the end of the finger was placed in an oilcloth cot which was used for all the experimental work without being changed, and from the cot ran a fine silk thread up over a small brass pulley (always kept well oiled) through a guide, and was fastened to a small aluminum marker of triangular shape. From the other end of the marker ran a small rubber band to an upright support. The point of the marker rested on the smoked ribbon, at a place on its surface quite close to the vertical axis of the drum, and so neatly was this whole apparatus constructed and so slight was the tension of the rubber band, that it was hardly perceptible to the subjects and did not interfere with the freedom and naturalness of the movement. The tension was not altered throughout the experiments. The thread and rubber band were renewed in duplicate at about equal intervals and thus the mechanical errors in the recording device were reduced to a minimum.

The smoked paper ribbon was driven by a gravity motor of sufficiently constant speed to reduce the error of inconstancy to

less than 1 per cent. Its rate was 1 cm. = 1.54 sec. The driving mechanism was enclosed in a $1\frac{1}{8}$ in. soft-pine box, lined with very heavy felt, and the only sound audible was an exceedingly faint, and not unpleasant whirr, which soon became accommodated and was never again noticed. A control string passed from the motor up over a pulley to the other end of the belt to where the experimenter sat, and thus the movements of the experimenter were very slight.

As the finger moved up and down while the ribbon revolved, tracings were made on the smoked surface and, since the pointer accurately recorded the full extent of finger movement as well as such qualitative differences as suddenness and quiverings in flexion and extension, the smoked paper ribbon translated much of the voluntary movement into visible terms.

No suggestions or illustrations were ever given as to rate or extent of finger movement; each subject was allowed to make his own pattern, and for this a little preliminary tapping was employed using empty 5-iambic lines. In recording the introspection, which was done without inserting a screen between subject and experimenter, great care was always taken to betray no sign that the introspection given was agreeable or not to the results sought after. At least, all conscious control of the matter was assiduously avoided.

The apparatus was arranged so that the subject sat facing the window, from which only a patch of sky was visible; the aspect was northerly, and there being little or no direct sunlight, the lighting of the room was fairly constant throughout the whole period of experimentation.

PRELIMINARY EXPERIMENTS

The first experiment to be tried with the above tapping device was a decasyllabic line made of five iambs, repeating la-mo. This line was repeated five times. The instructions ran as follows: "This is an experiment upon the psycho-motor effect of the sounds in poetry; while you recite the line, tap at each accented syllable; take your own time to do it, tap in a natural way, in as long or as short strokes as you please; say it in a clear voice

and then introspect upon the three factors of feeling-tone, sensations and imagery, if all three come; otherwise just give me the introspectional conscious content, much or little; it is the sounds and their effects which you are to attend to. I pull this string and start the motor; after that, whenever you are ready, recite and tap; the line is to be spoken and tapped five times; pause between the lines just enough to control the start of the next line; have you got the instructions in mind? are you ready? etc." This instruction was not repeated in toto at every hour's work to every subject; as much of it, however, as was deemed necessary from psychognostic reasons was repeated, in order to get the same 'set' for each group of experiments. Inasmuch as the motor field was so narrow, the tapping soon became automatic, and the instructions could be reduced to: "This is to be tapped as the others were,—on the accented syllable." And since most of the experiments were written out and the accents marked in red, this fact rendered full instructions obsolete.

La-mo was followed on the same day by two other experiments, la-bo and la-ro. All of the eight subjects found the la-ro pleasant; one subject, W., found la-mo unpleasant, and A. found la-bo unpleasant. In general, la-mo was found to have a "soft, smooth character," like the gentler sounds of nature; la-ro, on the other hand was said to represent the roar of waves and to have less personal reference than la-mo; while la-bo implied something insistent and was referred to as "trivial."

One cannot lay much importance upon the affect-motor correlations in these experiments, for the subjects had not yet become accustomed to the tapping; A. and W., for example, found it more convenient and natural (?) at first to represent the ictus by an up-stroke of the finger. The down stroke was suggested, and they attempted it, but for the first few weeks, at least, found that it was more difficult to employ it. The records, nevertheless, were measured with respect to the ictus, whether it had been functioned by an up or a down stroke. It seemed very curious that an accented syllable should be represented by means of the weaker of the two movements of the finger.

Of these three combinations, la-bo appears to have aroused the greatest feeling of energy. A. seemed to find it so; B. certainly declared it as such; L. and T. also indicated the same tendency. But B. moved his finger farther in the recitation of la-mo and la-ro than he did in the more "energetic" la-bo. So did L., while T. who found la-ro to represent "something substantial" employed the greatest force for that sound, and was consistent with respect to la-bo, which he called more active than la-mo. But this was all in the learning stage, and it is not surprising to find that practice increases the length of the tappings, on account of the greater familiarity and confidence with the work which it brings. The subjects were asked to rank these three experiments according to pleasantness, and the following scheme shows what relation degrees of pleasure have to motor discharge in this first group of experiments. (Descending pleasantness represented by A. B. C.)

Subject	(A Tapp.)	(B. Tapp.)	(C. Tapp.)
	Av.	Av.	Av.
A.	-ro 44.1 mm.	-mo 38.3 mm.	-bo 44.9 mm.
B.	-ro 78.1	-mo 83.5	-bo 76.2
F.	-ro 45.8	-mo 58.0	-bo 46.4
L.	-ro 51.8	-mo 66.7	-bo 50.7
N.	-ro 86.8	-bo 87.7	-mo 83.3
T.	-ro 76.0	-bo 73.6	-mo 62.6
W.	-ro 54.8	-mo 24.6	-bo 55.3
Z.	-ro 43.5	-mo 51.2	-bo 42.3

Three of the subjects, A., W., and Z., ascend in length of tappings as the feeling tone ascends. Three of them, B., L., and T., all give the medium stroke to the experiment they found in the middle degree of pleasantness but all three also ally the greatest degree of pleasure to the least amount of motor discharge. The other two, F., and N., show no correlation at all.

The next two experiments were the combinations de-ho, and ho-de (both long vowels). From graphing the objective results it appeared that ho-de produced on the average a greater motor output than did de-ho. Also the curve of the latter rises and falls,—from the first to the middle a rise, and from the middle to the end a descent; this was general for all the subjects: some reported a little exhilaration, strain and the like, but no feeling of fatigue, or exhaustion. The ho-de curve,

on the other hand, rises almost continuously from start to finish, with a remarkable rise on the fourth foot of the fifth group, and a no less striking descent on the last accented syllable of the series. But the last three groups show the same general tendency,—that of emphasizing the motor prominence of the fourth foot of the group. The first group of either, however, shows almost the same kind of form, which may be due to the persistence of the motor “set.”

Three of the subjects, A., L., and T. preferred *ho-de*; in each case the tapped strokes were longer for the more pleasant; but in the former experiments, only one of them, A., showed this feature. All the other subjects, B., F., N., W., and Z. manifested a preference for *de-ho*; all but N, as mentioned above, tapped shorter strokes while reciting it. Four of the subjects found the vocal construction caused by the “*-de*” an unpleasant feature. But inasmuch as there was no objective standard of intensity or other vocal quale which was to be followed, the matter of constriction cannot be raised to a very high importance. One can say “*ho-de*” with countless degrees of energy and the like, and usually no subject intensified an unpleasant sensation; rather was the voice weakened and lowered to avoid it. On the same day, also as *de-ho* and *ho-de* were given, the combination *ra-fo* (both vowels long) was given. The explosive character of the *f* tended upon repetition to destroy the pleasantness with which it started out.

The graphings showed a remarkable steadiness of motor reaction for this combination until the last group of five iambs was reached.

The next two experiments were *de-sto*, and *sto-de* (vowels both long). Curiously enough, the differences in the amount of motor discharge did not appear until the fourth and fifth groups, and while the *ho-de* graph kept rising after the third group, and *de-ho* fell, here the case was altered completely; *de-sto* showed an ascent, but in the middle of the line only (!); but again, the accented *O* produced a slower reaction than did the *E*. One must remember, of course, that not only is the accented syllable different in each of these four experiments.

but also is the unaccented syllable. Furthermore, the subjects reported that not only did the iambic attempt to become a trochee, but the unaccented syllable also tended to demand an accent!

Correlating the feeling-tone with the motor discharge of these two experiments, we find that subjects A. and F. tapped longer strokes for the pleasant than for the unpleasant experiments; N., W., and Z. reversed this; L. tapped longer strokes for the unpleasant than for the indifferent, while B. and T. tapped longer strokes for the pleasant than for the neutral. A comparison of these results with those given previously shows very little constancy.

THE PSYCHO-MOTOR EFFECTS OF N

The next seven experiments were constructed to ascertain the effect of accented N; the unaccented syllable, "be" (short e) was chosen because it seemed to be about as explosive as N, and thus would be a good balance for it. The experiments were: be-ne (e short), be-ne (e long), be-ni (i short), be-ni (i long), be-na (a long), be-nu (u short) and be-noo (oo long). N is also a much used sound in the language. Seeing that these experiments furnished a better body of material than any pairs or triads which had preceded, it was decided to correlate according to the mean, the mean variation, and the range. Introspectively, N appeared to arouse an attitude of negation. This group of experiments also appeared as the conjugation of a verb, and took on at once with most of the subjects a distinctly "oriental" character. The N dominated consciousness, in spite of the changing final vowels. The experiments were all given on the same day; which may also account for the following constancy in numerical results.

Subject	Rank list. Average of the tappings for each subject.							Variation
	Be-nē	-nē	-nī	-nī	-nā	-nū	-nōō	
A.	b	b	c	c	b	b	b	2
B.	h	h	h	g	g	g	g	3
F.	e	e	e	d	e	d	e	2
L.	d	d	d	e	d	e	d	2
N.	g	g	g	h	h	h	h	3
T.	f	f	f	f	f	f	f	0
W.	c	c	b	b	c	c	c	2
Z.	a	a	a	a	a	a	a	0

If, however, we take the averages of the tappings for the pleasant, unpleasant and neutral experiments, no such harmony is manifest. As follows:

Subject	Average of			P U N arranged in order of magnitude
	P	U	N	
A.	52.9	53.3	51.4	U P N
B.	78.0	76.7	80.4	N P U
F.	62.9	62.4	65.0	N P U
L.	62.3	62.5	65.0	P U N
N.	78.5	80.8	—	U P —
T.	68.0	68.1	65.2	U P N
W.	53.8	—	—	— — —
Z.	46.0	49.1	—	U P —

But even if no judgments of unpleasantness or neutrality were made by some of the subjects, yet the above table shows that when such judgments were made, it was not at a time when the tappings were the longest; one is again at this place referred to Fere's "sensation et mouvement", *op. cit.* Where comparison is possible in these above citations, rarely did the pleasant feeling tone go with the longest tapped strokes. The balance hangs almost evenly between neutrality and unpleasantness in this respect.

The rank lists of the mean variations, hereafter denominated by M.V. and of the ranges, that is, the millimetric distance between the longest and the shortest tapped strokes, denominated later by Rnj, show no positive correlation. In this instance, also, the averages of the M.V. and of the Rnj. for the P., U., and N, experiments is hardly significant.

The graphs for these experiments showed that be-ně and be-nē are similar in their capacity to arouse equal amounts of motor discharge. The average difference is but 2mm. All things considered, the increase of motor output was fairly steady from start to finish. Benĩ and Be-nĩ showed a less increase from start to finish and in the third and fourth group of five iambs stood somewhat apart. There was a general rise in be-nũ, but be-nō fell almost precipitously at the close. Be-nā tended to duplicate be-ně and be-nē. These differences can hardly be correlated with those of feeling tone, for the be-ně was found to be pleasant by 3 persons, Unp. by 4, and indifferent by one; be-nē followed it quite faithfully in the graph. The

chosen pleasant by 7, and Unp. by one. Likewise, be-nũ was found pleasant by two persons, Unp. by three, and yet this graph does not ascend at all like the other one or with so great an upward slant; moreover, be-nõõ, which was chosen pleasant by all the subjects (including the 7 who chose be-nē as pleasant), did not produce the same kind of a graph in appearance as did be-nĩ. Furthermore, there was no report from the subjects that they felt the finger strokes getting longer or shorter in any such way as these graphs indicate they must have done. And every one of the subjects contributed to the increases and decreases. One can but conjecture then, that some of the neural currents find their way out of the central system along that motor channel which is already in use, without making their functional nature known to the introspective consciousness. It was unpleasant, also, for nearly all of the subjects to be aware of their lip and tongue movements; and while some of them actually did raise the pitch of their voices at the finish of be-nā and be-nĩ, yet they had no notion of it, much less of the fact that they were tapping in co-ordination with this general increase of effort. The only introspection they gave on this matter was "a feeling of difficulty" (*e.g.* with be-nĩ) and a "feeling of activity" etc. (*e.g.* with be-nĩ). That both of these should produce the same general increase of tapping is interesting.

THE EFFECT OF LONG OO

The next experiments were of the same general character. Ro (long o) was chosen as the unaccented foot, and the long accented vowel was oo (long). Both being long, open vowels, a good balance was expected. Furthermore, the long oo vanishes quite readily into a long o, preceded by R. Unlike the former group, which was devised to study the effect of accented N, this group intended to bring into prominence the mouth resonances, rather than the articulation pressure of the consonant N. The following consonants, in the following order, were prefixed to the accented long oo: B, M, V, TH (sonant), D, Z, SH, J, L, and G. As usual, the iambic decasyllabic line was employed, and repeated five times.

From the introspection given for these experiments it was found that the long vowels employed in them dominated the combinations and had a non-personal reference. Frequently the effect became soporific, and again, when the consciousness of facial expression involved while reciting them became observed, the subjects were inclined to call the emotion thus induced one of "supplication" or "complaint." None of the consonants attached to this vowel ever became at all "hard," or difficult to say.

The rank list for the mean of these experiments.

Experiment	Ro-boo	-moo	-voo	-thoo	-doo	-zoo	-shoo	-joo	-loo	-goo
Subject										
A.	d	e	e	e	f	d	e	d	e	f
B.	e	d	g	g	d	f	f	f	f	e
C.	i	i	i	i	i	i	i	i	i	i*
F.	f	f	c	d	e	e	d	e	d	d
L.	b	c	b	b	b	b	c	b	b	b
N.	g	h	f	f	h	g	g	g	h	g
T.	c	b	d	c	c	c	b	c	c	c
W.	h	g	h	h	g	h	h	h	g	h
Y.	a	a	a	a	a	a	a	a	a	a†

Comparing these ranks with those of the be-ni type of experiment and with only those subjects who took part in both, we find them showing the following divergences from a steady position:

Be-ni, etc.,A. 2, B. 3, F. 2, L. 2, N. 3, T. 0, W. 2.
 Ro-boo, etc.,A. 5, B. 6, F. 8, L. 2, N. 5, T. 3, W. 3.

The first group represented here contained 7 experiments, the second, 10. One can expect a wider latitude of variation in a greater quantity of material. F., alone seems to have increased the ratio of divergence more than would be expected. It was noticed, also, that the organic stirrings which some of these experiments, like, for example, Ro-thoo set up, was not shaken off by him (F.) until several of the subsequent experiments had been performed. Besides, -moo and -voo had disturbed his original position in the ranks, which was "f." He never afterwards regained it in this set of experiments.

The introspection for the above experiments contained many expressions of "feelings of activity," "struggles," "quiet states,"

Subject C. begins to tap the longest strokes of any and † Subject Y. remains in the lowest position. It is the rank of the other seven subjects, who varied the length of their tappings most and whose tappings are nearer to the length which needs to be considered especially.

and so on. Let us compare these with the amounts of motor discharge in the tappings:

A. felt "-moo" to be easy, and "thoo" to be a struggle; and the M. for -moo is greater than for -thoo; again, in -loo, he felt activity, but the M. for -loo is not as high as it is for -shoo, -joo, and -goo, in each of which there were unpleasant mouth sensations. B. felt -joo to be the most energetic, and his M. for this is the highest of the series, 94.8; -thoo, which brought visual imagery of a dense crowd, was accompanied by a M. of 94.0; -zoo, with "openness," has an M. of 91.0; but while with th, "crowdedness" is correlated with 94.8 mm., "sultriness" in the imagery of -doo is correlated with only 77.6 mm. in the mean of the tappings.

C. shows some nice correlation between energetic and passive states in connection with -boo, -moo and -voo; but -loo is higher in the M. than -joo. F. gave the lowest of his M.'s to -voo and -thoo, which he found the most difficult to say, while in the sounds which brought a feeling of activity, he taps the longest strokes. L. does not seem to offer correlation either way. N. during states in which activity is felt, taps longer strokes than when some restraint is manifest; *c.f.* -voo, -doo, -zoo and -loo as compared with -boo, -thoo, and -joo. T. and Y. do not seem to furnish any definite correlations. W. taps variously for the energetic sounds, yet gives a M. of 99.8 to -zoo, which did not seem to appear energetic to him.

Thus the three factors of "free activity," "restraint" and "quiet ease" do not correlate with the amounts of motor discharge in any way as one might expect. Three classes of subjects are evidenced in the above records;—those who tend towards relaxation in the finger when they feel it in the imagery or in the utterance of the sounds, those who do the opposite, and those who vary throughout the experiments. But it is perhaps too early in the work to make any general statements.

Correlation of feeling-tone with motor discharge
Experiments: ro-boo.....ro-goo, as before

Subjects	Pl.	Unpl.	Neutral	
A.	83.6	84.2	84.9	N U P
B.	89.3	77.6	85.7	P N U
C.	103.1	108.8	109.2	N U P

F.	85.6	79.6	—	P U —
L.	61.4	64.2	63.0	U N P
N.	93.6	90.4	—	P U —
T.	85.5	76.1	—	P U —
W.	96.0	—	88.8	P N —
Y.	46.6	46.0	—	P U —

This seems to give a decidedly different sort of result from that of the Be-ni type of experiment revealed with respect to the length of the tapings during the pleasant experiments; but if the single experiments are taken into consideration, it will be found that while subjects A. L. T. and W. do tap the longest strokes for the most pleasant of the pleasant experiments, yet the other subjects do not do so; B. C. F. and N. show no preferences, while Y. taps the shortest. But this division of the subjects into classes does not run parallel with the above division into classes on the basis of motor output and feelings of restraint, activity or quiet ease. But until we come to a set of experiments in which each one of the subjects is represented in all three feeling tones, it is hardly fair to pit one set of results over against another to the detriment of either. It may well be that the continuance of pleasant states or of other kinds has its own special effect upon the motor resources.

Considering the three rank lists, M., M.V. and Rnj. together, find that the number of aberrations from a steady position is very great, and only the following remarks are appropriate:

1. Subjects C., F. and N., approach and sometimes maintain some degree of regularity in the Mean Variation and in the Range, yet only one of these, C., remained steady in the Mean rank list. Here, in the M.V., this subject has four f-positions and in the Rnj., three g-positions, with a general tendency to maintain them. Subject F. who had eight displacements in the Mean rank list, steadies himself with three h-positions in the M.V. rank list, and with three g-positions in the Rnj. rank list, but only in the latter does he tend toward making that letter his noorings. Subject N., with five displacements in the Mean rank list, shows here steadiness in the M.V. list, tending toward the a position, and in the Rnj. list his tendency is toward maintaining the same position also. But Y. who was absolutely steady in

the Mean rank list, here shows only three h-positions in the M.V. and but three c-positions in the Rnj. rank list.

2. Most of the other subjects, who tended toward some steadiness in the Mean rank list, here are scattered up and down the scales in complete disorder.

Correlations were shown before between the feeling tone and the averages of all the tappings for the pleasant, unpleasant and neutral combinations. There follows a similar table, showing the correlation between the feeling tone and averages of all the mean variations and ranges of all the tappings during the various feeling tone states.

(Repeating the former correlations with the mean.)

Experiments: ro-boo.....ro-goo.

Subjects	Pleas.	Unpl.	Neut.	(Mean)	N U P
A. M.V.	3.1	3.4	2.4		U P N
Rnj.	18	19	12	"	U P N
B. M.V.	3.7	3.2	4.1		P N U
Rnj.	20	13	19	"	N P U
C. M.V.	4.5	3.7	2.9		P N U
Rnj.	19	24	16	"	U P N
F. M.V.	3.5	3.0	—		P U*
Rnj.	20	18	—	"	P U
L. M.V.	3.5	3.4	4.9		U N P
Rnj.	18	17	23	"	N P U
N. M.V.	2.2	2.9	—		P U
Rnj.	12	14	—	"	U P
T. M.V.	3.8	3.4	—		P U*
Rnj.	20	19	—	"	P U
W. M.V.	3.0	—	2.7		P N
Rnj.	16	—	17	"	P N
Y. M.V.	3.1	3.0			N P
Rnj.	17	22			P U
					P U
					U P

* Where only two kinds of affective judgments are made, of course the possibility of correlation is better, but even chance would give as good correlations as N., W., and Y. show.

The graphs for these experiments showed an entirely new character in the visible record of the average of the tapings for all the subjects. With few exceptions, the rise is only initial, but not a great deal of importance is to be given to the first group of five iambs in any of the experiments, because no preliminary tapping was done by any of the subjects; they all began to tap and recite at the same time. The averages of the Be-ni type of experiment were all below 70 mm.; these are all above 77 mm.; evidently all the subjects got more familiar with the work. Indeed, all of them seem to have by this time passed the period of the "Anregung," as can be easily demonstrated from the tables which are to follow. It remains to be seen whether the subjects respond to the material of the experiment in such a way as to furnish correlations between feeling-tone and motor discharge in point of Mean, Mean variation and Range that will be of any service in determining the psycho-motor effect of the speech elements in poetry. Referring to the graphs again, it appears that the vowel OO swallows the consonants which precede it, and to produce in the drawings the visible effect of OO rather than of B, M, TH and so forth. It remains to be seen whether the other vowels to be experimented upon perform this same usurpative function or not. It will be remembered that ÖÖ dominated also the introspective consciousness in these experiments.

THE EFFECT OF LONG E

The next ten experiments were devised to exhibit the effect of long E. The unaccented syllable was "la" (given as the Italian A, but it immediately became the neutral vowel).

In general, the effect of "E" was to produce feelings of tension, and as a long vowel, it was thought to take decidedly less time than either O or OO. Some of the subjects tried to "put force into it," but did not succeed; it appeared to cause restraint, rather than the "expected sense of outward control." All of the subjects called its pitch very high and not at all like the effect of most words containing long E's. It also appeared to be more modified

by the consonants preceding it than were the vowels in the other previously given experiments.

From constructing the rank lists for the mean of the tappings for these experiments, one sees greater variation from a steady position than with either of the two previously given groups of experiments. As follows:

Be-ni	A. 2	B. 3	C. 3	F. 2	L. 2	N. 3	T. 0	W. 2	Y. 0
Ro-boo	5	6	8	8	2	5	3	3	0
-be	10	11	8	9	3	7	9	3	0

Should we ask whether the change in the rank for each subject denotes a change in the feeling tone, the answer is doubtfully given either way. Y. found all these experiments pleasant, and keeps the same rank, but this subject's tappings are way lower in length than any of the other subjects'. W., who finds the last nine of these experiments pleasant and who varies very little in his position in the ranks, may be said to be fairly constant, but subject B., who also found the last nine experiments pleasant, varies his position in the ranks more than any of the other subjects (11 points). L., who is quite steady, varies his position even when he finds consecutive experiments are equally pleasant or otherwise; while N., who finds the first eight experiments pleasant varies most during the first part of the rank list.

Let us examine once more the averages of the tappings with reference to feelings of activity, hindrance and the like. A. felt a strain while reciting -fe, and his average is low; -ke and -le, which were felt to be active, show high averages. C. found -ne more energetic than -le, but tapped shorter strokes for it; -ge he found to be "powerful," and his average tapping is the highest for this sound. But -che is also quite active, and yet the average of the tappings is low. He thought he was tapping very long strokes for -the, but he was mistaken. F. began to feel strain sensations with the recitation of -ne, and from this point on he taps longer strokes; he called -ke less free than -le, and taps longer strokes for the latter sound. When L. found -ke a "hard" sound, his tappings were lower. Usually, the more "harmonious states of mind" brought the lowest averages for N.'s tappings. But when W. felt the freest, his tappings were the longest. Sub-

jects A., F., N. and possibly W. seem to be keeping quite constant; they tap the longest strokes in the freely active states, and vice versa.

Correlation between feeling-tone and motor-discharge, with respect to the mean of all the experiments, grouped under the three degrees of affect, P., U., and N.

Subjects	Experiments: -be.....-ge.			
	Pl.	U	N	
A.	85.0	81.3	85.0	P N U
B.	83.0	—	87.4	N P
C.	93.1	85.6	86.6	U P N
F.	81.5	77.0	—	P U
L.	68.3	74.8	65.5	U P N
N.	88.5	—	90.2	N P
T.	79.3	76.2	78.0	P N U
W.	94.8	91.8	—	P U
Y.	50.8	—	—	

Comparing this with the correlations for the -boo experiments, we find F., L., T. and W. somewhat similar in their preferences, but the other subjects vary exceedingly. L., indeed is the only one giving three judgments who duplicates himself.

Of all the subjects, Y. appears the most constant all the way through. N.'s ranges rank fairly steadily, but the Mean is not constant. W. and L. represent the best averages, after Y. C., who varies much in the F.T. as the experiments proceed, also varies much in these rank lists, but other subjects do not correlate in the same way.

Correlation between feeling tone and the averages of all the M.V.'s and Rnj.'s for the various experiments (together with the previously given data for the Mean).

Subjects	Experiments: -be.....-ge.			(Mean)	
	Pleas.	Unpl.	Neut.		
A. M.V.	4.3	5.4	2.6	"	P N U
Rnj.	23	21	13		U P N
B. M.V.	4.1	—	3.0	"	N P
Rnj.	48	—	11		P N
C. M.V.	3.9	2.7	2.3	"	U P N
Rnj.	21	19	20		P U N
F. M.V.	3.9	3.2	—	"	P U
Rnj.	16	18	—		U P
L. M.V.	3.4	3.7	4.8	"	N U P
Rnj.	15	16	18		N U P

N. M.V.	2.3	2.1	—		P N
Rnj.	21	11	—		P N
				"	P N U
T. M.V.	4.2	3.4	4.6		N P U
Rnj.	21	14	16		P N U
				"	P U
W. M.V.	3.6	3.5	—		P U
Rnj.	23	21	—		P U
					—
Y. M.V.	2.8	—	—		—
Rnj.	14	—	—		—

Here W. alone remains constant. All the rest vary almost as much as is possible with three permutable terms.

Something must be said now in regard to the last two sets of experiments in point of constancy in tapping during all the pleasant, unpleasant and neutral states. We observe that the final average of the mean, mean variation and the range do not adequately represent in most of the cases the general results. If one studies the variations from these averages, he will see that especially in the "pleasant" experiments, there is almost no confidence to be put in these figures as representative. It is not so much so in the case of the "unpleasant" experiments. Space does not permit a full review of this interesting point, but *in general, the pleasant states have more varied ways of representing themselves in the tapping than do the others.* The subjects frequently show that there is more variation from the mean of the "pleasant" tappings when there is no interruption in the affective tone as the list precedes, than when some other condition is manifest. This is very curious. And the objections that might be brought against any such method of experiment seem now to have plenty of reasons for their existence. Some might say that there should have been more careful judgments on the degrees of pleasure, to obtain a more accurate correlation, but it was deemed a very arbitrary matter to oblige the subjects to say "pleasure 1, 2, or 3" when they did not feel such a difference to be manifest.

Our next interest is in the graphs for these long E experiments. Ke, -ne, -ve, -me, and -le all start stronger than do the others, and also move straight across the page; while the others ascend fairly well together, but end in different degrees of strength.

The "scatter" of the first group of these graphs is greater in the second and third groups of iambs,—that of the others (the -be, -ge, -the, -che, and -fe) is prominent only in the last two groups. One might say that L, M, N, and V, being pleasant, had here shown positive correlation between pleasantness and motor discharge, since they are all lower than those of the most unpleasant experiments, those employing K, G, B, and TH, but -ke is in the group which shows the less motor discharge. Also -che, which everybody found pleasant, is next to the very topmost graph of the lot, which means that its average is to be placed with the other pleasant experiments. Ranking the experiments in a descending order of pleasantness, below which are the final averages of all the tappings, it can be shown, that with the exception

Exp.	-che	-le	-ne	-me	-ve	-fe	-the	-ke	-ge	-be
Av.	80.2	85.8	82.8	83.2	82.0	78.6	81.2	86.6*	78.4	78.4

of -ke*, the correlation runs positive with the pleasantness. As for -ke, it was chosen as indifferent by two of the subjects. But this apparent correlation may be due to the tapping of but one subject, W., who gave the longer tappings to the pleasant combinations.

The next experiments to be tried were devised with a view of discovering the effect of the "aw" sound. They were five in number. The unaccented syllable was De, (long E). Aw was preceded by these consonants: f, th, t, n, and g.

Introspectively it proved to appear pitched very low, to have a tendency to become nasal; not very musical, but arousing more organic stir than any sound previously used.

The rank list for the Mean of these experiments is, with symbols P, U, N indicating the feeling tone, as follows:

Experiment:	-faw	-thaw	-taw	-naw	-gaw
Subject					
A.	d-P	c-U	c-P	c-P	c-P
B.	f-P	d-U	f-P	f-P	h-P
C.	g-P	g-U	e-P	h-U	i-N
F.	c-N	e-P	e-P	d-U	e-N
L.	b-P	b-U	b-P	b-U	b-N
N.	h-P	h-P	g-P	g-P	f-P
T.	e-P	f-P	d-P	e-N	d-N
W.	i-N	i-U	h-P	i-P	g-N
Y.	a-N	a-N	a-N	a-N	a-N

Notice here that subject Y. would hold position -a- in the ranks, regardless of feeling tone; and that L., whose position in the ranks is -b- in these experiments, shows here lower tapping averages than he has for some time. It is hardly possible to make any statement about these ranks, except to say that the subjects are all more anchored to one position than in the case of either the Ro-boo or the La-be experiments.

In regard to feelings of effort and activity, C. described -naw as requiring effort, but the average for this experiment is almost the lowest of the series. -Taw, which gave a feeling of activity, is correspondingly high, but -thaw, which affected him the same way, fails to show in these averages; -faw, also requiring effort, is parallel in effect to -naw. The other subjects do not furnish enough examples to make correlation exact.

Grouping the above results according to feeling tone, and taking their averages, we obtain:

Subjects	P.	U.	N	
A.	64.9	66.7	—	U P
B.	80.9	73.6	—	P U
C.	88.6	85.3	97.8	N P U
F.	76.4	71.3	70.5	P U N
L.	57.8	55.0	58.0	N P U
N.	86.6	—	—	— — —
T.	75.0	—	73.5	P N —
W.	90.6	97.1	87.2	U P N
Y.	28.9	—	—	— — —

which, as results along this line, are not parallel with any that have been obtained before.

The rank lists for the M.V. and the Rnj. show that of all the subjects, only W. and Y. keep some sort of anchorage in them; the rest vary indiscriminately.

Grouping (and averaging) all the Means, Mean-variations and Ranges according to feeling tone, we obtain the following:

(Order of greatest to least)

Subject	
A. M.	U P
M.V.	U P
Rnj.	P U

B. (as above)	P U P U P U
C.	N P U P N U P N U
F.	P U N N U P N P U
L.	N P U P U N N P U
N.	all P
T.	P N P N P N
W.	U P N P U N U P N
Y.	all N

From the graphs for these experiments it appears that -gaw ascends steadily; -ge, in the preceding series, on the other hand, maintained a horizontal position. -Naw and -ne are likewise opposite in tendency, indicating some comparison in regard to these consonants. But -faw and -fe show the very opposite traits, as well as do -thaw and -the. One is tempted to correlate with the articulation-character of these consonants, but the amount of experimentation is as yet too meager. However, one thing more may be noticed, and that is that all of these -aw experiments but one, namely -naw, show in their final averages that the vowel "aw" tends to swallow up the consonants, in a way that the vowel "e" never did. Time did not permit any further experimentation with this vowel sound; furthermore, it is not a very important one in the tables of sound frequencies for English poetry. For equal bulks of material, nevertheless, and for those consonants which were used alike before accented -aw and -e, it is not idle to point to these results as showing something quite significant in the psycho-motor effect of the speech elements of poetry. Any one can see that the whole matter is one of amazing

complexity; later results may induce some sort of generalization of a specific character, but whether pro or con the matter of vocal valences, cannot be foretold at this time.

EXPERIMENTS UPON THE FOUR MOST USED LONG
VOWELS IN ENGLISH POETRY: A, E, I, O

We next undertook an extended study of the psycho-motor effect of the long vowels A, O, I and E. Ten subjects took part in the experiments.

The experimental material was made on this plan: The unaccented syllable was "la" (neutral vowel), and the various consonants were prefixed to each of the above vowels to make such combinations as "La-BA," "La-DA," "La-CHE," etc. there were twenty-four experiments on each vowel, which at the same time were experiments on each of the consonants employed; thus we had four experiments in which the consonant B was used, and so on for all the series. The experiments were all given to each of the subjects in the same order, at the rate of about eight or ten an hour.

The last two experiments in each series of twenty-four are slightly different from the rest; in the tables presented later they are called A, A₂, O, O₂, etc. The twenty-third experiment in each series, A, O, I, E, consisted simply in reciting the open vowel five times in a group and for five groups, filling in the unaccented syllable subjectively. Experiment number twenty-four in each group is a line of verse in which all the accented vowels are the same, thus: A₂ is the line, "The gray and rainy April makes the May." O₂ is "The homeless ocean moaning o'er the shoal"; I₂ is, "The dying fire lights the silent sky," and E₂ is "And dreaming seem to hear the weary sea." No such lines exist in poetry, but they served the purpose in hand.

It was found by introspection that the characteristic thing about the long A was its "flat and uninteresting" quality. It was by no means energetic, and when liked, it was termed "soft and quiet"; vocally it was felt to be directed downwards rather than upwards. As far as the vowel-vanish is concerned, it was so little noticed by the subjects that we need not mention it; of course

the repetition of the same iambic foot brought it so closely into contact with L in the unaccented syllable, as almost to nullify the effect of the vanish.

One rather remarkable thing was noticed in the numerical results for the A₂ experiments: The mean of the tappings for every subject in this experiment drops below what it was for the previous experiment, A; with most of the subjects, also, it is lower than their average for all the other A experiments. Thus an influx of consonants, to say nothing of meanings, tended to reduce the lengths of the tappings. Changes in the apperceptive consciousness appear to induce changes in the motor setting.

O, from the introspection given upon the experiments concerned with this vowel, was more easily said than A, directed from the mouth more horizontally, more of an object of the esthetic consciousness, and more associated with the wind and water sounds of nature than the preceding one. Consciousness had more play with regard to O than A,—one could inspect the fringes and return to the focus, or maintain disparate foci quite easily during its recitation.

The character of the long I was found to be considerably more intense and forceful than that of A or O. Also the diphthongal character was very poorly concealed under the constant recurrence of the unaccented consonant. But I is a true diphthong and the introspection thus faithfully gives a prominent place both to the E-vanish and to the Italian A with which it begins. Its pitch seemed at once higher, its utterance less smooth, and the mouth movement more conscious than that of the preceding vowels; the drop and lift of the lower jaw was ever consciously prominent. It was frequently remarked that this vowel had very little connection with feelings of personality; the labial consonants had very much more to do with one's self than did the other consonants, and all felt much more intimate than did any of the vowels.

E was the most intense of all the vowels, feelings of strain at once appearing; but it was also more easily controlled by the muscles of the vocal apparatus than was I. Not so resonant as

the O, but it had far more "color" than the A. Reference was always external, the word "he" appearing to be thoroughly onomatopoeic. The tense condition of the mouth during its utterance often gave a feeling of weariness, as the position of the cheeks, lips and jaws is more rigid than in the case of the other vowels experimented upon.

If one should ask how these four vowels stand in the order of pleasantness, the answer is that O was chosen pleasant 126 times out of 240 judgments, I 123, E 120, and A 119 times. The consonants were preferred in the following order: R (28 out of forty judgments), L 27, N 27, V 25, M 24, B 24, D 23, Z 22, Ch 21, F 20, P 19, W 19, J 18 K 18, S 18, T 18, Th 17, St 17, H 15, Q 15, G 14, and Sh 12. One is referred in this connection to an article in the *American Journal of Psychology*, 1912, by Louise Roblee and M. F. Washburn, on the "Affective Values of Articulate Sounds," in which quite similar results are brought forth; the judgments of pleasure and displeasure were in general confined to the final consonants and vowels, and many more sounds were used by these experimenters than we have employed in the above experiments. But that S and I are quite neutral in character, as the above-mentioned article indicates, we have not found to be the case in our own work. I was very insistent, and S became the basis for more unpleasant judgments in the transmutations than any other single sound in the language.

If we compare the consonants in the order of their pleasantness with their order of frequency, as given in the introductory paragraphs, it will be seen that the two orders do not entirely correspond; nevertheless, it is plain that the pleasanter of the sounds, as found in the brief sampling of the consonants, are those which occupy the positions of higher frequency; had all of the consonants been combined with all the vowels, the discrepancies might have been less. Three factors seem to militate against pleasantness in the pronunciation of a consonant: breathiness, vigorous movement of the lips, and the employment of the ends of the tongue in articulation.

A correlation between feeling tone and motor discharge was

found in the following way: If one arranges the averages of all the tappings for each of the experiments upon any one of the vowels in the descending order of motor output, and places side by side the same twenty-four experiments arranged in the ascending or descending order of pleasantness, it will be seen that in the case of the A-experiments that the more unpleasant were correlated with the greater expenditure of motor energy and vice versa. So with the other three vowels, the O, I, and E. This is exactly in line with what was found in the case of most of the earlier experiments in this investigation. Differences in one to one correspondence appear, however in the case of each of these vowels: with the long A, there are seven such correspondences; with O, there are eleven; with I, but two, and with E, five. The average displacement for the others is with the A, nearly nine points; with O, eleven; with I, eight; and with E, nine. It was also to be learned that the explosive consonants aroused the motor consciousness more than the softer and more liquid sounds. Besides, those sounds requiring the more facial movement while uttering them arouse the more general somatic activity.

Differences in the time taken to recite these experiments were not to be correlated with either of the above factors of affection or motor output; it is true that the more explosive sounds tend to be said very quickly, perhaps indeed, because many of the subjects tried to say them quickly to get done with them. But the liquids also went quickly, because they blended well together in the combinations,—indeed the whole line of five iambics often went like one ten-syllable foot, according to the introspective report.

As typical of the numerical results of these simple vowel and consonant experiments we next present the averages of the tappings by each subject together with the mean variation and the range, and also the rank lists for the above results in the case of the long O experiments. It will be noticed that most of the subjects change their position in these rank lists quite frequently: this will not mean that there was a corresponding change in feeling tone, but only in motor settings and motor arousal. With the

vowel, O, however, less variations from average position occurred in the case of each subject. The character of O from the introspective report is an interesting corollary to this fact: O was the most pleasant of the four vowels, and the play of consciousness about it was greatest on the side of introspection, but apparently not as regards the motory end of the matter.

The mean of the tappings for the long O experiments follows:

Subject	-Bo	-Do	-Fo	-Go	-Ho	-Jo
A.	86.2	85.3	92.4	90.5	88.2	80.7
B.	94.8	93.6	79.7	81.6	81.3	80.8
C.	110.0	106.3	105.1	105.6	105.1	104.5
D.	90.2	84.6	88.4	83.2	93.9	81.2
F.	90.2	96.8	96.7	92.6	92.2	93.8
K.	41.6	47.6	51.6	53.4	54.5	44.7
L.	77.6	79.0	71.4	62.1	62.5	67.5
M.	89.2	91.2	91.5	91.5	90.3	95.0
P.	126.5	125.2	115.4	120.8	120.4	117.6
S.	103.7	102.4	98.8	105.8	108.0	109.8
	-Ko	-Lo	-Mo	-No	-Po	-Qo
A.	68.1	80.5	79.7	65.0	91.3	88.6
B.	89.5	64.6	84.1	92.7	88.7	83.4
C.	103.2	99.6	101.7	111.5	107.1	111.1
D.	92.7	101.5	97.8	96.6	98.5	90.7
F.	94.8	87.8	90.6	89.5	82.2	90.2
K.	43.9	35.9	30.1	25.2	44.0	37.3
L.	71.8	70.9	65.3	66.0	72.2	67.5
M.	91.5	93.7	94.0	90.7	92.2	98.0
P.	116.1	117.8	115.4	121.0	124.1	127.8
S.	108.3	104.5	108.5	112.2	107.4	107.6
	-Ro	-So	-To	-Vo	-Wo	-Zo
A.	81.2	84.2	83.3	88.1	83.7	87.1
B.	95.0	91.6	91.1	89.3	95.0	91.8
C.	109.0	109.6	105.2	101.4	102.8	106.8
D.	94.4	88.0	91.2	95.9	96.3	96.0
F.	86.6	88.9	87.4	86.7	79.8	83.2
K.	67.5	55.7	37.2	42.1	44.9	42.8
L.	68.0	72.0	74.9	77.4	63.8	74.0
M.	98.9	88.8	92.8	90.8	90.7	91.7
P.	117.3	122.2	118.4	124.4	124.8	125.4
S.	111.9	104.4	110.7	113.2	113.6	113.7
	-Cho	-Sho	-Sto	-Tho	-O-	-O2-
A.	84.5	75.1	86.1	75.8	75.0	74.6
B.	77.4	81.8	86.4	92.6	82.7	101.4
C.	105.9	107.5	105.5	100.2	106.7	97.4
D.	93.3	93.7	93.5	94.7	102.0	101.8
F.	96.8	97.8	93.8	84.8	93.8	95.2
K.	44.5	49.2	31.1	39.8	53.8	45.7
L.	74.1	70.0	78.8	73.1	58.3	71.8
M.	96.3	94.0	103.4	96.0	97.4	95.4
P.	117.8	118.9	122.7	125.4	121.1	124.2
S.	117.4	118.5	118.0	115.5	120.5	125.6

The rank list for the Mean: La-Bo, etc.

Subject	B	D	F	G	H	J	K	L	M	N	P	Q
A.....	c	e	f	e	d	c	b	d	c	b	e	d
B.....	g	f	c	c	c	d	d	c	d	f	d	c
C.....	i	i	i	h	h	h	h	g	h	h	h	i
D.....	e	d	d	d	g	e	f	h	g	g	g	f
F.....	f	g	g	g	f	f	g	e	e	d	c	e
K.....	a	a	a	a	a	a	a	a	a	a	a	a
L.....	b	b	b	b	b	b	c	b	b	c	b	b
M.....	d	c	e	f	e	g	e	f	f	e	f	g
P.....	j	j	j	j	j	j	j	j	j	j	j	j
S.....	h	h	h	i	i	i	i	i	i	i	i	h

Subject	R	S	T	V	W	Z	CH	SH	ST	TH	O	O ₂
A.....	c	c	c	d	d	d	d	c	c	c	c	c
B.....	f	g	e	e	f	f	c	d	d	e	d	g
C.....	h	i	h	h	h	h	h	h	h	h	h	f
D.....	e	d	f	g	g	g	e	e	e	f	g	h
F.....	d	f	d	c	c	c	g	g	d	d	e	d
K.....	a	a	a	a	a	a	a	a	a	a	a	a
L.....	b	b	b	b	b	b	b	b	b	b	b	b
M.....	g	e	g	f	e	e	f	f	g	g	f	e
P.....	j	j	j	j	j	j	j	j	j	j	j	j
S.....	i	h	i	i	i	i	i	i	i	i	i	j

The mean variations for these experiments: La-Bo, etc.

Subject	Bo	Do	Fo	Go	Ho	Jo	Ko	Lo
A.....	4.2	7.1	3.7	5.0	3.5	3.6	6.7	2.6
B.....	4.6	3.2	3.1	3.1	3.8	4.8	7.2	4.7
C.....	3.7	2.4	2.1	2.4	3.1	3.3	2.7	2.8
D.....	2.1	5.6	2.6	5.2	1.8	5.0	2.8	2.2
F.....	3.8	3.3	3.0	3.3	5.6	4.3	3.4	5.5
K.....	4.1	4.9	4.2	3.4	4.1	4.0	2.9	4.8
L.....	2.8	2.5	4.1	4.8	5.5	4.5	5.6	3.8
M.....	3.0	1.6	2.8	2.2	2.3	2.4	2.4	3.4
P.....	1.9	2.1	5.0	3.0	2.2	3.4	3.8	3.8

Subject	Mo	No	Po	Qo	Ro	So	To	Vo
A.....	3.3	4.0	2.8	4.9	5.7	3.2	4.8	5.3
B.....	4.9	4.3	7.3	5.2	4.5	5.8	5.1	7.0
C.....	3.1	2.8	3.2	2.9	2.8	3.3	3.2	2.4
D.....	1.4	2.2	2.9	3.2	2.5	3.6	1.8	3.9
F.....	3.2	3.8	6.2	3.9	3.6	3.6	3.9	3.6
K.....	4.4	3.7	3.8	4.8	5.6	4.3	5.4	4.4
L.....	3.5	4.1	2.3	4.1	2.7	3.1	6.0	3.8
M.....	2.4	4.2	3.3	4.0	2.6	2.9	3.4	4.2
P.....	3.8	3.7	5.9	2.5	1.8	2.4	2.5	2.5
S.....	2.6	1.7	4.0	2.6	2.2	4.6	4.1	2.5

Subject	Wo	Zo	Cho	Sho	Sto	Tho	O	O ₂
A.....	4.1	4.1	5.4	7.1	5.2	4.1	4.2	4.1
B.....	4.8	5.0	4.1	5.5	6.0	5.8	4.1	4.1
C.....	2.7	3.7	3.0	3.3	2.5	2.2	3.0	3.0
D.....	2.8	3.9	2.1	1.6	2.6	3.5	1.6	1.6
F.....	3.0	4.0	3.2	2.7	2.9	4.7	1.9	1.9
K.....	4.2	5.8	5.9	3.4	4.7	5.2	4.7	4.7
L.....	4.3	4.3	4.8	5.0	4.5	5.4	5.4	5.4
M.....	2.6	3.2	3.6	3.9	3.8	5.5	2.7	2.7
P.....	1.7	1.7	2.0	2.5	1.9	1.6	1.6	1.6
S.....	2.2	2.2	2.2	2.2	2.2	4.1	4.1	4.1

The rank list for these mean variations: La-Bo, etc.

Subject.....	B	D	F	G	H	J	K	L	M	N	P	Q
A.....	h	j	g	i	f	e	h	b	f	g	b	i
B.....	i	e	e	e	g	e	i	h	j	j	j	j
C.....	e	c	a	b	e	c	b	c	d	c	d	c
D.....	b	i	b	j	a	j	c	f	a	b	c	d
F.....	f	f	d	f	j	g	f	e	i	f	e	h
K.....	g	h	i	g	h	f	e	i	g	e	f	h
L.....	c	d	h	h	i	h	j	f	g	h	a	g
M.....	d	a	c	a	c	b	a	d	b	i	e	f
P.....	a	b	j	c	d	d	g	e	h	d	h	a
S.....	j	g	f	d	b	a	d	g	c	a	g	b

Subject	R	S	T	V	W	Z	CH	SH	ST	TH	O	Oz
A.....	j	d	g	i	g	g	j	i	i	g	i	h
B.....	h	j	h	j	j	h	j	j	g	i	j	g
C.....	c	e	c	a	c	c	f	a	b	d	b	e
D.....	f	e	a	f	d	b	a	b	e	c	c	a
F.....	g	g	e	d	e	f	b	e	d	f	f	b
K.....	i	h	i	h	h	j	e	h	j	j	g	i
L.....	e	c	j	e	i	i	g	d	h	h	h	j
M.....	d	b	d	g	b	e	i	f	f	c	i	d
P.....	a	a	b	b	a	a	c	c	a	a	a	c
S.....	b	i	f	c	f	d	d	f	e	b	e	f

The rank list for the ranges: La-Bo, etc.

Subject	Bo	Do	Fo	Go	Ho	Jo	Ko	Lo
A.....	20	32	21	26	16	16	35	12
B.....	24	14	18	16	17	20	27	25
C.....	18	15	9	14	15	17	16	14
D.....	11	28	12	30	12	22	20	11
F.....	18	18	15	22	23	18	21	28
K.....	18	22	16	19	18	23	29	21
L.....	15	13	19	20	21	19	25	17
M.....	14	9	13	10	10	11	16	16
P.....	8	10	20	12	11	21	12	22
S.....	19	22	24	18	20	8	15	26

Subject	Mo	No	Po	Qo	Ro	So	To	Vo
A.....	23	23	15	23	22	15	29	25
B.....	30	21	26	28	23	24	39	28
C.....	11	15	14	15	11	23	17	14
D.....	9	12	13	16	13	17	12	19
F.....	24	19	28	17	24	13	20	18
K.....	21	20	19	29	20	25	25	27
L.....	18	16	21	24	14	18	27	22
M.....	12	22	16	21	21	12	22	20
P.....	19	18	30	11	12	21	14	17
S.....	10	9	27	22	9	16	18	12

Subject	Wo	Zo	Cho	Sho	Sto	Tho	O	Oz
A.....	25	20	19	31	25	19	21	22
B.....	32	21	23	25	30	30	18	26
C.....	19	15	13	13	11	11	17	12
D.....	13	12	11	7	15	17	7	10
F.....	12	18	15	17	20	24	11	17
K.....	28	28	26	12	18	22	21	23
L.....	22	16	21	37	22	18	27	18
M.....	14	17	18	19	21	23	19	21
P.....	18	7	9	18	19	7	16	11
S.....	20	14	14	11	24	33	20	20

The rank list for the ranges: La-Bo, etc.												
Subject	B	D	F	G	H	J	K	L	M	N	P	Q
A.	i	j	i	i	e	c	j	b	h	j	c	g
B.	j	d	f	d	f	g	h	h	j	h	c	b
C.	e	e	a	c	d	d	d	c	a	c	b	a
D.	b	i	b	j	c	i	e	a	a	b	a	i
F.	f	f	d	h	j	e	f	j	i	f	e	d
K.	g	h	e	f	g	j	i	f	g	g	e	j
L.	d	c	g	g	i	f	g	e	d	d	f	h
M.	c	a	c	a	a	b	c	d	d	i	d	e
P.	a	b	h	b	b	h	a	g	f	e	j	a
S.	h	g	j	e	h	a	b	i	a	a	h	f

Subject	R	S	T	V	W	Z	CH	SH	ST	TH	O	O2
A.	h	c	i	h	h	h	g	i	i	e	i	h
B.	i	i	j	j	j	i	i	h	j	i	e	j
C.	b	h	c	b	e	d	c	b	a	b	d	c
D.	d	e	a	e	b	b	b	a	b	c	a	a
F.	j	b	e	d	a	g	e	e	e	h	b	d
K.	f	j	g	i	i	j	j	c	c	f	h	i
L.	e	f	h	g	g	e	j	j	g	d	j	e
M.	g	a	f	f	c	f	f	g	f	g	f	g
P.	c	g	b	c	d	a	a	f	d	a	c	b
S.	a	d	d	a	f	c	d	b	h	j	g	f

In measuring the ranges, it often happened that two or three subjects could have had the same position; re-measuring, however, or allotting to the subjects that position which they had previously tended to maintain, obviated the difficulty; for example, where subjects A. and B. were equally set for the position C in the rank lists, but had previously maintained positions B and C respectively, we assigned to them positions B and C, in order both to have ten positions, and to give each of them the benefit of the doubt. With differences of tenths of a millimeter as the basis for many of the correlations, it was not always easy to determine the exact status of affairs for any one subject with respect to another better than by the above method.

If one is again asked what changes of position in the rank list for the Mean denote, or even what changes in the average of the tappings denote, we are somewhat at loss to give a fully satisfactory answer; it is not due to a change in feeling tone so much as it is due to various manifestations of the motor consciousness during the continuance of the same feeling state. As was previously noticed in the first year's work, it is usually

quite a while after the onset of a new feeling state, that a change takes place in the motor manifestations. And insofar as we correlate with the introspective report as a basis, we find that the mean variation of the tappings made during pleasant states is greater than the mean variation of the tappings made during unpleasant or neutral states; thus pleasure is manifested, at least in this experiment, by more varied expressive means than are the other affective states of consciousness. The introspection in connection with the pleasant experiments is richer, the associative functions are more operative, and the general bodily and mental condition is more indicative of ever new and varied manifestations in those states in which fine esthetic feelings are present than in those which appear to indicate the presence of cloggings, inhibitions and mutually antagonistic impulses.

Graphing these experiments revealed the following characteristic differences between the vowels:

The ascending order of motor output followed the series as presented, A, O, I and E. This may, however, be due to practise alone. But within any group of experiments other differences are quite significant, especially if compared with the introspection as given above; the O-graphs showed the least scatter, the E and I the most. E and I also march straight across the page, while A shows an "Anregung" incessantly throughout the series. E and I are also spoken in a shorter time than are O and A, and besides, the O-graphs are all indicative of the fact that this vowel was spoken in more nearly the same time even though preceded by the various consonants than were the E and I. Here, in the case of O, steadiness of motor discharge, as evidenced by the fact that the vowel tends to swallow the consonants which precede it, is correlated with steadiness and evenness of introspectional content and attitude; with E and I, on the other hand, the exact opposite is the case. The number of factors involved is many, and the final result may perhaps be tentatively stated as follows: Quickness of utterance is correlated with greater motor output; strain in the vocal apparatus with unevenness of motor output; ease with which the vowel is spoken

dominating the strain-effect of the consonants; and a negative correlation between unevenness (though unfelt) in the motor output, and also amount of such output and the pleasantness as aroused in the introspectional conscious content.

From a different graphing of these experiments, four in a group, on the basis of consonants preceding the vowels, where in each group of four drawings, one finds those graphs together which show the differences between the four vowels under the influence of the same initial accented consonant, we found that in nearly all the cases the A graph is the lowest, frequently very much the lowest, while again, the O, E, and I graphs exchange positions of height and extent to some considerable degree. But this is quite natural,—if the O had been *by far* the pleasantest vowel, or the R *by far* the pleasantest consonant, we might have expected the R and O graphs to appear unique and different from the others, but the vowels are almost equally pleasant, a difference of but seven judgments of agreeability separating the O and the A (the extremes), and the consonants grade very gently from the most to the least agreeable. So it is perhaps correct to say that the different graphs represent the matter not so much from the vowel side as from the consonant side when we compare the graphs for any one vowel together but that comparing one of these large groups with another, we have vowel differences rather than consonant differences before us.

The introspection revealed the fact that entirely different states of mind were aroused according to changes in the direction of the attention; instructions were given to neglect the physical sensations and to think of the sounds as much as possible, in order to have constant conditions for all the subjects. But this does not seem to have worked very satisfactorily in many of the above cases; frequently, indeed, mixed feelings were reported; the subjects would say: "The sounds are not unpleasant, but I do not like to say them"; or, "The sounds as heard are all right, but the everlasting mouth movement is exceedingly hard to keep out of the focus of consciousness." Absolute restraint was impossible: it would also have been quite unwise, for we were study-

ing the motor as well as the introspective consciousness and to find out just what happened in apparent conflicts between the two, especially on the side of the affections, was considered just as valuable as anything else. It was thought better to let things take pretty much their own course in the matter rather than to be too severely restrictive. But that the introspection was not universally given on the same elements in consciousness is at once evident; whether it could be, whether one can abstract one element and keep it abstracted throughout the series is very doubtful indeed. And yet, when we compare the amounts of motor discharge for the different subjects, the question comes up: "What was it that was called pleasant or unpleasant?" Well, the only thing to say is that the whole experience extended toward this or that type of affective tone, and that is about all that can be said. And if that is insufficient, then some other method must be devised to experiment upon these simple vowel and consonant combinations than we have employed. One will also notice that the subjects talked about some of the combinations having a higher pitch than others,—unless the pitch was changed, subconsciously, of course, to relieve the uniformity, this is quite incomprehensible; surely the vowel A does not take a different pitch in connection with some of the consonants than it does with the others, and if it seemed to, it is not unlikely that this was an illusion due to the greater intensity required to enunciate the explosive consonants. And as there was no uniformity on the judgments of pitch, even among the two or three subjects who made them, it is hardly possible that the pitch judgments indicate anything objective.

One final point also needs to be emphasized; which is that the tendency to make words out of these meaningless experiments was super-strong with nearly all of the subjects. As one subject said in regard to the transmogrifications: "It tantalizes me dreadfully because the words I get out of them have such disjunctive meanings." So that some severe critic might call this whole work, "An experiment in the delayed associations of misspelled words." But following such caustic criticism, let such

a person suggest some better way than we have used to experiment on the psycho-physiologics of the sounds of the language, and thus help us out of the dilemma. We admit right at the start that the whole realm of psychological esthetics seems to be constituted chiefly by its difficulties.

SIMPLE EXPERIMENTS CONSTRUCTED FROM THE TABULATIONS OF SOUND FREQUENCY IN ENGLISH POETRY

In connection with the next two sets of experiments reference must be made to the previously mentioned tabulations of sound frequency in the poets. If one arranges these percentages in order of magnitude for each one of the poets, it will be noticed that certain sounds are almost equally prominent for all of them, especially in the unaccented lists; and here we refer to the short U, I and A. Of the accented sounds, the consonants, rather than the vowels appear to be common property, inasmuch as R, T, D, L, S and M usually stand at the head of the lists.

The next twenty-seven experiments were devised to show the effect of the most prominent of these accented and unaccented sounds. They nearly all contain two unaccented and three accented letters. But these combinations, unlike those which have hitherto been employed, usually end with a consonant, L, N, D and T predominating. Thus the body of sound produced by their utterance is something more solid than we have had before; and the organs of articulation exercise more control than they did in the case of the long, open vowels.

No single poet's preference for certain sounds is especially represented in these combinations; the whole twenty-seven of them merely exhibit the most used accented and unaccented sounds of English poetry "ueberhaupt." The introspections for these experiments, which were given in the following order, is of interest:

I. Ne-rol. (Iambic foot, vowels both short; repeated five times in succession for each of the five groups.)

Implies the joys of rustic work and pleasure; visual imagery of the fields in summer. Soft and musical; suggests rapidity of movement; imagery of some

May morning. Soft, drawn-out thing; not very active nor deep, but it has body. Very musical and easy to say; sounds like the ringing of a bell; slightly monotonous, but not dull.

II. Un-ral(e). (As above, metrically and in groups; short "u," long "a".)

"R" the best thing about it; seems slightly inharmonious and perturbing; no definite imagery. Not energetic; couldn't keep the word "unreal" out of mind. "Ral" gives a feeling of contraction; seems inefficient; thinks of the pattering of hail or of big water drops.

III. Id-rel. (Vowels both short.)

Sounds have a bell-like quality; seems to refer to some celebration. Hard to keep the two syllables apart. Became "id-well," "did-well"; dislikes the jump from the first syllable to the second. Musical, hopeful sound; keeps ascending in pitch.

IV. Ri-tin(e). (First "i" short; second, long.)

Visualizes self on the sea shore on a warm, summer day; feels the heat and his own body distinctly. Makes him frown. Seems narrowly concentrated in space. Musical; in major key; encouraging; calls up the word "time." Emphatic, but monotonous; gets nowhere.

V. Tu-lin. (Both vowels short.)

Energetic and poetic; visualized the sea; heard the wind and the sound of the breakers. Very easy and pretty sound to make; suggests a light, fleet movement. Seems rapid; "tu" is light; "lin" heavy; good combination of sounds; very easy to coördinate finger and voice. Soothing and quieting; a lullaby; the alternation of the vowels is charming.

VI. Ti-ren. (Both vowels short.)

Sharp and concise and easy to produce; but the "n" seems to negate the expected climactic character of it. The superficial gloominess of a rainy, indoor day implied; not very "deep" sound; visual imagery dark gray. Couldn't keep track of the counting. Forceful sound; calls up the words "to arms!"

VII. Ti-rel. (Vowels both short.)

Bell-like; musical and melodious; implies recreation; visualizes a country dance in the moonlight; slightly erotic. Energetic and speedy; runs together well; suggests the full pleasure of animal spirits. Just a happy little fool's song; jolly. Good lyric poetry; "It may be flip, but never mind"; the sounds run together beautifully. Exciting and exhilarating; feels the pitch to be very high; brings a joyful and exuberant feeling.

VIII. Un-dol(e). (First vowel short; second, long.)

Has distinct musical quality, but the tone is sad and mournful. Romantic, but doleful; recalls the "Nebelungen Lied"; implies the pathetic fallacy. Quiet and sombre; calls up the tolling of a bell; exceedingly passive thing. Means a mild lament, or self-pity; the nasal sensations almost become unpleasant.

IX. A-ren(e). (Short "a," long "e.")

Almost meaningless and nonsensical; seems to be calling someone by name. Not poetic; thought of things colloquial. More energetic than "un-dol." Very uninteresting and commonplace; "ri-tin" was intellectual; this is stupid. Has a romantic quality; seems like some amorous declaration; musical. A small sound; it is minor music; makes one quiet and thoughtful; might arouse pity and sympathy.

X. Ri-nad(e). ("I" short, "a" long.)

Something mournful about it; but not much to it. Induces a slow, passive state. Seems to stay way back in the mouth cavity; can't raise the pitch enough to make it effective. Seems contracted and nasal; has no life or activity to it; too inward. Emphatic; almost a battle cry; very dignified sort of a thing.

XI. Un-rin. (Both vowels short.)

Emphatic; the rhythm is easy and regular, but the nasal quality is not enjoyable. Something hopeless about it; seems like a cry; Shelley's poetry came to mind.

XII. Ti-ra. (First vowel short; second, long.)

Thinks of something like political excitement; "sis-boom-bah-rah" and Roosevelt prominently in mind. "It's just some conversation." Nice and quick; happy and joyous; expansive; suggested "hooray." Implies jubilant and exultant action.

XIII. Ni-dal. (Both vowels short.)

Reminds of very fine and pleasing music. Active and energetic; suggests the military. Calls to mind the girls of Biskra in the street of Ouled Nail. "It tumbles out of the mouth before you want it to"; implies following the line of least resistance. Something important and also impatient about it; implies hurrying.

XIV. Ri-leet. (Short "i.")

Like a bird song; bright and vivacious. Not very deep, but joyous; images a woodland scene in the summer; birds and squirrels plentiful. Feels hurried; thinks of the song of a lark. Thought of "relief," "rillet," etc. Very dainty, light and springy; something bright and feminine about it.

XV. Ti-reen. ("I" short.)

Thinks of the mightiness of nature; wild gray ocean and sea gulls imaged. The explosive quality of "ti" well counterbalanced by "reen"; it's like a litty sung to oneself. This is romantic and "eulogistic"; might be a love sonnet, or some manifestation of devotion.

XVI. Ri-nel. (Both vowels short.)

Has a bell-like quality; "rin" is subjective; "el" objective. Like a bell; fanciful, but not solemn. Seems to swell in volume as it proceeds; good sound with which to call any one. Something personal, fatalistic, and strong.

XVII. Ni-lur. (Short "i"; "u" as in "fur.")

Seems deceitful, and slippery; too smooth to have any body to it. Implies a Byronic despair; visual imagery of a gray cold autumn sky. Difficult to say; gets way up in his nose; suggests peevishness; wanted to prolong the "lur" so as to get a firmer hold on the "ni." Hard to say; lacks body; felt almost tongue-tied. Calls to mind some foolish person, perpetually grinning; mouth sensations disagreeable. Very emotional; an intoxicating riot of sounds; full of color.

XVIII. Ri-dev. (Both vowels short.)

Very peculiar; thought of a hot, summer day; also some fiery, physically exciting passion obtruded. Unusual sound; "devil" the only thing that came to mind. Dramatic; alluring; wanted to make it impressive; something funereal about it. Has resonance, but gives a drawn-in, contracted feeling. Funereal and mournful thing; yet has musical quality and fascinates one.

XIX. Ni-rees. (Short "i.")

Very musical; sounds like whistling. Feels the "s" stops one short; like putting on the brakes suddenly while driving an auto. Mouth movement seems delicate; tends to fuse into "niresnires," etc., without stopping between the syllables; like singing a little tune to himself; feels contemplative. Sort of a "love motif"; imagery of a woodland scene, with birds and soft, quiet places. "S" softens, hushes, smoothes; very light and dainty sounds; "s" also seems like spreading something over a broad surface. Foreign sound; slightly wistful; yet gives a feeling that something is inevitable. Quieting, slightly monotonous sound; thought it descriptive of the waves on the sea shore.

XX. Un-reen. (Short "u.")

Something profound about it; a sad, unsatisfied cry, either sexual or spiritual. Sounds like a call for help; or else it is some exposition of an important theme. Doesn't allow one to expand; a climax foreshadowed, but not reached. Something romantic, supplicating and pathetic about it.

XXI. Ni-ral. (First vowel long, last vowel short.)

Imaged a big country fair, where everybody was having the time of his life; "ni-ral" is everybody calling everybody else. Called up visual image of Millet's "The Lark." Makes him keep his mouth open all the time; gives a cold feeling all over; the "i" seems like something pointed; almost deprecative.

XXII. Thi-ra. ("Th" sonant; "i" short; "a" long.)

Has much quality, but the lisping character of the "th" almost killed the feeling tone. The syllables do not seem to belong together; "th" irritates; seems like a lover's lisp. Peculiar mixture of sounds: "th" always repulsive; "ra" very fine; almost a case of mixed feelings. Difficult to say; seems like a scraping movement along the ground. "A horrid, tongue-tied lisp."

XXIII. Ne-mal. ("E" long; "a" short.)

Implies a superficial pessimism or complaint. Tends to become "nemel" and "nemalne" (trochaic); seems to be just a matter of daily conversation, buying and selling, and the like. Insistent and affirmative; "ne" is the disturbing factor. Implies maliciousness, anger or irritation; it climaxes into a veritable fury.

XXIV. Ri-naz(e). ("I" short; "a" long.)

Very subjective thing; "az" makes it so inward. Slightly energetic; imagery of a rainy day in the country. Slow, monotonous, sombre, deadening; demands much attention to keep saying it. Smooth, but lacks body; like the voice of a mediocre clergyman giving a nice, homely sermon. Quiet-ting, harmonious quality to it; implies the pleasant acceptance of a situation, suited to one's abilities. It means homage to some Oriental monarch whose name is "Rinaz." A lullaby; something almost hypnotic about it; fascinating.

XXV. Ro-len. ("O" long; "e" short.)

Expresses activity, but a superficial kind. Active, and quick; rolls right along. Clear cut, vigorous and manly sound; personal reference to it. "Rol" should have the accent; hence it shuts off the effect of the intended iambic. Rather matter of fact and unimaginative. Rather strong and vigorous; tendency to anticipate the accent with the finger. Pleasant alternations of the vowels: "en" brings one right up standing.

XXVI. Thi-nal. (Both vowels short.) (Th surd.)

"Th" is the disagreeable part of it: something remorseful about it. Gives a blurred effect; the whole thing seems to lisp. "Th" ruins the otherwise pleasant effect of "nal." Easy to say, but the "nal" is too nasal; it whines; reverberations of the sound felt throughout the face. An unimaginative, practical, downright statement of fact. Harsh, complaining, and unsatisfying; the pitch is too high to be pleasant.

XXVII. Ri-neen. (Short "i," long "e.")

Too nasal to be musical; refers to some one other than himself. Rather tiresome work: thought of rowing fast and hard for no particular purpose. Too nasal; the final "n" is the worst part of it; not so mature a sound as "rolen." "Neen" is cold and hollow: too low in pitch; makes one short of breath and demands great depth of voice; "ri" is by itself quite pleasant.

Even from these fragments of the introspection given in connection with the above experiments, it is plainly seen that the responses of the subjects to the material indicate that they had "gotten into" the business of introspection better than ever before. Much of it, is of course not pure introspection; but the instructions were only: "give me what is in your mind after you have recited this combination twenty-five times." In describing the effect of these sound combinations, then, if to say, "it is like

this or that" may not be introspection, yet it did not seem possible to obtain any other introspective results than the above. And yet, in the light of future experimentation, just this kind of answers upon the effect of the sounds in poetry seems to be the only thing one can obtain, and furthermore, the results obtained from introspecting upon passages of poetry transmogrified into meaningless jargon justifies to a considerable degree the method of introspection which most of the subjects employed. One thing more, also; the idea that the subjects would all introspect upon the physical sensations or learn to do so was quickly expelled from the mind of the experimenter; the threefold instructions,—to give feeling-tone, sensations, and imagery proved to be too autocratic; what we have in the above, typical introspection is mostly feeling-tone and association. However, inasmuch as most of the subjects did not like to think of the movements of the organs of articulation while they were speaking, to have insisted that they do so would have been fatuous.

All in all, the introspection is the valuable part of this set of experiments so far as we have gone with them. Diligent and careful study of the rank lists for the above experiments has failed to show that changes in feeling tone, changes in the feeling with regard to "activity," "passivity," "energy," and the like states can be traced down to the numerical results with any certainty. On the whole, however, the experiments in which the subjects found difficulties of enunciation and the like, produced the longest tappings. Feelings of free activity and pleasurable, "dolce far niente" states usually correlate with lower tapping averages than do other states. The word "activity," however, must not be interpreted to mean a "feeling of work" or "exertion"; often it was hard enough work to recite the combination, but the doing so may have been pleasant or unpleasant, depending upon many factors singly and in a constellation, such as pleasant or unpleasant associations, difficulty or ease of counting the five iambics, and the like; and inasmuch, also, as we have hitherto failed to attempt to "grade" the feelings of pleasure, activity, and the like numerically, correlations of any sort do not show up with

any degree of nicety either way. But the grading of feeling tones is a matter that is on a psychological brink where the footing is horribly slippery; checking up the results would have been necessary, and with this kind of experiments, where the effective phase appears to be so fragile and at the mercy of every other psychological factor, it can hardly be supposed, at least from the results we have already obtained, that to give these experiments all over again in various orders, would have been either wise or fruitful.

We next take up the matter of general correlations between feeling tone and motor discharge, taking all the pleasant, unpleasant, and neutral experiments in groups by themselves and contrasting the means, mean variations and ranges with one another.

Subject	Pl.	Unp.	Neut.	
A. M.	95.9	92.7	91.7	P U N
M.V.	3.0	3.6	3.1	U N P
Rnj.	15	19	17	U N P
B. M.	88.0	86.1	88.6	N P U
M.V.	3.1	3.6	4.6	N P U
Rnj.	18	17	21	N P U
C. (as above).....	107.3	107.4	104.8	U P N
	2.5	2.6	2.7	N U P
	10	11	12	N U P
F.	89.2	89.7	100.1	N U P
	3.2	3.1	3.4	N P U
	17	18	16	U P N
L.	73.8	72.4	74.0	N P U
	3.6	3.7	4.1	N U P
	16	18	20	N U P
N.	86.7	84.8	83.3	P U N
	2.9	2.5	2.7	P N U
	15	11	14	P N U
T.	76.7	—	73.2	P N
	3.4	—	3.6	N P
	18	—	19	N P
W.	95.1	95.2	95.6	N U P
	2.4	2.2	2.5	N P U
	12	10	13	N P U
Y.	50.7	52.3	47.1	U P N
	3.1	4.1	3.2	U N P
	14	18	17	U N P

The difference between these various averages is very slight, and in every case the variations from it are great. But they are usually greater for those which have been obtained while the subjects were in a pleasant state of consciousness than otherwise. Neutrality and unpleasantness appear to work up to a better level in the motor consciousness than does pleasurability; furthermore, introspectively, there are more varied states of pleasure than of unpleasantness; if what the subjects gave introspectively is of any importance, this appears to be empirically substantiated,—they got after a while to be very reticent about the introspecting upon the unpleasant combinations, saying only such things as “very bad,” “I do not care for that at all,” and the like. But when they got a pleasant combination, they would even wander into forbidden fields of introspection and bring back material which had apparently no connection with the subject in hand. Not all of them, however, but it is quite the fact that the subjects who found most of these combinations pleasant, show up the most negatively in these correlations; at least those who got into the most effervescent states of mind offer those numerical results which are the most recalcitrant to satisfactory correlation.

HINTS OF A TONAL CALCULUS

We now turn to the graphs for these experiments. Tiren and Tireen were taken together; and Unrin and Unreen also; these were drawn in pairs to show the differences obtained from those experiments which were the nearest alike. We treated in the same way the graphs for Tira and Thira, and also for Rinaz and Rinad. The remaining nineteen graphs were arranged in groups for similar purposes of comparison.

Comparing Tiren and Tireen, it appeared that the short “e” in Tiren was responsible for the elevation of this graph above the other. Apparently, also, the “*long*” E did not “live up to its privilege,” for the graphs were of exactly the same length, even group by group. But the Tiren exceeded the other graph in height only at the beginning and end of its course. Also, the effect of the Tireen was steadier than that of Tiren. The latter showed an average rise toward the fourth iambic and then a

sudden descent. This also holds true in all the groups but the last, as appeared from the long Tiren graph.

A comparison of the next two, Unrin and Unreën, showed less difference in the general motor discharge aroused by these two graphs than was evident in the case of the former two. This might be due to a number of things; first, the difference in the structure of the unaccented syllables in these pairs: -un may determine the motor supply as much as the -rin or the -reen. But the differences in the accented vowels are also to be taken into account, for in the one pair, short "e" and long "e" alternated, while in the other, short "i" alternates with long "e." Hence we have two variables, and not one to deal with.

Rinaz and Thira produced the strongest effects of the Rinaz-Rinad-Thira group and they were nearly equal in height and very similar in form; Rinad and Tira were exactly identical in form, but not so close together as were the other two. Evidently "th" and "z" gave the impetus to the responses, and the open "a" was in each case provocative of restraint in the tapping, for the open "a" experiments took longer time to utter than those which closed with "z" or "d." Rinad was found by the subjects to be a rather poor stimulus, while they attributed to Rinaz a sort of hypnotic or lulling character; yet the graphs show that the latter of these sound-combinations was more arousing than the other. But as a general thing indifferent states were correlated *Mean-*wise with a greater motor output than were the pleasant. But Rinaz was the more pleasant of these two.

Comparing Niral and Nidal with one another it appeared that the "r" as an initial accented consonant has a greater motor effect than does initial "d." And yet the "d" can be given a much more explosive vocal character than the "r." But the long "i" in Niral must not be forgotten. Nermal showed very well, especially in the fourth group, the insistent character which was attributed to it in the introspection. Comparing the lengths of these graphs does not seem to throw any light on the matter of correlation, for while the long "i" in Niral might be construed as that factor which gives the length to this graph, yet Nerol is

equally long, but Nerol was the first of this series to be given, and perhaps the subjects took longer to say it because it was something new in the way of utterance.

The next group of graphs showed among other things, the various effects of final "l." It is not surprising that Idrel took a longer time to repeat twenty-five times than did Tirel; even the manipulation of the organs of articulation is a more difficult matter for the former combination; time is about the only noticeable difference between the two graphs, their height being about equal. Undol and Unral were also close quantitative equals, but the slightly greater effect of Unral at the beginning of each group, as was seen from a combination graph, allies this effect of "r" with those noted above. It would seem then, that sometimes articulation force (*e.g.* the explosive character of some consonants) is represented in the tapping in an inverse proportional.

Tirel proceeded more evenly across the page than did any other of this group of graphs, but in the final summation graph, Rinel showed that the average stress on each of the twenty-five iambs was exactly the same. Unfortunately such summation graphs were not duplicated to any but the slightest degree in the longer ones and so their significance is doubtful; one point is to be made, however, and that is that where lack of uniformity between the separate groups of the larger graphs is manifest, all that can be stated about the summation graphs as regards one another is just as significant as that which can be stated about the longer graphs.

In regard to the general effect of final "l" in these combinations, one thing is quite remarkable; and that is the frequency with which the fifth iambic of a group ends with a *descent* in the curve. It is exactly fifty per cent: about ten per cent of the time, also, there is no change from the fourth to the fifth foot. The conclusion seems to be that the tendency of final "l" is to produce its greatest motor effect in some other foot than the fifth, when repeated in the manner employed in the above experiments.

The next five graphs cannot be so strictly compared with one another, but if we consider those having long vowels, it appeared that they extended slightly farther to the right than did the others. The longest, "Ritin," seems to offer some sort of positive correlation in regard to the long "i" in the accented syllable, but by comparing it with those which we have hitherto considered, we find that *"length" of vowel is an equivocal expression in reference to the motor consciousness.*

It is proper now to see whether we can deduce anything from the above twenty-seven experiments by combining them in various groups, for many of them contain exactly the same factors in either the accented or the unaccented syllable.

First: arranging these experiments in the order of greatest to least amount of motor discharge called forth in the tapping we get column I. The figures represent 425 tapped strokes (9 subjects, each one tapping 25 times.) Opposite these numbers are the feeling tone judgments of P, U, and N, with the number of times each judgment was made, regardless of which subjects contributed to that particular mass of judgments.

		P	U	N
Idrel	88.4	5	3	1
Tirel	87.4	8	1	0
Tulin	87.0	6	1	2
Nemal	86.8	3	3	3
Rolen	86.8	6	2	1
Ritin	85.8	3	2	4
Thinal	85.4	1	7	1
Niral	85.4	4	4	1
Thira	84.8	4	3	2
Rinaz	84.8	8	1	0
Aren	84.6	5	1	3
Undol	84.4	7	1	1
Unral	84.4	3	3	3
Rineen	84.0	4	5	0
Unrin	83.6	2	3	4
Nerol	83.6	7	0	2
Tiren	83.6	5	3	1
Unreen	83.4	4	4	1
Ridev	83.2	2	5	2
Rinel	83.0	5	2	2
Rinad	82.8	3	3	3
Nirees	82.8	7	2	0
Nidal	82.4	5	2	2
Tireen	82.0	6	2	1
Tira	81.8	5	1	3
Nilur	81.6	2	5	2
Rileet	80.8	8	1	0

If, now, one arranges these same experiments in two columns, one indicating a decline in the amount of motor discharge, and the other a descent in pleasurability, it can be seen how far, generally speaking, these two factors are correlative. Likewise, taking the motor decline once more and pairing it with another list indicating the decrease in unpleasantness for the same experiments, another set of correlations can be obtained. We have determined the pleasurability and unpleasantness on the following basis: where under the P column or the U column we find the same numbers, *e.g.* P U N and P U N, we have

5 2 2 5 3 1

called the first of these the more pleasant, and the second the more unpleasant; where we find the following situation P U N P U N, we have called the second of them the more

6 1 2 7 1 1

unpleasant. Here, as in the former experiments upon the simple vowels and consonants, we find that pleasantness and amount of motor discharge are inversely correlated. The average divergence of the position of terms in each column is about ten points away from a one to one correspondence. We now come to another interesting phenomenon. Taking those experiments which are nearest alike, and computing from their difference the effect of one vowel over another, or one consonant over another in the motor consciousness, we can draw the following tentative conclusions. To begin with pairs of experiments:

Exp.	M.D.	P	U	N	Here the unaccented "Th"
Thira	84.8	4	3	2	has a greater motor effect than
Tira	81.8	5	1	3	does "T." Also the feeling tone

correlation is strong for the increased motor effect of the unpleasant. Take another:

Exp.	M.D.	P	U	N	Here short "i" accented has
Unrin	83.6	2	3	4	a greater motor effect than does
Unreen	83.4	4	4	1	long "i"; and the same feeling

tone correlation also holds true as with the above.

Again, Exp.	M.D.	P	U	N	In this pair the short
Tiren	83.6	5	3	1	vowel may be credited
Tireen	82.0	6	2	1	with the greater motor

effect.

And in Exp. M.D. P U N We find the accented
 Rinaz 84.8 8 1 0 find "z" as that ele-
 Rinad 82.8 3 3 3 ment which gives the
 greater motor effect. But in both of the above, feeling tone
 inferences are dubious. Let us now compare several groups:

Exp.	M.D.	P	U	N	
Ri-tin	85.8	3	2	4	Here the long accented vowels seem to have the advantage, and the "naz" and "nad" do not contradict the deductions about them made immediately before.
Ri-naz	84.8	8	1	0	
Ri-neen	84.0	4	5	0	
Ri-dev	83.2	2	5	2	
Ri-nel	83.0	5	2	2	
Ri-nad	82.8	3	3	3	
Ri-leet	80.8	8	1	0	

Exp.	M.D.	P	U	N	
Ti-rel	87.4	8	1	0	Here the short vowels again produce, or assist in producing, the greater motor effect. Deductions on the basis of feeling tone are hardly possible in either of these two groups. We make two more comparisons:
Ti-ren	83.6	5	3	1	
Ti-reen	82.0	4	4	1	
Ti-ra	81.8	5	1	3	

Exp.	M.D.	P	U	N	
Ni-rees	82.8	7	2	0	Here the feeling tone enters again as an apparent factor, but rather contradictorily to what has been deduced from the experiments as a whole.
Ni-dal	82.4	5	2	2	
Ni-lur	81.6	2	5	2	

Exp.	M.D.	P	U	N	
Un-ral	84.4	3	3	3	Which completes our groups of this kind.
Un-dol	84.4	7	1	1	
Un-rin	83.6	2	3	4	
Un-reen	83.4	4	4	1	

In connection with the next matter, a few symbols are necessary. Let K₁ = the accented consonant which stands immediately before the accented vowel, thus: ni-Ral, un-Dol, etc. Let also K₂ = that accented consonant standing immediately after the accented vowel, or which closes the accented syllable.

thus: ni-raL, ri-niZ, etc. Let also V stand for the accented vowel, and let the letters i, e, a, etc., represent the short vowels, and the letters I, E, A, etc., represent the long vowels. Let also the symbol > stand for the "greater than" and the symbol < stand for "less than." Then, in general, taking all the commonly used accented final and initial consonants of the accented syllables, we have:

$$K_2L > K_2N = 17.4 > 15.5$$

$$Ve > VE = 17.3 > 12.0$$

$$Va > VA = 17.5 > 12.8$$

$$\text{and } K_1L > K_1R > K_1N = 17.0 > 14.0 > 13.6,$$

so that the motor effect of $K_1N <$ the motor effect of K_1R and also $<$ that of K_1L . We can say, then, that of the consonants, K_1L and K_2L have the greatest motor effect, and the short vowels also, in general, rank with them. This is significant, and shall be used later in the correlation of large masses of sounds.

3. TRANSMOGRIFICATIONS OF ENGLISH POETRY

This part of the work includes several kinds of experiments. According to the plan outlined in the early paragraphs, we are now to consider the effect of more complicated collocations of the speech elements than have been hitherto employed. The poets themselves are experimented upon from now on, and in a two-fold manner: first, by casting into decasyllabic lines the sounds as they appear in the tabulations of frequency, and then by employing both single lines and ten-line passages from poetry to show the various effects of combination with and without meaning.

THE EFFECT OF MEANINGLESS SINGLE LINES FROM THE TABLES OF SOUND FREQUENCY

We now turn to the first of these experiments. They were devised to show the psycho-motor effect of those sounds which certain poets use most frequently as well as those which they use less frequently, and in each case they attempted to illustrate the proportion of use by the frequency of repetition in the

decasyllabic line into which they were arranged. Taking four of the poets from the tables of sound frequency hitherto mentioned, we find them using the sounds of the language in the following proportions. We take only the first twenty or more sounds from each one, accented and unaccented alike. (Short vowels, small letters; long ones, large.)

Acc.	Keats		Byron		Tennyson		Arnold	
	Unacc.	Acc.	Unacc.	Acc.	Unacc.	Acc.	Unacc.	
R	3.9 u	5.3:R	5.3 u	5.3:R	5.0 u	5.3:R	5.5 u	4.0
N	3.7 i	4.1:N	4.0 N	4.5:L	4.5 i	4.0:N	4.6 i	3.4
L	3.5 N	3.5:L	3.5 i	3.4:T	3.1 N	3.2:L	3.6 N	3.0
S	3.4 R	3.1:E	2.9 R	2.7:M	2.7:Th	2.9:T	3.3 a	2.9
T	3.1 T	2.3:D	2.5 T	3.1:S	2.6 R	2.8:S	3.1 D	2.6
D	2.5 S	2.0:M	2.4 Th	2.3:E	2.5 D	2.1:E	2.3 Th	2.5
e	2.1 Th	1.9:S	2.4 e	1.7:a	2.3 a	1.8:A	2.2 L	2.1
V	2.0 e	1.7:T	2.2 Z	1.7:I	2.1 L	1.8:D	2.2 Z	1.8
u	1.9 O	1.7:O	2.1 O	1.6:D	2.0 Z	1.8:O	2.1 R	1.7
E	1.8 L	1.6:A	1.8 D	1.6:N	2.0 T	1.8:F	2.1 E	1.7
P	1.7 a	1.5:u	1.8 a	1.5:Z	1.9 Th*	1.6:I	2.0 T	1.6
M	1.9 D	1.5:I	1.6 I	1.4:P	1.9 Ng	1.0:M	1.8 e	1.4
A	1.7 K	1.4:e	1.5 E	1.4:i	1.8 W	1.0:o	1.5 H	1.4
F	1.7 Ng	1.1:a	1.4 Th*	1.3:B	1.7 E	.9:e	1.4 Th*	1.3
i	1.6 Th*	1.1:i	1.4 o	1.2:o	1.6 I	.9:K	1.4 W	1.3
K	1.6 M	1.0:P	1.4 L	1.2:F	1.4 e	.8:W	1.3 S	1.1
H	1.4 W	1.0:K	1.2 S	1.0:W	1.4 e	.7:Z	1.3 B	1.0
I	1.3 E	.9:o	1.1 M	.9:u	1.3 O	.7:i	1.2 A	.9
O	1.3 F	.8:F	1.1 W	.8:K	1.3 H	.7:u	1.2 K	.7
W	1.2 V	.8:V	1.3 B	.8:e	1.2 OO	.6:a	1.0 Ng	.7
Z	1.2 Z	.7:Th	1.1 P	.8:O	1.2 K	.6:B	1.0	
				:G	1.2 St	.6:P	1.0	
						:St	1.0	

* Surd.

All but one of these poets, Byron, employs his sounds approximately in the ratio of ten accented to eight unaccented ones. Byron uses nine unaccented ones to every ten accented ones. We took the first seven accented consonants and the first three accented vowels, and the first five unaccented consonants and the first three unaccented vowels from the other three poets; in Byron's case the same number of accented sounds were used, but six unaccented consonants and three unaccented vowels were taken to make his 10:9 proportion. Taking then, ten accented sounds from each of these poets, and the proportionate number of unaccented sounds, and arranging them in a line of five iambs with the

most used sounds in the more prominent places in the line, namely the first and the last feet, we obtain the following four experiments:

Keats: Nĩ rŭl sũ vēēd rĩ nēst یت ěl یتh rēēn.

Byron: Zũ rēēn tĩth nōle đēr tŭll thĩn sōde tũ rēēm.

Tennyson: Thũ rǎl dĩ rēēt thũ nĩme rǎ dēēs ũn tǎl.

Arnold: Thũ rēēn đĩn lāse zǎ fōde nĩl tāne thũ lēēr.

The 10:8 and the 10:9 proportions are fairly well kept in these combinations, as can be proven by counting the sounds. The arrangement of sounds is quite arbitrary, but in making such a combination, there are many things to be considered besides mere proportion. For example, one must test by means of his ear what combinations are suitable to follow one another; and when strict proportion cannot be followed, one must be judicious.

If, now, we take the next ten accented sounds in the lists, and combine with them the proper number of the next eight or nine unaccented sounds, we have the following experiments for these poets:

Keats: Lǎ mǎz mō kĩf dē pōth lǎ pām wē hō.

Byron: Dō thǎv lō pĩ sē kē đǎ thĩp lĩ vǎ.

Tennyson: Tē pōz lĩ bĩn ěng wō zĩte nũf wē nōp.

Arnold: Rē mōz ět wĩk sǎ thũ wē zēm hē mĩ.

(It must be noticed that when the appearance of the list indicates that more than three vowels to seven consonants are the materials out of which the experiment would normally be made, we have followed the list, rather than discarded the prominent elements in it.)

These experiments were presented in pairs: first the two representing Keats, and after that the others in the order in which they occur above, finishing each poet as we proceeded. Each line was tapped and recited five times. The instructions were to read the line over until it became easy to read; but not to wait until associations came up; then the tapping and reciting went on as it had done before, the instructions again being to

"tap at each accented syllable." A few of the subjects practised reading the lines over once silently and tapping at the same time, in order that full preparation for the experiment would not be lacking. The idea was to get the full effect of the line *while tapping*, in order that the motor manifestations could be called representative in the fullest sense of the word. The introspection which they gave for these experiments follows:

Keats I. (The first ten accented and the first eight unaccented sounds.)

Visual imagery of maples and other deciduous trees in the fall of the year; the air is frosty and the whole scene is grayish; very objective.

The rhythm feels like dance music; very contagious; thought of evening bells; very romantic. Gentle thing; not much action to it; a little Spencerian; deals with pastoral things.

Keats II. (The second group of Keats' most used sounds.)

Visual and auditory imagery of the surf; also of fields; sounds go well together. Seems short and broken up; compares it to an "Italian" salad. Brings imagery of the sea; thinks of something like the "Ancient Mariner." Not very active.

Byron I.

Something superficial about it; kinaesthetic and visual imagery of idly following things about. Minor, meditative and solemn; attributes this to the long vowels; thinks of something like "Crossing the Bar." An epic, recounting adventures. Deep and funereal.

Byron II.

Active thing; relates to strength and power and brawny arms. Sounded like the speech of a big-chested, half-civilized people living in a cold climate. Rather dramatic and superficial.

Tennyson I.

Inactive; sad throughout; subjective thing. Evokes pity and compassion; elegiac thing; sorrow, not wild, but quiet and domestic implied. Very rhythmic; gives a feeling of solemnity felt in the midst of happy surroundings. Images of a quiet, restful woodland scene. Asked if it was Tennyson. Just a little dramatic; not quite sincere. A little tragic and sorrowful; thinks of fate.

Tennyson II.

Rhythmical and dramatic; sounds Shakespearian. Kinaesthetic imagery of many motions in different directions. Makes him short of breath; thinks of dying gladiators; mentioned "Heldenleben." An explanation in some dialect of an unfortunate event.

Arnold I.

Something strong and military about it. Rhythmic, but not very deep. Medieval and romantic; seems to be a description. Thinks of the "Meister-singers"; active and objective.

Arnold II.

Unpleasant taste images aroused; something psycho-pathic about it. Interesting, but not emotional; full of irritating things; more like conversation than anything else. Leaves one a little gasping; sounds like some one timidly trying to recite.

It must be remembered that these combinations were presented to the subjects without their knowing what poets they represented; nor was it a guessing contest, either; no stress was laid upon anything but a good reading of the lines.

There follows the correlation between feeling tone and motor discharge, in general, for all the subjects in the above experiments.

Subject				
A. M.....	94.3	91.0	104.9	N P U
M.V.	2.1	2.0	3.4	N P U
Rnj.	10	12	16	N U P
B. (as above).....	74.0	—	72.2	P N
	3.4	—	3.6	N P
	15	—	17	N P
C. " "	92.5	—	82.0	P N
	3.6	—	5.5	N P
	19	—	27	N P
F. " "	73.5	75.7	—	U P
	3.6	3.8	—	U P
	17	18	—	U P
L. " "	72.0	74.8	—	U P
	3.2	2.7	—	P U
	18	15	—	P U
N. " "	85.2	84.6	—	P U
	2.4	2.0	—	P U
	12	11	—	P U
T. " "	75.3	77.4	—	U P
	3.0	3.7	—	U P
	20	13	—	P U
W. " "	87.8	85.9	91.2	N P U
	2.3	2.5	2.4	U N P
	10	13	12	U N P
Y. " "	39.9	41.9	52.0	N U P
	2.3	3.2	3.0	U N P
	15	17	19	N U P

Comparing these results with all similar correlations, we find that none of the subjects have remained constant during the experiments, as is evidenced by the following. A.'s longest tappings were twice for the pleasant, twice for the unpleasant and twice for the neutral. B.'s were three times for the pleasant, twice for the unpleasant, and once for the neutral, etc. The whole list is given below.

A.	P 2	U 2	N 2	
B.	P 3	U 1	N 2	
C.	P 1	U 2	N 2	(this subject did not begin with the others)
F.	P 3	U 1	N 2	
L.	P 0	U 3	N 3	
N.	P 3	U 1	N 1	(only one kind of judgment was made in one series)
T.	P 4	U 2	N 0	
W.	P 2	U 1	N 2	(only one kind of judgment was made in one series)
Y.	P 1	U 1	N 1	(this subject did not begin with the others, and in one series made only one kind of judgments)
	<hr/> 19	<hr/> 14	<hr/> 15	

Comparing the M.V. and the Rnj. in a similar manner gives equally varied results.

Let us now consider the graphs for these experiments. Their resemblances were more striking than their differences, and it is not unlikely that the reverberations set up in the motor consciousness just by this new material were too strong to be altered by the other factors involved. In the first place it was found that these experiments produced a much lower motor output than did those preceding them; Idrel had reached a height of 88.4 mm.; none of these reach anything above 78.8 mm. (the Arnold II). Massing together the P, U, and N judgments, and correlating them with the amounts of motor discharge as shown by these graphs, we obtain:

		P	U	N
Arnold II.	M.D. av. 78.8 mm.	3	4	2
Tennyson I.	78.6	7	1	1
Byron II.	78.4	8	0	1
Tennyson II.	78.0	4	4	1
Arnold I.	77.6	4	4	1
Byron I.	77.4	5	4	0
Keats II.	77.2	7	1	1
Keats I.	76.6	7	1	1

This comparison would seem to indicate that the most pleasant experiments go with the strongest *and* the weakest tappings,

while the most unpleasant tend to produce those which are midway between the greatest and the least. No correlation with the feelings of activity adds anything to what we have already observed, that sometimes, and sometimes only, the feeling of ease means longer finger strokes, and feelings of inhibition and difficulty mean shorter strokes.

None of the experiments numbered III are given for any of the poets, and hereafter only No. I of the first three is presented, because it represents better than do the others the differences in sound frequency for the poet. No. III, however, was constructed by combining half of the first and half of the second of the above groups of sounds, Nos. I and II, to illustrate the poet's use of those elements which are not either very frequent nor infrequent, and thus we had a set of three experiments which began with the liquids, and ended with the gutturals and fricatives.

Experiments numbered IV, V, and VI in the case of each poet are transmogrifications of single lines of poetry; experiments VII, VIII, and IX are lines of poetry rearranged, keeping the accented words of the original accented, and the unaccented words unaccented. Experiments X, XI, and XII are lines of poetry, "clothed and in their right mind," and for each of the twenty poets experimented upon there are these twelve experiments, which start with the tonal elements, pass through the nonsense verse of transmogrification, through also the ungrammatical poetry into the normal meaningful lines from which all had been ultimately derived that preceded them. Thus we had 240 experiments upon the poets arranged in such a way as to supply sufficient data for a compact thesis in itself. In the case of nine of the twenty poets, the same material entered into the transmogrifications, rearrangements and the meaningful lines, so that Experiments IV, V, and VI were the transmogrifications of the material in Experiments X, XI and XII respectively, and Experiments VII, VIII and IX were the rearrangements of the same material as had been used in the others. In the other eleven poets, which were experimented upon first,

only now and then was such a symmetrical arrangement carried out. We decided upon the method of repeating the same sounds in three different relations in order to exactly determine what both grammar and meaning had to do with the effects of the poetic line upon the motor and the introspective consciousness. But only in the case of four of the poets do we give the experiments from IV to XII. What has been omitted will be considered in later, summarizing paragraphs.

It was the purpose at first to obtain experimental material from the poets which would duplicate in tonal quality Experiments I, II, and III; after a futile search for such lines, it was given up; too many extraneous elements entered into the matter. In the first place, very few lines of the poet actually follow the tonal pattern of the first experiment in our series, and those that do, usually contain inverted iambics, if they contain iambics at all; in the second place, to find decasyllabic lines that are regularly accented in the iambic pattern is not always possible, and when such are found, they are likely to be some of the poorest lines, esthetically considered, that the poet suffered to leave his pen. So that in every case almost, Experiments I, II, and III are in a class by themselves, and the other nine experiments of the series are in another class.

THE TRANSMOGRIFICATION OF SINGLE LINES OF POETRY

The business of transmogrifying single lines of poetry is quite difficult, for the restrictions placed upon one in this work are very rigid,—words must not be made, and sounds must not be left out; again, to avoid making words, one may have to construct a very badly sounding line, and sacrifice to the purpose of the experiment much of his artistic predilection for the beauty of tones; let any one who doubts the difficulty of the matter attempt the task, and he will find that we have but lightly touched on the obstacles to be encountered. The transmogrifications we have used in this experiment are but a fifth of the number which we attempted to complete with satisfaction to ourselves.

These experiments were presented in the order in which they are given in the following pages. The first four poets represent

mented upon were Byron, Keats, Arnold and Tennyson, and the forty-eight experiments upon them were well under way before the I and E Experiments had been completed. This was a relief to the subjects, who rather chafed at the idea of having so many similar combinations week after week.

There were fifteen experiments performed on Keats, but we have omitted all but twelve in our consideration, because we discovered that the others were of no importance for the work in hand. Our original plan had been to experiment upon very many more than twelve decasyllabic lines for each poet, but time did not allow, and this had to be given up. Neither were the subjects able to react to twenty experiments an hour as well as to twelve.

The introspection for the Keats and Byron experiments is very interesting indeed, from the very start, and the transmogrification of the famous line from Byron's "Apostrophe to the Ocean" was a decided success, but it is not beyond cavil that the line may have contained too many hints at the sense of the original verse.

After the introspective account of the matter, we have given the numerical results which we shall consider at once.

Keats IV. Wõn fěmz ē nānj öv dī nāl těr ěn tēēv.,* transmogrified from "One faint, eternal eventide of gems."

Nothing in the introspection compared to the original meaning or mood of the line.

Keats V. Tōō zowb ĭth bīnd thă răwl mě tǐng wǐn tī. From "To bind them all about with tiny rings." Introspection.

Visual imagery of a farm; auditory imagery of some bells ringing. Sounds like an ode; vocal placement seemed forward in the mouth. Gives a light and cool effect; the sounds vary in pitch very much; more resonant than the former one.

* From now on the diacritical marks will have their usual significance over single vowels; but when two vowels occur together, the following interpretation is required: ēē = ē; öö = the oo in "brook"; öō = the oo in "food", etc; a as in "ask" is indicated by no mark at all; in addition to this, the ow, aw, ou, oi, and other diphthongal sounds are pronounced as usual in English. Differences between the sonant and surd th, are not indicated here, though they were in the experiment.

Keats VI. A thûrn ūs tōō thŭ bow tōō flāndry bīde. From "A flow'ry band to bind us to the earth."

Very rhythmic line; Chaucer's poetry came to mind. Imagery of a high tower; romantic scene, maybe some battle being narrated; romantic and idyllic. "Flaundry" a strange word; thinks of "Flanders" and "laundry" at the same time.

Keats. Experiment VII. So cooling very still was and the air. From "The air was cooling and so very still."

Imagery of the twilight. Visual imagery of a bright green color. Cool sensation in the mouth. Very conscious of lips.

Keats VIII. The dwindled of its trace and edgings brim. From "The trace and dwindled edgings of its brim."

Imagery of snow; "trace" a "cold" word. Feels lips to be very active.

Keats IX. Across the move would blue a little cloud. From "A little cloud would move across the blue."

Gives an inane feeling; "move" and "blue" are "sticky" words; "cloud" doesn't go with them. Cannot say "little cloud" fast enough to suit the line. The word "across" is too hard; the "k" sound sticks. Lips rather prominent in consciousness.

Keats X. A bower quiet for us and a sleep.

Odd that the guttural should have been used,—"quiet." "Bower" the only "quiet" sound. Uses too much breath for the meaning.

Keats XI. "With lucent syrops tinct with cinnamon."

Thinks of toddy and cordials; just the opposite kind of a scene came up. Much taste imagery; visual imagery in bright colors. The line hisses too much; imagery of peppermint. Tickles the tongue.

Keats XII. "That broadest o'er the troubled sea of mind."

Mind in a quiet uncertainty. Thought of Byron, and Hamlet; visualized a cliff. Feeling of a cosmic melancholy. Gives a gentle melancholy; "mind" too abrupt.

Byron IV. Shŭn dōle ow rōd thŭ nārķ blōre ō lānd ēēp. From "Roll on thou dark and deep blue ocean, roll."

Visual imagery of some one on a rock by the sea; sounded like a foreign language. Subject A.—Imagery of the ocean, in a storm; the jerky effect here is justifiable. B.—Imagery of a ship on the ocean in stormy weather; seemed to take more force. D.—Imagery of the sea; heavy waves; dark colors. F.—Sea imagery; "nark" is very hard. "Blōre o" is the blowing of a horn. K.—Great deal of resonance; no imagery. L.—Sea concepts aroused; thinks of Norsemen, etc.; very thrilling. M.—Feels the roar of the ocean; visual imagery very rich. P.—Counting bothered a little; imagery of Hō-

land. S.—Mixture of Persian, Arabic and Hindustani language; great deal of imagery of the ocean; sounds fill the mouth.

Byron V. *Thũ vī lānd sã nĩl jĩ tã frõ the vëë.* From "The inviolate island of the sage and free" (with two unaccented syllables omitted).

Narrative poetry; thinks of natural scenes. A gesture could do it all better than words. Imagery of gaudy colors; not smooth sounds. Not very smooth; especially the fourth iambic; imagery of a pastoral scene. Imagery of some big man saying this in a thunderous voice. Lip sensations unpleasant.

Byron VI. *Hãs mowd stĩ rĩne whã stësh rã mĩd thũ krãthe.* From "Amidst this wreck where thou hast made a shrine."

Describing a very interesting place; something troubled and dolorous about it. Implies a rough activity. All the sounds very unpleasant; do not fuse. Too staccato; but "su krath" very good. Just a strange, incomprehensible language.

Byron VII. "With stirred as rose her dream leaves with the air." From "Stirred with her dream as rose leaves with the air."

Ethereal quality about the sounds. Rhythmic; thought of a sleeping girl. The words "air" and "stirred" the best of all; olfactory imagery.

Byron VIII. *At bluelit moon and midnight on the deep.* From "At midnight on the blue and moonlit deep."

Imagery of a ship at midnight; pleasant self-feelings. Smooth, soft, and gentle combination.

Byron IX. *Upon beheld decline who hath my brow.* From "Who hath beheld decline upon my brow."

Thinks of a pessimistic old man. Monotonous; takes too much breath. Thinks of some unsuccessful person.

Byron X. "And temple more divinely desolate."

"Emp" sounds pointed. "Desolate" just suits the meaning; it's a cold, blue word; "divinely" doesn't have anything to do with the line. The "hiss" in "desolate" very expressive.

Byron XI. "Yet I was born where men are proud to be."

Prosaic and pessimistic; sounds very resonant. Very strong physical sensations. "Yet" always a vocal surprise.

Byron XII. "And silent rows the songless gondolier."

Very dark and somber feeling induced. "Silent" the only silent word in the line; "gon" in "gondolier" is too guttural. Cheerful and lively line in spite of the intended meaning; "songless gondolier" a vocal equivocation.

The remarkable thing about the graphs representing these twelve experiments upon Byron and Keats was that in nearly every case the motor discharge for the Byron Experiments is greater than that for Keats, and the final averages showed that the combined finger movements for the Byron experiments were eighteen metres longer than they were for Keats. But we must not be too sanguine; the tonal elements in Byron are not the ones which have hitherto been those arousing the strongest reactions, and the fact that Keats was experimented upon first may indicate that we have only practise curves before us in this instance. But there was something rousing and enthusing in all the Byron experiments which the subjects did not find in those on Keats, which may account for the matter more exactly. The character of the lines transmogrified and employed in other ways is very different for the two poets, as any one can see from a glance at the material; but whatever may be the nature of the individual sounds used in the above experiments, we find here that order and arrangement are potent factors, and single decasyllabic lines may produce different effects than do larger passages. One could also notice in these graphs the form quality of the decasyllabic groups,—the regular thing in a rhythmic line is for the first and the last feet to demand more motor discharge than the intermediate iambs. And the form-quality of the Byron lines always differed from those of Keats. Again, the mean variation of the tappings for the meaningful lines is less, according to the graphs, than it is in the first six of the experiments for these poets.

Every one will admit that Byron is a more oratorical poet than Keats, and that there is a power and vigor to Byron's poetry which is not found in any other English poet. It would seem that the temperamental character of the poet had gotten into these experiments, and that also the quieter and more pastoral nature of Keats had not been omitted from the experiments which we graphed with those of Byron. It is admitted that the experiments were made with the different poets in mind, and that even the first three experiments were patterned upon what was

conceived to be the poet's tonal characteristics, but the results are what they are, and whether from one cause or another, they show that the experiments on Byron called for the more activity on the part of the motor consciousness. This is enough.

The next experiments to be considered and compared are those on Arnold and Tennyson.

Arnold IV. Nōr hī būt smōrd hīz nēērd ūkt lōd ōrn wābez.
From "But he looked on, nor smiled, nor bared his sword."

Thinks of a battle; (but "smord" never meant "sword" consciously).
"Smord" and "neerd" indicate the presence of trouble.

Arnold V. Lō mōrdz ūm stūr dīle hūs kēr mīze hīz māfe.
From "Like some old miser Rustum hoards his fame."

Tragic narrative poetry.

"Hus" the highest tone in the line.

"Mafe" not very pleasant; the auditory consciousness much more pleasant than the reading consciousness.

Arnold VI. Zī nōld bē krēm ēs lāj ē rūld shō hīze. From
"Behold, she cries, 'so many rages lulled.'"

"Z" the prominent consonant.

"Kremlin" aroused by "krem."

Seemed to be a "begging symphony" of the Mohammedan beggars.

Arnold VII. As she her echo stormy screams sails by. From
"Echo her stormy scream as she sails by."

"S" the right sound for this meaning.

Imagery of excited movement.

The "s" sounds are very unpleasant; sounds like a poorly oiled wheel.

"Sails" too heavy a word for an unaccented syllable.

Arnold VIII. All who pained desert lion some of day. From
"Of some pain'd desert lion who all day."

Hard to say "pained" in the time allowed.

The words "some" and "desert" disturbed the rhythm.

Arnold IX. Her it the glass lake flying over shall. From
"Shall the lake glass her, flying over it."

Sounds very pleasant; gives a "thin," damp feeling.

Lips much in consciousness.

Mood aroused akin to that of Wordsworth's Lucy poems.

Arnold X. "The sails that gleam a moment and are gone."

"Can this be the same poet as the last nine experiments illustrated?"

Liked the sense but not the sounds or rhythm.

Arnold XI. "Before this strange disease of modern life."

Sounds unpleasant per se; "s" too frequent.

"Strange," very unpleasant and nasal; the "ern" of modern also a bad sound.

Arnold XII. "He lies in death upon the common sand."

"Death" as a sound is very pleasant here; one cannot expand while saying it.

The words "common sand" go too quickly for the meaning.

Tennyson IV. Fōr thārīng kērn hē tōft ā sēēfly thīle. From
"He therefore, turning softly like a thief."

Thinks of the ocean and the surf; "a cynical line."

Means something soft, quiet, and subdued; "kern" is a lovely word.

The letter "k," while dissonant, only brings out the harmony of the other sounds the more.

Tennyson V. A whīl īn dōst ūr gēnward nīspērs ēēp. From
"Again in deeper inward whispers 'lost'."

Thinks of a soft, subdued scene; evening.

Thinks of whirling dust; whispers; deep; etc.

Thinks of Gray's *Elegy*.

Tennyson VI. Whār lāmz tōō ūrkīng wīle tōō bīsk ān vōrn.
From "Scorning an alms to work whereby to live."

"Vorn" is Scandanavian; thinks of the early Britons.

"Lamz" = "lambs," but they were not frisky; something dark and wearisome about the line.

"Urk" brings a dead stop.

Sounds like Matthew Arnold; "urk" a little hard.

Very personal, and sad; gloomy melancholy; fatalistic.

Implies a moral situation.

Tennyson VII. But will made fate in weak by time and strong.
From "Made weak by time and fate but strong in will."

Uninteresting, abstract, philosophical.

The sounds too short for the meaning.

The sense takes away the pleasure of the sounds.

Tennyson VIII. Now crimson sleeps the now the petal white.
From "Now sleeps the crimson petal, now the white."

Imagery of carnations, visual and olfactory.

"Crimson" visualized as "pink."

The sounds are good even without meaning.

The sounds of the word "petal" are too light for the rest of the line.

"Crimson" the best sound of all; thinks of English pudding.

Tennyson IX. To scorning live whereby an alms to work.
From "Scorning an alms to work whereby to live."

Stupid sort of a line; "scorning" a dreadfully "hard" word.

Laborious combination; has internal bodily strains.

Didn't feel that the tapping was at all expressive.

Tennyson X. "And on the mere the wailing died away."

Rather onomatopoeitic; but "died" has a little too much movement about it.

Tried to be sad, but did not succeed.

Tennyson XI. "The silent water slipping from the hills."

The word "silent" means distance and seclusion.

"Sliding" would have been better than "slipping" for onomatopoeia; the short "i" is too full of impact.

Too much sound for the sense.

Sounds slightly artificial and banal.

"Slipping" is most annoying; gets no motion out of the line.

Tennyson XII. "And all the coverlid was cloth of gold."

"Coverlid" strangely pleasant for a "k" sound.

Nasal; "cloth of gold" pleasant to say; *but got visual imagery of a scarlet cloth.*

"Gold" the conspicuous word.

Mouth seems very open in the last two feet.

"Lid" doesn't fit in.

Something "insincere" about it.

As typical of the numerical results obtained from the experiments numbered I to XII, we present those for Tennyson. First a table showing the results from the mean of the tappings for each subject arranged both according to feeling-tone and also to the triadic grouping of the experiments as outlined above. In the first column stand the abbreviations for the names of the subjects, and in the last column the averages of the tappings for the pleasant, unpleasant and neutral experiments, regardless of group.

The next table presents the same results quâ mean variation. And the third table shows the rank lists which exhibit the acme of the steadiness attained in this whole group of 240 experiments. The mean alone appears to be significant in point of comparison with the general psycho-motor correlations hitherto obtained.

TENNYSON, EXPERIMENTS I-XII. MEAN

Subject	I-III	IV-VI	VII-IX	X-XII	Av.
A. P.	86.3	81.6	87.4	90.0	86.3
U.	—	87.6	89.6	87.7	88.3
N.	—	—	93.5	—	93.5
B. P.	75.8	78.5	—	78.8	77.7
U.	80.1	75.8	79.1	—	78.3
N.	83.8	—	—	—	83.8

C. P.	91.8	87.9	83.5	76.6	84.9
U.	—	—	87.3	—	87.3
N.	—	87.9	—	—	87.9
D. P.	103.8	104.4	102.6	106.1	104.2
U.	98.8	—	96.0	97.9	97.6
N.	—	—	96.8	—	96.8
F. P.	82.0	79.9	83.0	80.5	81.4
U.	77.2	73.0	79.6	—	76.6
N.	—	—	77.0	80.8	78.9
K. P.	56.1	47.9	—	50.8	51.6
U.	—	—	47.4	49.1	48.2
N.	—	—	52.4	—	52.4
L. P.	69.3	67.3	77.1	78.5	73.1
U.	70.2	—	80.9	83.5	78.2
N.	—	—	69.0	—	69.0
M. P.	85.0	87.8	—	—	86.4
U.	85.2	80.1	90.6	85.7	85.4
N.	—	85.7	—	88.8	87.2
P. P.	127.0	131.2	131.7	134.1	131.0
U.	127.9	—	—	—	127.9
N.	—	132.4	—	—	132.4
S. P.	—	113.5	111.3	111.2	112.0
U.	115.6	—	—	—	115.6
N.	—	—	—	—	—

TENNYSON, EXPERIMENTS I-XII. M.V.

Subject	I-III	IV-VI	VII-IX	X-XII	Av.
A. P.	3.7	2.3	4.7	3.8	3.6
U.	—	4.1	3.3	3.9	3.9
N.	—	—	3.4	—	3.4
B. P.	3.9	4.3	—	4.4	4.2
U.	7.1	4.4	5.8	—	5.7
N.	4.5	—	—	—	4.5
C. P.	4.0	4.7	4.6	4.2	4.3
U.	—	—	7.1	—	7.1
N.	—	3.8	—	—	3.8
D. P.	3.8	1.7	1.4	2.0	2.2
U.	3.2	—	4.6	2.8	3.5
N.	—	—	5.6	5.6	5.6
F. P.	4.5	6.0	5.4	4.6	5.1
U.	3.7	4.5	6.1	—	4.7
N.	—	—	4.3	7.2	5.7
K. P.	6.2	5.3	—	6.0	5.8
U.	—	—	6.2	5.5	5.9
N.	—	—	5.7	—	5.7

L. P.	5.1	4.1	4.8	3.7	4.4
U.	3.4	—	3.7	3.6	3.6
N.	—	—	5.1	—	5.1
M. P.	2.4	5.0	—	—	3.7
U.	3.9	4.8	3.3	5.4	4.3
N.	—	4.1	—	4.1	4.1
P. P.	1.7	2.0	1.6	2.0	1.8
U.	2.4	—	—	—	2.4
N.	—	2.6	—	—	2.6
S. P.	—	2.0	3.1	2.6	2.6
U.	—	—	—	—	—
N.	2.8	—	—	—	2.8

TENNYSON. RANK LISTS. EXPERIMENTS I-XII.

Subject	Mean											
A.	e	g	f	d	e	g	g	f	f	g	g	g
B.	c	d	c	e	d	d	c	d	b	e	d	b
C.	g	f	g	g	f	f	e	e	e	d	b	b
D.	h (throughout)											
F.	d	c	d	f	c	c	d	b	d	b	e	e
K.	a (throughout)											
L.	b (throughout)											
M.	f	f	e	c	g	c	f	g	g	e	f	f
P.	j (throughout)											
S.	i (throughout)											

MV.												
A.	d	f	e	g	c	e	b	g	e	f	d	h
B.	h	i	g	e	g	f	e	j	h	j	d	d
C.	b	h	j	f	a	i	j	b	d	d	g	a
D.	e	c	d	a	a	a	j	a	f	a	a	i
F.	g	g	f	j	j	g	h	f	i	h	i	g
K.	j	j	i	h	i	j	f	i	j	e	j	j
L.	i	d	h	d	c	i	g	e	g	g	e	e
M.	f	e	b	i	f	h	c	c	c	i	h	f
P.	a	a	a	c	d	b	a	b	a	b	b	a
S.	c	b	c	b	b	c	d	d	b	c	c	b

Rnj.												
A.	e	g	g	d	b	f	b	f	d	i	d	i
B.	f	j	e	e	f	g	d	g	g	j	g	b
C.	b	i	f	h	h	c	j	h	e	f	f	g
D.	a	b	b	c	a	b	g	b	j	a	b	c
F.	g	h	h	g	i	i	f	d	h	a	j	h
K.	j	f	j	i	j	j	e	j	i	d	i	j
L.	i	d	i	f	d	d	i	e	f	c	c	f
M.	h	e	d	j	e	h	c	c	c	h	h	e
P.	c	a	a	a	g	a	a	a	a	b	a	a
S.	d	c	c	b	c	e	h	i	b	e	e	d

In the experiments upon Byron and Keats, subject A. was the only one who gave longer tapplings for the pleasant than for the unpleasant lines; D., L., and P. do the opposites in both cases;

all the rest of the subjects are apparently inconsistent. In the experiments upon Arnold and Tennyson subjects B. and C. give the longer tapplings to the pleasant lines; D., K., M., and P. do the opposite, and the rest of the subjects are again self-contradictory, to a more or less degree. The apparent conclusion is,—new material, new motor manifestations.

Both of the rank lists for the mean (Arnold and Tennyson) are splendid,—indeed, they are rarely ever so consistent thereafter. But the rank lists for the mean variation and the range are not as consistent as they have been before.

From the graphs for this work it was noticed that in every case, the Tennyson experiments took a shorter time to be spoken than did those of Arnold. Also in graphs X to XII appeared much more form quality in the Tennyson than in the Arnold graphs. Even in the final average for all twelve experiments this character is well defined. Again it is concluded that there is something about the sounds or the arrangement of the sounds in the Tennyson experiments which caused the fingers of the ten subjects to move eighteen metres more while tapping the Tennyson experiments than while tapping those for Arnold.

We find also that characteristic feeling-tones, moods and the like are correlated with the type of motor discharge which accompany them. Where one is individual and significant, so is the other. Not only were the Byron graphs indicative of a greater length of tapped strokes than those of Keats, and the Tennyson than the Arnold, but also the general appearance, the steadiness, the form of the graph began at least at the tenth experiment on these poets, and very frequently before this, to take on an individuality as drawn on paper, as much as did the content and quality of the lines as spoken and understood. So that the motor expression we had used, namely the tapping movements of the right index finger, appeared to be not only one that tended toward automatism, but also one that drained away the general somatic supply in a manner characteristic to the poet and fitting to the purpose in hand. A fair analogy to this is found in the case of singers sympathetically accompanying

themselves upon the piano; where the two forms of musical expression are identical, at least functionally, in so many ways as to be regarded as indicative of that unity of the esthetic consciousness which in this particular instance expresses itself in blends of behavior.

To take up each poet or each pair of poets who were experimented upon would be too tedious, and so we shall now turn to a consideration of the rest of these 240 experiments only insofar as they supply data for the main thesis involved in our problem. At the end of the series there appear two very interesting tables, which summarize and focus the matter of the correlations in a very interesting way. One generalization is not out of order in this connection,—the lyrical poets, as every literary man well knows, have employed a phonetic device which the other poets have not; name the lyric poets, and you name the users of liquid sounds in their poetry; name again the lyric poets, and you have named those not only whose lines transmogrify best, but also those who will produce in these tappings, as recorded in the graphs, the finer form quality of the curve of the motor discharge.

At the end of the introspective and other reports of the experiments upon these remaining sixteen poets, we shall consider the graphs for all of them.

Coleridge I. *Thũ nāse tă nēēl rĩ tām đĩn rāde thũ lēē.*

Reminds him of the sound of cymbals.

Sensations cluster around "n" and "s."

Very highly colored sounds.

"E" is conspicuous; imagery of the sea.

Stays in the mouth.

Consonants more prominent than the vowels.

Coleridge IV. *Rũ thăđ thă bāng tĩs lēēt stā rűsk tĩ pā.* From "Beat its straight path along the dusky air."

Visual imagery of rustic scenery; simple concept.

Sounds run back and forth in the mouth.

Hard to say "sta rusk." Rather quiet sounds otherwise.

Sounds like water gushing and then flowing smoothly.

Something Russian about it; more epic than lyric.

Coleridge V. *Rĩ vōze kā strōn sōv mōrst bā făđd thũ wāme.* From "A storm of waves breaks foamy on the strand."

Has a funereal air; imagery of scenes of death.

"Wame" means country life.
 Images somebody laboring under a burden.
 Felt as if standing at bay, in desperation.
 Forceful; hard sounds, but not unpleasant.
 Sounds like a strong rebuke.

Coleridge VI. Thũ zũrv lēs nōōm äll blēthēr wīldēn fōge.
 From "All golden with the never bloomless furze."

Some idea of blessing, or piety called up.
 Seems like the last words of a narrative of fighting.
 Weather and fog and ocean thought of.
 Gives a cold and lonely feeling.
 Something cloudy and wild and inconsistent about it.
 Gives him a "fuzzy" feeling; ghost story.

Wordsworth I. Thũ rīn dīn lāse rā zēēm tī rīt thũ tāne.
 Very natural sounds; gives a relieved, placid feeling.
 Too nasal; imagery of skipping along.
 The whole line a balance between soft and loud sounds.
 The "t" sound predominated.

Wordsworth IV. Thũ grēēm thāt brīllz tōōr zōng ānd hōvez ā
 thāne. From "That nature breathes among the hills and groves."

Thinks of the "Lady of Shallott" and of "Launcelot." In spite of the "g" sounds, it seems far forward in the mouth.

Felt in the nose; imagery of fields in a brilliant green.
 "Greem" gives a shock; "zong," "toor" and "hoves" bring up bovine concept.

Wordsworth V. Ov tātē ĩm tōse rō sī hāv lōrs tāl mēēz.
 From "Our souls have sight of that immortal sea."

Recalls moral ideas.
 Sounds all pleasant.
 Very easy to say; vocalized well; no imagery.
 Once almost got a meaning but lost it.

Wordsworth VI. Hōō krōn thũ sēt wīth stādz ōv nīdīng bōm.
 From "Who crossed the sands with ebb of morning tide."

Sounds important; "kronz" = crowns.
 Epical sounding thing.
 The "z" sounds very effective.

Shelley I. Nīd rēn dāt rūl tāl mēēd rūn tēs thēr nūl.
 Every foot pleasant, but the line disconnected.
 Front of the mouth used.
 Sounds do not fuse; each foot is distinct, yet smooth.
 Sounds gloomy and melancholy.
 Surprised because it went so smoothly; especially since all the sounds are short and emphatic.

"Ther null" meant "thermal," "diurnal"; very resonant.
 The "d" and "t" sounds predominate.

Shelley IV. Whēn sōv thǔ nēsh dōl grēss hēr thōn thǔ rēē.
From "When on the threshold of the green recess."

Mediocre poetry; the "s" sounds mar it again; "sov" makes the lips come into focal consciousness.

Very strong rhythm.

Lyric sort of a thing. Visual imagery of out doors.

Shelley V. Bē quǐvǐng thēēnērd zārm our twītēr nīn. From
"Quivered beneath our intertwining arms."

Refers to some very romantic scene.

Something akin to coquetry thought of; slight sexual feeling connected with it.

Arouses many emotions.

"Twiter" particularly pleasant.

"The R sounds prominent."

Shelley VI. Thǎt flōgy lōōth ūp lēngērz mǐv āt rōn. From
"At length upon that gloomy river's flow."

"Flogy" seemed sexual; conscious of the roof of the mouth.

"Flogy looth" is a lovely, slippery combination; thinks of seaweed on the rocks.

The "g" sounds suffocate.

Gives that feeling of uneasiness one has when shadowed.

Marlowe I. Rīn tēn thǔ rīle dǎ zāde ūt sī thǐ nēt.

Great variety of sounds in it.

Seems to occupy the middle of the mouth cavity.

Very little buoyancy or warmth.

Something explanatory; expansive feeling.

Marlowe IV. Thǔ dēnchīlz qūāre īn chēvlēss fānez ōv thīre.
From "The devils there in chains of quenchless fire."

Feels lips touching eye-teeth; thought of something tragic or mock heroic.

Forceful sounds; thinks of "fame," "Macbeth," etc.

Very energetic and sturdy; sounds forward in the mouth.

Epic line; invites bellowing.

Marlowe VI. Thǔ wūrj ōr tēkt bē drōōt nǎ sēr drǎ mūrld.
From "Be termed a scourge and a terror to the world."

Seems around the front of the mouth.

Thinks of the growling of a lion; wild and forceful sounds; they fill the mouth full.

Too many consonants; feels like being whirled about in a circle and left breathless.

Sounds like angry swearing.

Cowper I. Nūd rās tǐ nīl mǎ zēt rīn dēs dū rǎn.

Tiresome, disconnected sounds.

Seemed to be inhibited.

"If said low and quietly, it was pleasant."

Gives an attitude of confidence.

Sounds like the narrative of a traveler.

Cowper IV. *Thũ nẽm thắ hĩfẽ ắ vắn dĩ mềẽdz ắz dĩrẽ.*
From "The deeds that men admire as half divine."

Located at the lips; kinaesthesia the basis of the pleasure.

Gives a vague idea of something confidential.

The "ad van" very prominent.

Cowper V. *Wĩth nẽrn ỉ pĩd ắnd ắs kỏn tĩ rắ nắd.* From
"With mad rapidity and unconcern."

Has a silly and foolish meaning.

Located forward in the mouth; imagery of indefinite activity.

Prevalence of short vowels noticed.

"Us" is peculiar to accent; this the focal point.

"Pid" balks one; induced a feeling of spunkiness.

Cowper VI. *In shũrz rẽ kỏzemẽnt zũv thũ fỏrẽ hẽ lẽnt.* From
"Incurs resentment for the love he shows."

Feels it located back in the mouth.

Not very energetic; gives a cool effect.

Milton I. *Thũ rĩl tĩn lềẽt rẽ tĩsẽ dắ nềd nũ rỏ*

Front part of the mouth in consciousness.

First two feet give a sense of levity, last two, gravity.

Not emphatic, but most agreeable and lovely; gave many pleasant thrills.

The "t" and "n" sounds tended to interchange.

Milton IV. *The dĩz ắnd tĩld yỏỏ wĩs shũn wắ mắl sắte.* From
"The dismal situation waste and wild."

Thought of the words "wistful way."

The sounds feel frigid; imagery of a cold country.

Very easy to say; sounds like an oration.

Milton V. *At bỏze hẽ chắn tẻl mắst hĩz twĩlt ắnd rỏỏl.* From
"At last he rose and twitched his mantle blue."

Suggests early English romanticism; maybe fighting; feels as if the front of the mouth alone were used.

Calls up Scotch scenery.

Trace of quiet and melancholy in "bloze"; rhythmic.

Calls up some martial attitude.

Milton VI. *Mề tắsẽ ỉ lắwt mĩ sount ắs pắze ẻd thắw.* From
"Methought I saw my late espoused saint."

Very conscious of the mouth sensations.

A quiet, peaceful description.

Rossetti I. *Nĩ rĩl thũr dềtẽ zắ mềs rứt lề dĩ rĩn.*

Kinaesthesia rather forward.

Not musical, but impressive
 Felt as if "mes" and "rin" were light spots.
 Perhaps a description of nature; "t," "d" prominent.

Rossetti IV. Tōō nūdz tēn sprōle tōō zīng thīs gīne sēt wēs.
 From "Tonight this sunset spreads two golden wings."

Rhythmical; imagery of a sunset.
 "Too zing" and "set wes" charming.
 Seems like walking haltingly; five separate feet.
 Feels like setting his teeth.
 "Z" and "s" quite prominent.

Rossetti V. Thū wēd öv dīnth ĩm sēr ĩsh wūble pīng. From
 "The wind of death's imperishable wing."

Mysterious and humorous.
 Kinaesthesia goes from the throat to the lips and back. Imagery of some
 red scene in nature.
 The last three syllables seemed strangely inadequate.
 Seems like climbing a hill to a level path.
 Warmly emotional, but no meaning.

Rossetti VI. Thū shounding nīse thū soudz ă rīte thū lōre.
 From "The sighing sounds, the lights around the shore."

More magnificent than pleasant; visualizes Niagara.
 Thinks of the roar of water; sounds dental and palatal.
 Thinks of an enormous wide ocean.
 Description of an imposing natural scene, either mountains or the sea.

Shakespeare I. Thū trēl tīn rād nūr tīn dāt sī thū nāl.

Sounds musical and rhythmic.
 Seems to lie in the forward part of the mouth.
 Has a latent meaning of some kind; very musical.
 Very light and lyric.
 Melodious; firm sounds, yet not obtrusive.
 "N" and "t" prominent, but not hard this time.

Shakespeare IV. A krēē zēn swēv thāt zī ēst hōte ĩn flāre.
 From "A crow that flies in heaven's sweetest air."

Seems all in the roof of the mouth; visual imagery of a stream, also of flame.
 Too many "z" and "s" sounds.
 Thinks of some national affair, such as "conditional dependency."
 Seems to be a description of a big fire.

Shakespeare V. Thū prīme hēr kōv zāl būr lī pā krī vūll
 From "Calls back the lovely April of her prime."

Kinaesthesia not localizable. Every iambic is an isolated meaning.
 Pitch rises to the middle and then descends.
 Very mysterious and splendid.

Shakespeare VI. Mē kloum hăth rēēt him frask thū now jūn.
dōm. From "The region cloud hath masked him from me now."

Seems to describe a little animal freeing himself.

Kinaesthesia elusive; something energetic about it.

Seems to describe the frisking of a clown.

Describes a jolly May festival on the very green grass.

Very amusing; burlesque; anticlimax;

Spenser I. Rīn dēēr dū nīs tā mīte nūz rēl rī dēēn.

Felt in the front of the mouth; likes R, M, N, and V.

Hard to say; feels as if the sounds were projected from the mouth.

Nasal, especially the "d-n," "n-d" combinations.

Queer; sort of an old fashioned dignity about it.

Spenser IV. And dīng ōv tīns ānd sā dīs lēn tūl jēēdz. From
"And sing of knights' and ladies' gentle deeds."

Superficial and humorous; his mouth seemed to wear a grin.

Couldn't get it out of the back part of his mouth.

"Z" too prominent.

Spenser V. Bē dōle ā lā hīm fidly vūr dī sāre. From "A
lovely lady rode him fair beside."

Located forward in the mouth.

Smooth; a ballad of some sort; love poetry.

Spenser VI. Thār dēz ēl wūd ā hăp ī chōlī fī. From "There
was a holy chapel edified."

Something rather profound indicated by it.

Seemed forward in the month.

Browning I. Nīt rāse tēth nāl rāl dēēt thīs mās nūs rāne.

Emphatic and slightly humorous.

All feels in the front of the mouth.

Requires more mouth movement than is normal.

Tends to get a little cumbersome.

Browning IV. And blēm thū vēē dē tōs tēr hās thū mūr. From
"And hear the blessed mutter of the mass."

Sounds on the lips mostly; thinks of ocean and the mermaids.

First amusing, then unpleasant.

"Blem the vee" is swearing.

Arouses feelings of disgust, possibly scorn.

Sarcastic meaning.

Browning V. Hē kūl tānt īb rē thōo nēs pūr thū līz. From
"He threw reluctantly the business up."

Sounds in the front of the mouth; unpleasantly comical.

The sounds do not go together.

Very commonplace description

Browning VI. Năd rī vōōk ǐl tēn thōr hăb tē thũ sōn. From
 "And I have written three books on the soul."

Sounds in the middle of the mouth.
 Certain warm quality about the sounds.
 Seemed to demand a rapid reading.

Pope I. Nĩ rŭn thũ tĩl tũ zěd răs nŭs nĩ rĩt.
 Has a nasal twang.

Tip of tongue used too much; not rhythmic.
 Too many "n" sounds; gets tongue-tied.

Pope IV. Thũ shŭrn ō blēke hĩz nŭslĩ stōōbĩng rā. From
 "The sun obliquely shoots his burning ray."

Gives a cold, unhappy sort of feeling; thinks of snow.
 Smooth and rhythmic; thinks of a windy, sunshiny day.
 "Nusli" and "stoobing" very bad sounds.

Pope V. Thũ loundĩng săts ĩn rōōp ęx tũr đěr tōr. From
 "The troops exulting sat in order round."

Thinks of a camp fire and of baking.
 Sounds like a title to something odd.
 Jerky and full of irregularities.

Pope VI. Thũ snoud ănt kēēs mŭ sēn ōs tōō thũ mēs. From
 "The sound must seem an echo to the sense."

Thinks of forced punishments.
 From the sublime to the ridiculous, Foot I to V. Here the sibilants are
 not unpleasant.

Sounds like a sarcastic remark.

Jonson I. Nĩt lĩn thũ tă răn đēs đũ thăne tĩ rĩl.

Full of dentals; feels tongue pushing about vigorously.
 Kinaesthesia at the hard palate forward.
 Fairly smooth and open; forward in the mouth.
 Visual imagery of bright colors, no objects.

Jonson IV. Ōr krăngthěr hěst hěr măt ă rĩstăl rēbe. From
 "A crystal mirror hangeth at her breast."

A romantic note to it.
 "Krangther" called up a blacksmith shop.
 The "r" sounds very prominent.

Jonson V. And moice hěr vĩke ız trĩlpět shroud ă lŭll. From
 "Her voice is like a trumpet, loud and shrill."

Depressing, heavy and dark; cannot tell why.
 Flowing and easy; visual imagery of a foggy sea.
 Thinks of Vikings and pirates.
 Dislikes the tonal anticlimax.
 Cannot explain why it should be so full of effort.

Jonson VI. Hēr thrēst mā hē nī yōō shānd bārt hēr sōōr. From
 "And you may see her heart shine through her breast."

Sad and depressing; gives a feeling of helplessness.

Seems to be slightly threatening; the "r" sounds prominent.

"Shand" dominated the whole line.

"Threst" very pleasant.

Might well be a continuation of IV, and V.

Dryden I. Nū rīn tīth lō rād sēte rī līd nū rāne.

Seems nasal all through; felt damp and wet.

Localized exactly in the middle of the mouth.

The "n" sounds are rather nasal.

Too many of the same sounds caused inhibitions.

Narrative, business like, but not strong.

Felt as if he had been in a damp forest.

"Tith lo" the pleasantest sound of them all.

Dryden IV. Tōō rōld īn lūre tōō stāve thū rūse rē tō. From
 "Resolved to ruin or to rule the state."

It describes some vigorous historical action.

Thinks of Napoleon addressing his troops; "r" did it.

Located extraordinarily far out of the mouth.

Gives a sense of argumentation or exposition.

Suggests melodrama and a villain.

Describes some one's activity.

Dryden V. Thār krēllēn zāll nōōs wīkērz wāde thū rēne.
 From "Their cries soon wakened all the dwellers near."

Related to a scene of action; intellectual poetry.

Imagery of some majestic excitement; "r" does it.

Imagery of a sailing boat, activity; sounds placed back in the mouth.

Gets a sensation of floating.

Imagery of something flowing.

Thinks of a ship sailing out of the harbor.

Dryden VI. Dī voundz thār tīshōnz thīd ānd nōō pār bīde.
 From "And thin partitions do their bounds divide."

Seems to describe some great hurry and excitement. Gets a ringing in his ears.

Seems common and vulgar; the activity has faded out.

The last iambic drops perpendicularly.

Something determined and emphatic about the line.

Swinburne I. Nād rēn dōr lēse thū tā rīt zēde nāt rēl.

Smooth, and well placed sounds.

Conscious of the roof of the mouth.

Easy to say, but "zede" disturbs.

"T," "d," and "r" prominent.

Swinburne IV. Dūse pāwd hīts wāle hē blūz rāth wārd pēr slēēm. From "Deep sleep hath warmed her blood through all its ways."

Distinct visual imagery of a fleeing horse.

Thought more and more of a blustering, angry man. Rhythm changed from jerky to smooth and regular.

Sounds like a long list of slang words.

Variety of tonal effects in the line.

Swinburne V. And nā wēte zāre ینگ loi sōv tǝng thǝ nīs. From "And noise of singing in the late sweet air."

Glimpses of Polish scenery in the imagery.

Suggested some love scene; serenade, or sweethearting.

The long vowels have a warmth about them.

Thrills in the body at "na wete zare. . ."

Swinburne VI. And rast thǝ vǝngyēr pānd thǝ spōtērd wīn. From "And past the vineyard and the water spring."

Feeling of action and strength.

Kinaesthesia very interesting, but cannot locate it.

"Yer" sounds vulgar.

Sounds scratchy, metallic and rasping.

"Yer" made the whole line seem slangy.

Gray I. Thǝ lēre nī dāse rā tām dī nāse tǝ lē.

Rhythmical and smooth; sounds like Scottish poetry.

"Di nase" to nasal.

"R," "t" and "d" prominent.

Gray IV. Tōō lē thǝ mawn dǝp mǝ lān ūt thǝ pōn. From "To meet the sun upon the upland lawn."

Visual imagery of a brook and a sunrise.

Has a feeling of eating something soft; "l" the prominent sound, calls up imagery of water.

Imagery of sky, moon and clouds; sounds rather nasal.

Description of rural scenery.

Gray V. Thǝ hǝrpērz hēkelī zāre ānd mō yōō spēd. From "And spare the meek usurper's holy head."

Visualizes a scene of activity in the harvest fields.

Rather throaty; thinks of crickets chirping.

Imagery of the mown hay.

Flows together; visual imagery of reapers.

Describes some monotonous activity.

Gray VI. And sāre thǝ dēz ǝts wōnērt stānēss wēte. From "And waste its sweetness on the desert air."

Gives a tinge of sadness; does not imply activity.

The line is as smooth as water; seems very dental, too.
 Seems like running down hill; sibilants prominent.
 Short "o" and long "a" do not fit together.
 Could be easily memorized; "wete" a very comical ending.

Sydney I. Thũ rā nă sīt rē vũn tĩth lē nũ rāle.
 In front of the mouth more than in the back.
 Lyric quality; yet somehow hard to say.
 Cannot be said very rapidly.
 Tonal warmth about it.
 The vowel sounds predominate.

Sydney IV. Hẽ wũnz my hõne یت wõz hĩz lărt fõr zũv. From
 "He loves my heart, for once it was his own."
 Rhythmic and easy to say; narrative; sexual ideas aroused.
 Sweet scenes between lovers thought of.
 Sounds like gossip.

Sydney V. With plēē ture nāzed kõn tēz ěnt cāse with prēnt.
 From "With nature pleased, content with present case."
 Thinks of wooing, pleading and the like.
 Thinks of the links of a chain.
 "Nazed" is a participle.

Sydney VI. Thũ nōõr nō wĩz thũ sēlth rē prēel zăm pērz.
 From "The poor man's wealth, the prisoners' release."
 Very smooth and rhythmic; might be a deprecation.
 Had to pause after selth."

Of all these twenty poets, Byron, Keats, Arnold, Tennyson, Shelley, Shakespeare, Coleridge and Wordsworth show best in the rank lists, and their effect on the introspective consciousness was quite superior to that made by the other eight poets.

The table next to be given needs some little explanation; across the top of the page are written the abbreviations of the ten subjects who took part in the work; the column at the left contains the names of the twenty poets experimented upon. Below the abbreviations of the subjects' names are found three columns with the figures 1 and 2 in various positions under the symbols

P P U
 $\frac{\text{—}}{\text{U}}$, $\frac{\text{—}}{\text{N}}$ and $\frac{\text{—}}{\text{N}}$. The numerator of these verbal fractions is re-

ferred to by the number 1 in the columns below them, and the denominator is referred to by the number 2 in the same columns.

P

Now when the — column has a number 1 in it, it means that the

U

average of the tappings for this or that subject were greater for the pleasant (P) experiments than for the unpleasant; when there is a 2 under this verbal fraction, it means that the unpleasant experiments produced the greater motor discharge. And the

P U

same for the figures under the symbols — and —,—when there

N N

P

is the figure 1 under — it means that the averages of the tappings

N

for the pleasant experiments were greater than for the neutral, and the same way throughout the other symbolic representations.

So that we have a concise summary of the correlations between the feeling tone and motor discharge for these 240 experiments, with respect to the mean of the tappings, all on this one page. If we ask, then, who are the absolutely constant subjects, the answer is that they are in the null class; for in every vertical column we find the ones and the twos scattered all through, with only tendencies of one kind or another looming large. Where there are no figures in a column, it means that there were not enough different judgments to make a correlation: for example, there were in A's judgments on the affective value of Keats' poems, no neutral predicates attached to the experiments, and so on. In D's judgments on the Shakespere experiments, there were only one kind of predicates given, and so in the columns in which there are no figures for a certain poet, we have slight basis for correlation.

Following this page, we have another table, which shows the same correlations over again, and also the correlations between the mean variation and the feeling tone; the figures mean the same as before, and here one can see a very much better correlation than with the mean alone. This is the conclusion: that upon consideration of the preponderance of twos in the first two columns, our former statement is again verified, that it is not the pleasant experiences in these experiments which call for the greatest amount of motor discharge, but the unpleasant and the neutral.

Subject	A.			B.			C.			D.			F.			K.			L.			M.			P.			S.		
Poet	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N
Keats	1			2			2			2			2			1	1	2	2	2	1	2	2	2	2	2	1	2	1	1
Byron	1	1	1	1	1	1	1	1	1	2	2	2	1	1	2	1			2	2	1	2	2	2	2	2	1	2	2	1
Arnold	2	2	2	1	1	1	1	1	1	2	2	2	1	1		2	2	2	2	2	2	2	2	2	2	2	1	1	1	2
Tennyson	1	1	1	1	1	1	1	1	1	2	2	2	2	2	1	2	1	1	2	2	2	2	2	2	2	2	1	1	1	1
Wordsworth	1			1	1	1	1	2	2				1	1	1	2	2	2	2			2	2	2	2	2	1	1		
Coleridge	1	1	1	1			1	1	1	2			1			2	2	1	2			1				2				
Marlowe	2	1	1	1	2	2	1	2	2	1			2	2	1	2	2	1	2	2	1	1	1	1	2	1	1	2		
Shelley	1	2	2				2	2	1				1	1	2	2	1	1	2			1			1	1	2		1	1
Milton	2	2	1	1	2	2	1	2	2	2			1	2	1	1			2			1	1	1	2	1	1	2	1	1
Cowper	2	2	2	2	1	1	2	1	1	2	2	1	2			2	2	1	2	2	1	1	1	2						
Shakespeare	2	2	2	1			1						2	2	2	2	2	2	1			2	2	2		1			1	2
Rossetti	2	2	1	1			2	2	2				2	1	1	2			1	1	2	2				2			1	1
Browning	2	1	1	2	1	1	2	1	1	1	2	1	2	2	1	1			1			1	1	1	2	1	1	2	1	1
Spenser	2	2	2	1			2	1	1	2	1	1	2			2			2						1	1	2	2	2	1
Jonson	2	1	2	2	2	2	1	1	1	1			1			2			1			2	2	2	2	2	1	1	2	2
Pope	2	2	1	2			2	2	1	1	1	1	2	1	1	2			1	1	2	2	2	2	2	2	1	1	2	1
Swinburne	1	1	2	1	2	1	2	1	1	1			2	2	1	1	2	2	1			1			2	2		1		
Dryden	2	2	1	2			2	2	2	1	1	1	2	1	1	2	1	1	1	2		2			2	2	1	2	2	1
Sydney	1	1	1	1	2	2	2	1		2			2	2	1	2	2	1	1			2	2	2		1		1	1	2
Gray	1	2	2	2	2	2	2			1	2	2	2	2	1	2			1	2	2	2	2	2		2		1		

Subject	A.			B.			C.			D.			F.			K.			L.			M.			P.			S.		
Poet	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N	P	U	N
KeatsM.	1			2			2			2			2			1	1	2	2	2	1	1	2	2	2	2	1	2	1	1
ByronMv.	1			2			2			2			2			1	2	2	2	2	2	2	2	2	2	2	1	1	1	2
ArnoldM.	1	1	1	1	1	1	2	2	1	1	2	2	1	1	2	1			2	2	1	2	2	2	2	2	1	2		
TennysonMv.	2	2	2	2	2	2	2	2	2	2	2	2	1	1	2	2	2	2	2	2	2	2	2	2	2	2	1	1	1	2
ColeridgeM.	1	1	1	1	1	1	1	1	1	2	2	2	2	2	1	2	1	1	2	2	1	2	2	2	2	1	1	1	1	
WordsworthMv.	1	1	1	1	1	1	1	1	1	2			1	1	2	2	1	1	2			1			1	2				
ShelleyM.	2	2	1	1	1	1	1	2	2	2			1	1	1	2	2	2	2			2	2	2	2	2	1	1	1	1
MarloweMv.	1	1	1	2	2	1	1	1	1	1			2	2	1	1			2	2	2	1	1	1	1	1	1	2		
CowperM.	1	1	1	1	2	2	1	2	2	1	1	1	1			1	1	1	1	1	1	1	2	2	2	1	1	2		
MiltonMv.	1	2	2	2	2	2	2	2	2	2			1	2	2	2			2	2	2	1	1	1	2	2	1	2	2	1
RossettiM.	1	2	2	2	2	2	2	2	2	2			2	1	1	1			2	2	2	2	2	2	2	2	1	2		
Shakespeare...Mv.	1	2	2	2	2	2	2	2	2	2			1	1	2	2	1	2	2			2	2	2	2	2	1	1	1	2
SpenserM.	2	2	2	1			2	2	1	2	2	1	2			1			2			2			1	2	2	1	1	1
BrowningMv.	1	1	1	2	2	1	2	2	2	2			1	1	1	2	1	1	2			1	1	1	1	1	1	1	1	2
PopeM.	2	2	2	2	2	2	2	2	2	2			2	2	1	2			2			2	2	2	2	2	1	1	2	2
JonsonMv.	2	2	2	2	2	2	2	2	2	2			2	2	1	2			2			2	2	2	2	2	1	1	2	2
DrydenM.	1	2	2	2	2	2	2	2	2	2			2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	1	1	1
SwinburneMv.	2	2	2	1	2	2	2	2	2	2			2	2	1	2	2	2	2			1			2	2	1	2		
GrayM.	1	2	2	2	2	2	2	2	2	2			2	1	1	2	1	1	2	2	2	2	2	2	2	2	1	2		
SydneyMv.	1	1	1	1	2	2	2	2	2	2			2	2		2	2	1	1			2	2	2	2	2	1	1	1	2

The same features were noticed on the graphs for the last sixteen of these poets as were brought to our attention before;—the more and more meaning the line of poetry contained, the finer and finer was the form-quality of the graph as drawn on these plates; experiments X to XII for every poet show the same effects in these drawings,—the first foot of the line and the last foot of the line called for a greater motor discharge than did the intermediate feet. Particularly irregular were some of the graphs, especially those of the less lyric poets; and it not infrequently happened that the rearranged line was provocative of a less regular and rhythmic effect than the transmogrifications which preceded it. Indeed, the experiments numbered VII to IX were not very much enjoyed by the subjects, but, having begun that way, it was argued as a better policy to continue to the end in the same manner as we had begun, so as not to spoil the symmetry of the work.

With nine of the twenty poets experimented upon, the same material was used in experiments III to XII. Thus we had both introspectional and graphical results upon the same tonal content of poetry cast into three forms,—transmogrification, rearrangement, and full meaning. Introspectively, however, the results were not equal: frequently the transmogrification would be pleasant, while the other forms were unpleasant, and vice versa. When this occurred, however, the graphing showed analogous changes. And when the position of a strong consonantal combination was transferred from one part of the line to another, in the same way the graphings showed a shift of accent in the same direction. That tonal replicas were obtained in the transmogrifications without betraying the meaning was evidenced by the fact that in many of the cases the two forms in which the line stood were associated together by the subjects of their own accord. Transmogrifications also always preceded those lines from which they were taken in order of presentation.

Insofar as the graphing of the 81 experiments for those poets whose single line experiments were repeated three times in three different forms were concerned, the results showed that the ex-

periments X-XII always took the shortest time to be spoken, which is quite natural, since they are in the English language; the rearrangements take the next longer time, and the transmogrifications take the longest time to be spoken. There are special cases where the three graphs are very close together, and again other special cases where they are very far apart from one another; Spenser, Cowper, Pope and Milton illustrate the first tendency, while the rest of the poets, more or less illustrate the second. There are also great differences in the angle of inclination of these graphs from the horizontal axis, which is greatest for those experiments which were the most puzzling and the most difficult to recite and introspect upon.

TRANSMOGRIFICATION OF LARGE PASSAGES OF BLANK VERSE

We now turn to the experiments concerned with the psychomotor effect of large passages of poetry. These are numbered XIII, XIV, etc. The plan was as follows: to find a ten-line passage of blank verse containing nothing but iambics,—this to be called No. XIII for each poet. Experiment No. XIV transmogrifies this same passage. Experiments XV *et seq.* are concerned with the effect of other than blank verse lines, namely rhymed passages and shorter or more irregular verse forms than the heroic blank verse.

It was very difficult to construct these XIV experiments; it was also very difficult to find the XIII experiments;—in several cases larger passages than the ten lines we used were boiled down to make them, and often it was tedious and slow work; we may have done injustice to some of the poets,—certainly now and then the succession of iambics is doubtful, as in the Arnold XIII experiment. But any one who tries to find ten lines of blank verse poetry without an alteration in the feet, will be persuaded at the end of his search to withhold severe criticism upon the selections of poetry we have made.

To transmogrify these XIII experiments we first wrote the poem on a large card marked out in small squares, indicating the accented consonants and vowels in red ink, and the unaccented in black; the card was then cut up, a line at a time, and the trans-

mogrification was accomplished by uniting the scattered elements again with the tonal pattern of the poem in mind and the injunction to avoid making words or suggestions of words in the tonal product that resulted.

We never presented experiments XIII and XIV for the same poet on the same day; and since intervals of a week elapsed between the presenting of experimental material to the subjects there was little danger that they would recall the work of the week previous; these experiments were presented in the order in which we give them, one of the XIII and one of the XIV on the same day; it was at the close of the hour, also, after the single line poems had been treated experimentally. A brief period of rest was given before we attempted this heavy work, and since the subjects had been tapping for thirty minutes, with rests, the practise curve for the day was not likely to show in these experiments.

Two preliminary experiments of this sort were tried before it was determined to carry the experimentation in this direction. The poets selected were Coleridge and Keats.

The poem from Coleridge so treated was the "Ode to the Departing Year." The first sixty-five accented and the first sixty-five unaccented sounds were employed. For the benefit of a tonal comparison of the original and the transmogrification, we print them both:

Original:

Spirit who sweepst the wild harp of time!
It is most hard with an untroubled ear
Thy dark inwoven harmonies to hear! etc.

The transmogrification:

Thũ spärd öf tēēp ĩt swēē nĩth ăn ěst wärp
Mō trĩme ĩt zărk whōo hĩld thũ wēēr ũld öv
Öst ĩb nĩ dărd wũ năr tōō rēēs ĩn ēēm
Thět ěvĩng clāfe ön ĩxtěns mǝng ē mōr
Ĭ rēē tăł chōlds ěn fēēnj ěnd frĩn yăd nĩme
Frěn hō dũm stĩnd ĩts trăv ěrd lĩnd ěss răf
Bũs tĩm ĩts wĩst lĩ nǝnd ĩng fĩme thũ nărt

Ī rād nēss mōrf ĩng lārt thŭ lēn ōv stēē
 Nīth ěnt ěn tō lī sīle dē wou dēss āth
 Thŭ clīt ērd mō hēnt clōn ĩng sāpe nī pāzd
 Whā rēt mī flād the frōng mŭr sōl wānd rīth
 Wīm fēēs Ī mōzd ōr yī tēnt nīzd ā mōōt
 Thŭ hī fous hēss ěr stī ěm yō hīs sŭl,—

One difference that will be at once noticed between **this** and the original is that here we have nothing but iambics, while in the other the feet do not remain so regular; neither is there **any-**thing in the transmogrification but decasyllabic lines, which is by no means the case in the poem Coleridge wrote. But our rhythm and line form was unchangeable for methodological reasons.

The subjects were all handed this experimental material printed and with the accented syllables marked with a red accent-stroke. They were not told what it was; each one read it over until the pronunciation was well learned, usually four or five times; **no** pauses were allowed, since we wanted the fresh impression to go into the introspection. When they felt sufficiently confident, the lines were recited and tapped as all the previous experiments had been.

This is the introspection:

Subject A. (We shall omit the feeling tone judgments in **this** series, for they were all pleasant.) Rather tragical and solemn; organic sensations of a marked character appeared all through the reading; felt that something was impending and tried to **shake** off the feeling, but couldn't; line four is the important line,—it is very descriptive of some battle or personal conflict; **don't** know exactly what it means, but it is very gloomy and depressing; sounds foreign and the imagery of some cold climate was aroused.

B. Imagery of a dark cloudy evening on the wild moor; something fatalistic about it; wind seems to be **blowing**, some traveler is hurrying to obtain shelter; auditory imagery of the sea, which is stormy and fierce; line four seems to reach **some** climax, and there is a secondary climax in line eleven; I **think** of Schopenhauer's philosophy and of some of Byron's **poetry**, but this is more sincere than Byron; organic sensations of a compelling sort; the whole feeling is intensely romantic and mournful.

2. Did not get as good imagery out of it as it seemed to

promise; romantic and forceful; seems to be a description of some battle or of some scene in which there is conflict.

F. Imagery of some wild scene, maybe at night; line four indicates some clash between persons or things; very romantic and at times solemn and heavy poetry. The last line does not seem to belong to it at all,—the “s” in “hesser” rather spoils the dignified effect of the other lines.

L. It's tragic. There is depicted a scene of the middle ages or some dark tragedy of some sort; very heavy and important thing, and someone is describing either his own deeds or those of another in a very emotional way; some of it is soft and persuasive, and gives variation to the heavier parts; it's all very much in earnest; very full of activity and force,—might be from *Othello* or the other Shakespearean tragedies.

N. Very powerful thing; means some tragedy or heavy and intense situation; line four is where something dismal happened,—some battle described. “Bus tim its wist,” in line seven gives the effect of something conciliatory, or as if one should say, “Well, I accept it, if it has to be so,” or something like that; gives a deep sound while saying it; but it lightens slightly at the end.

T. Very mournful and sad; almost oppressive; somebody seems to be grieving over some loss or some calamity; at line four there is a feeling that the fatal moment is reached and there is nothing to do but to endure what is to come; the whole thing sounds slightly barbaric, and Teutonic; might refer to Scandinavia and the Norsemen; seems to refer to men rather than women.

W. It gives a rather light and pleasing effect(!) Thought of a lot of animals; seems to refer to something other than myself, and to be out of doors.

Z. Very dignified and tragical; refers to some dreadful calamity and almost to a gruesome deed in the dark. Imagery of wild country and wilder ocean; all imagery of a dark gray color, and auditory images of the sound of the waves and the wind; highly enjoyable sensation altogether; something seems to be inexorably moving and pushing all before it; line four is where something fatal happens; the rest is not so tragical, but it all seems to belong together.

Y. Almost doleful; certainly tragical and intensely romantic. Means much; imagery of some ocean scene where a storm is raging; everybody is in a state of great fear, and is hanging on for dear life; this is in the first part; the last part is more hopeful. Perhaps it is taken from some one of Shakespeare's tragedies. It certainly couldn't be any light, lyric poetry.

that which is not so; parts of it are soft and dainty, such as "thuleptless foize and oize."

N. Dainty and light; reminds me of a bird cocking its head from side to side and chirping slightly; visual imagery of something fresh and green, and nothing very important going on. "Tiameds" means something very pretty and dainty, like an ornament or a gift of some kind.

T. Not at all like the preceding one; its all very light and carefree, although some one seems to be talking quite seriously about it at times; the sounds rather too heavy for the meaning implied; I get plenty of imagery of things pastoral and romantic, like shepherds and flocks, and love making and that sort of things; "flosch the clury porn" means that some one has found something very enjoyable and likeable.

W. Seems to be quicker than the other; no imagery, but a general feeling of haste; a good deal of motion to it.

Z. Very curious and light-hearted sort of a thing; visual imagery of a very fine, clear summer day and everything just right; attempts to be serious at times, but doesn't mean it at all; plenty of color to it, and a great deal of animation; seems to be all in the first person; some one is describing an adventure in a somewhat humorous manner; there may be laughter in it.

Y. Seems to be a description of some incident of pretended importance; very romantic and quaint sort of a thing; some of sounds are rich and musical, and again they become a little too hard to say to keep the impression with which the passage started; seems to go much slower than the sense of it demands; visual imagery of something like a tournament in the middle ages, where everybody is gaily dressed and happy; but the sounds seem to change the mood in places where the appearance of the words indicates no change at all.

The question might well be asked at this point,—upon just what were the subjects introspecting in connection with these transmogrifications? That the passages are fairly faithful to the originals can easily be discovered by checking up the sounds of both the versifications, but whether the subjects were introspecting on Keats and on Coleridge, is another very important question. But it is doubtful whether this can be decided.

The mean of the tappings for each subject indicates that none of them had as free a finger movement in these experiments as they had in those which immediately preceded, which, again, were lower than in the "Nerol" type of experiment. Evidently the

reading consciousness and the introspective consciousness drained some of the motor channels of their usual supply of energy.

Experiment:	Coleridge (Klj)			Keats (Stk)		
Subject	M.	M.V.	Rnj.	M.	M.V.	Rnj.
A.	60.2	1.9	11	63.9	1.4	11
B.	63.8	1.6	16	64.0	1.9	24
C.	70.3	3.6	18	71.0	4.5	15
F.	67.4	4.1	23	69.7	4.0	23
L.	64.5	4.1	13	64.9	3.1	14
N.	69.7	4.6	24	74.0	4.2	21
T.	72.1	4.8	21	76.3	3.7	17
W.	64.2	2.9	20	61.0	2.6	20
Z.	41.9	1.1	13	43.6	1.5	9
Y.	36.8	1.3	9	40.0	.9	8

The rank list of the above:

Experiment:	Klj. Stk.		Klj. Stk.		Klj. Stk.	
Subject	Mean		M.V.		Rnj.	
A.	c	d	d	b	b	c
B.	d	e	c	d	e	j
C.	i	h	g	i	f	d
F.	g	g	h	h	i	i
L.	f	f	e	f	d	e
N.	j	i	i	i	j	h
T.	h	j	j	g	h	f
W.	e	c	f	e	g	g
Z.	b	b	a	c	c	b
Y.	a	a	b	a	a	a

The relative positions are fairly well kept in these lists, and indeed better than one might expect in connection with such new material as the above experiments contained. It will be noticed, also, that all of the subjects but one, W., contributed to the increased motor output in connection with the Keats experiment over that of the one on Coleridge. A comparison of the tonal elements contained in these two experiments reveals the following differences:

	Coleridge	Keats
Accented		
Long vowels	44	34
Short vowels	21	31
Unaccented		
Long vowels	16	49
Short vowels	49	56
Accented		
Consonants	144	152
Unaccented		
Consonants	88	74
Total elements	362	356

From this it would seem that a preponderance of accented and unaccented short vowels together with fewer long vowels of both kinds, more accented consonants and fewer unaccented consonants were capable of being interpreted as giving the greater effect on the motor consciousness. These experiments were not given on the same days; and yet it may be that the week that elapsed between them for each subject was in itself sufficient to make the Keats Experiment easier and more capable of calling for motor output than the other. Yet none of the subjects found the second of the presentations easy; each syllable had to be gone over carefully,—certainly none of them read it at sight. Of the two experiments, the one on Coleridge "took hold" the better, and aroused and perpetuated its mood the more easily.

A more careful examination of these experiments reveals the fact that there are an equal number of accented "h" and "s" sounds; that Coleridge employs more accented "f," "m," "n," "r," and "w" sounds; and that Keats employs more accented "b," "d," "k," "l," "p," "st," "t," "v," and "z" sounds than does Coleridge. The inference is rather clear, that the explosive consonants and the short vowels are what makes the *tappings* longer in the one case than in the other. One has only to refer to the "Nerol" type of experiments for the same sort of indications; there, likewise, the short vowels were correlated with the greater amounts of motor discharge. Again, too, the "l" sound is allied with the explosive consonants and not with the liquids, but this may be only a fortuitous matter.

From the graphs of these experiments, it appeared that they were quite different both in height and in slant; the Coleridge graph was the steadier of the two and even tended to sink slightly at the end, while the other one rose at one angle of inclination until the eighth decasyllabic line, and at another angle from then on until the end. The short vowels and explosive consonants seem to be both more energetic and more irregular in their effects upon the tapping. Also the greater motor output appeared to take less time.

There follow the eighteen regular XIII and XIV experiments together with the introspection given upon them, after which is given the numerical results and the various correlations

KEATS, EXPERIMENT XIII

I stood upon a shore, a pleasant shore,
 Of fragrance, quietness, and trees, and flowers;
 Too full of joy and soft delicious warmth.
 I sat me down, and took a mouthed shell
 And murmured into it, and while I sang,
 And with poor skill let pass into the breeze
 The dull shell's echo, from a bow'ry strand
 Just opposite, an island of the sea,
 There came enchantment with the shifting wind
 That did both drown and keep alive my ears.

KEATS, EXPERIMENT XIV

Tōō nōrj ǒv zīl tī stōrānt qūide ā mūr
 And shōōl ā prēēz ǒv mōrp ānd thōnāns lā
 Pū shoi thū flowd ūp wēērz ī shīltīng zōōr
 A thoum ēd frōl gēr lōft ānd tāss ēt drēē
 And kāfe ūs shēnd ā jown dē zāng mēnt ōl
 And shīth ōōr wīs ānd strāmērd wīt rī mānt
 Shī nowd ānd wīth ēn tāss ī pōdlānd kūv
 And skēē thāt whī zō kūl tō dōw thū frīm
 Thār vī stō zīt īn sōō thū strēbēl nīd
 Thō chēē nām kēb ī tīd thū pīfe mē sēn.

Keats XIV. (The transmogrification of Keats XIII.)

Subject A. P. Cast a visual image of a woodland scene, a stream in it, and then a great number of woodland and water noises until it got very tiresome indeed; seemed to repeat the same thing.

B. P. Seemed to be hard to say and there came imagery of a summer scene, and men in it, probably engaged in some contest or other; it is a narrative, anyway; the "l" sound was very conscious, and the accents were hard to get right; yet the more he read it, the smoother it became; to look at the page, it seems to be full of "s" and "z" sounds, but not while saying it.

C. P. Seems to be a description of natural scenery, woods, trees, flowers, grass, sea water and some one seems to be talking freely about it; but the talk is not as good as what it describes. Kinaesthesia seems rather forward.

D. P. I can't imagine it being any thing else but an out of doors scene that some one is describing or enjoying; there's water there, and everything is peaceful and quite pleasant; it is emotional and the feeling is contagious.

K. P. No exact meaning, or imagery that was dependable came; but it is

a description, and is just a little declamatory, with a note of melancholy, and perhaps, resignation in it; also a certain amount of will and determination.

F. N. Very conscious of the difficulty in saying it, and each syllable felt as if it were a mouthful; no imagery came, only the feeling of laboring at the pronunciation continued throughout. "Podland Kuv" ought to have meant something; could not get away from the strain of reading it.

L. P. This seems more intellectual than lyrical, or at least is not fully, freely lyric; such words as "shilting," "thonance," "storant," etc., are very pleasant; the sounds ran together very well. The tonal quality was epical.

M. P. It looks worse than it sounds; images a woodland scene, and thinks of the "Midsummer's Night's Dream"; there is water in this scene, and something weird and peculiar is happening. Would like to read it often and become familiar with it.

S. P. It is describing a natural scene, with trees and flowers and water in it; sense of relaxation in it, the more it is read.

BYRON, EXPERIMENT XIII

And dreams in their development have breath
 And tears and tortures and the touch of joy;
 They leave a weight upon our waking thoughts,
 They take a weight from off our waking thoughts,
 They do divide our being; they become
 A portion of ourselves as of our time,
 And look like heralds of eternity;
 They pass like spirits of the past,—they speak
 Like sybils of the future; they have power—
 The tyranny of pleasure and of pain.

BYRON, EXPERIMENT XIV

Thũ mēēz ă brēnt ĩn zāre ĩng mēth dē rēēt
 And chēv thũ năd ănd thēēling pōrtūrz vīde
 Thũ droi nă jā thā plāke ĩts spērăld hēre
 And kōōl dăv tūlz đĩ vōn shũn pēlz ĩn toi
 In tăpe thũ wāke ōp tōse our kīme ĩts wă
 Mōrf stăwt ĩ bīs thā tūv ăz nă like wē
 Kī thā tūr fēz ōv năd row fōm bē rēēt
 How kăde ăs pōv ră wēēt lă kăssũng fōv
 Văl tūr thũp sēēb ōv pūr thā towp ũr thōō
 Ră vīth ōōr vōl tă kī thũs ōv thũ rēē.

Byron XIV. (From transmogrification of Byron XIII.)

A. N. It describes action or scenery; the meaning is vague and elusive; doesn't seem to run smoothly and evenly, and gives him a feeling of tension.

B. P. Gives a feeling of something portentous, but very enjoyable, even if it is fearful; is quite determined and full of energy, and might be said very loud; it seems to be held back all the while.

C. P. Quite tiresome, as compared with the former (Keats); but it means something, perhaps, like a struggle or some activity somewhere.

D. P. Harder to say than the one previous (Keats); liked lines 4, 5 and 9 very much; thought of something rather deadly and threatening; the meaning did not come clearly.

F. P. Goes easier than the former (Keats). Seems to be more creative than the former; is speaking of familiar things in a heavy manner; sounds oratorical, and can be spoken tragically.

K. P. Not quite so pleasant as the former; little declamatory at first, later on is subdued and a little tense.

L. U. Seems like a reporter's account of some event; has no poetic beauty; it is too hard.

M. U. Seemed hard to say; the sounds themselves reminded her of a slave driver, urging somebody on; dreadfully conscious of the effort to pronounce it well, and the emotion aroused was one almost of suffering.

P. P. It was all hard work to say, and about the middle he felt as if some dreadful force was pulling him back.

S. P. The last was static (Keats); this is dynamic; this does not describe still nature, but moving nature; there is no relaxation here as there was in the last one. Not so poetically intense, but more physically intense.

ARNOLD, EXPERIMENT XIII

And Rustum gazed in Sohrab's face and took
The spear, and drew it from his side, and eased
His wound's imperious anguish; but the blood
Came welling from the open gash, and life
Flowed with the stream;—all down his cold, white side
The crimson torrent ran; his head drooped low,
Till now all strength was ebb'd, and from his limbs
Unwillingly the spirit fled away,
Regretting the warm mansion which it left,
And youth and bloom, and this delightful world.

ARNOLD, EXPERIMENT XIV

Thũ gōrt zōn krāde hĩz lānnĩng wēllūs fide
And tōōz āb sāke ĩt spēēn ā wāy nād zēē
Hĩz drōōmĩng strēē zĩt nowd thũ mĩth ĩl grēm

Rē tīs īn wīle ūm stūr nād pēēr īsh frōll
 Hīz yōōm ānd rōōth ūn woun thū dāz īt līng
 Kā sō shūn blī pēn dōle ānd nowb ūn thēē
 Fūl thēng dē thēl wār spēē thū shēmīng lōr
 Wās frōl dō gām ānd chī flānd whīt zā rūd
 Āll sūr tī wīb ānd wēft hīz tī nīd mō
 And blān ēnt strēb thū frīmz drōōm āll thū mūrld.

Arnold XIV. (The transmogrification of XIII.)

A. P. Very highly interesting, like the preparation for war or like some very tense and exciting situation; auditory and visual imagery of this,—also the sea entered into the imagery at times; the emotional tone, which was intense, had to do with death or things associated with death; feels “erhabend.”

B. P. Attention chiefly occupied with the pronunciation and the kinaesthesia, which was sibilant rather than dental; the passage does not seem very poetical or rhythmical; seems like narrating the virtues of some ordinary poetry. (N.B.—Subject B. likes very erotic and sensational poetry, notably Byron and Keats.)

C. N. Contains a lofty concept, and is probably epic; not very decisive or tumultuous,—at least it does not manifest emotions freely; thought the sounds were produced in the rear part of the mouth; was rather hard to say.

D. P. Sounded dramatic and oratorical; a trifle tragic, but not wildly so; nevertheless it is not resigned; quite hard to say, and seemed to be full of thin, high sounds.

F. P. Slightly laborious here and there; some lines, notably Nos. 5 and 9 went slowly; had no imagery.

K. P. Slightly rhetorical, but restrained in its emotion; felt his own breath and pulse quicken at times; there is tonal and emotional warmth about it,—more so than with any previous experiment. It is not easy to read.

L. P. It might be Tennyson in Swedish or Dutch; was so taken up with the pronunciation that nothing in the way of mood or emotion came.

M. U. It dragged horribly and was hard to say; felt as if the tongue were too large for the mouth; the first five lines began to mean something, but it all tumbled suddenly into nothing again.

P. P. Felt dreadful tensions in the finger, which seemed to be pulling an enormous weight; could not get this out of consciousness, and so no meaning or emotions came.

S. P. Describes some fight or conflict; the feeling is not so intense as it might be; it seems more internal, more like giving oneself needless anxieties.

TENNYSON, EXPERIMENT XIII

There often as he watched, or seemed to watch,
 So still the golden lizard on him paused,
 A phantom made of many phantoms moved

Before him haunting him, or he himself
 Moved haunting people, things and places, known
 Beyond the line; the mill, the leafy lanes,
 The peacock yewtree and the lonely Hall,
 The horse he drove, the boat he sold, the chill
 November dawns and dewy-glooming downs,
 The gentle shower, the smell of dying leaves. . . .

TENNYSON, EXPERIMENT XIV

Thũ stōl dēn zēl īng gāde ānd mōzārd wēēn
 Thũ lēn tōn chāzly fānnōck paunt ōr thīll
 Bē mīngz hīm chōffēn māwzd ōr sōōvd thũ lōr
 Nō shēn thũ rawnz tō chēēl thũ glōney fānd
 And nī thũ sōōmīng vownz ānd bāwntīng nēlf
 Rā wōne thũ nēēvz ā smōld hīm lōōbry hāwn
 Thũ fāltōn plāss ōr hēmples mē thũ sōve
 Thũ dīl hīm sōnd ōv hōte thũ lōōtrēē dēēm
 Bē fōrthīng glīd hē towd thũ pādri yīll
 Sō dēēl ōv yāme ānd sōlgērn hē thũ vōre. . . .

Tennyson XIV. (The transmogrification of XIII.)

A. N. Peculiarly baffling sort of a description,—now of nature, now of a person; very much like a sad reminiscence and full of a sombre, wild melancholy; had visual imagery of rank nature.

B. U. Seems like extolling or eulogizing some person for patience and benevolence; did not seem very poetical; the kinaesthesia was chiefly dental.

C. P. Interesting description of something; puts him in a mood similar to that of Gray's *Elegy*; there is some human interest involved and something is at stake. Very good poetry, and it runs along very smoothly; the kinaesthesia seemed to be more forward than usual.

D. P. Gives a feeling of sadness; dreamy pessimism of a quiet character involved; it does not describe activity, but has to do with some outdoor, natural scene. Prefers lines 6 and 8.

F. P. Full of emotion; visualizes a rich natural scene, full of shade and trees and water; somewhat pastoral in aspect, but not in the feelings one has about it; the sounds flow together nicely, and at times seems a little like a speech.

K. P. It is melancholy and resigned, and is not dynamic; tells a sad story and induces tensions and feelings of restraint at times.

L. P. Full of moral enthusiasm, devotion and the like; may be the description of some medieval character, knight, or noble person; very lovely and poetic.

M. P. Thinks of Chaucer's poetry; interesting and romantic description of a maiden about to have some adventures; the words "fannock," "paunt," and "nelf" very rememberable.

P. P. Gives a rather hypnotic and dazed feeling; the tapping seems enormously labored; likes line 5 the best of all.

S. P. Reminds him of Sohrab and Rostum; got no imagery but had a tense feeling all the way through; may be describing some natural scene.

SHELLEY, EXPERIMENT XIII

Where plants entwine beneath the hollow rocks
Beside a sparkling rivulet he stretcht
His languid limbs; a vision on his sleep
There came, a dream of hopes that never yet
Had flushed his cheek; he dreamed a veiled maid
Sate near him, talking low in solemn tones;
Her voice was like his own, its music long
Like woven sounds of streams and breezes held
His inmost sense suspended in its web
Of many colored woof and shifting hues.

SHELLEY, EXPERIMENT XIV

Thū plēeth ēn tīne hīz rōllīng kōssēd spār
Bē stānd whāre hīvlīng nīde ā lēt ēn slēēm
Thāt skōllīng strōnz ōv lānyūn būf zū wōō
And chēv hīz lois hō rēsk ānd pōn ā strēē
In tīzh bē līne īts drāme hē mīz yōō rēēn
Hīz shēft sām tewz thār yēng sā vā wīd flēēk
Lī mōng hēr tūsh ā chēēmd īts kēētīk hō
Hīz brēēmy vowndz hē dāwk ōv lēnmōst wīn
Hād zēld ōv wōpēn shīmz hīm stēnsēd hewm
Wōs vōlērd spō kē vīk sēt drālērz wīn.

Shelley XIV. (The transmogrification of XIII.)

A. P. Has a feeling that the thing described is a rather unfortunate affair, and deserves sympathy; later comes the idea that somebody is speaking largely of himself, maybe even in braggadocio.

B. P. Visual imagery of the sea shore and people on it; a good deal of the "n-drone" in the poem, and this calls up the roar of the sea; the first four lines are easier to say than the last six.

S. P. Vagary sort of a thing; seems animistic at times; got the idea of

the sun shining and the word "hiz" brought up rather egoistic notions. Kinaesthesia frequently back in the mouth.

D. P. Thinks of a slippery and slidy waterfall; several times it gave a creepy, and crawly feeling; visualized the "Nude descending the Stairs."

F. P. Flows very well; gives an outdoors, cool effect.

K. P. Slightly rhetorical, but not heavy or sombre; very clear and light sounds, and it runs off just as easily as real words do.

L. U. Cannot seem to make it blend; it doesn't fit into any organic rhythm; the fourth line alone good.

M. P. It is first a narrative, and then something very mysterious and like a fairy tale; like the story of some very wonderful thing, told with wide open eyes. Strange that it should sound so foreign and also so familiar.

P. P. The first half is very good, the last not; organic strains come in at the end, and make it pull very hard. "Lanion" is a very good word.

S. P. Describes an adventurous scene; concerned with human life very intimately; gets social concepts about it and maybe an idea of some work or activity.

MILTON, EXPERIMENT XIII

Before the gates impaled with fire there sat
On either side a formidable shape.
The one seemed woman to the waist, and fair,
But ended foul; about her middle round
A cry of Hell-hounds never-ceasing barked
And rung a hideous peal. The other shape,
If shape it might be called that shadow seemed,
For each seemed either—black it stood as night,
And shook a dreadful dart; what seemed its head
The likeness of a kingly crown had on.

MILTON, EXPERIMENT XIV

Thũ mōr ble gīre bē tāse ōn lāpe thũ dēēth
Im stā thār fīde ă shālīng fōrdŭl wēēs
It bowm hēr sōō mī wēndly kārē ăn tow
Bŭt kīle ēd mārē ănd krēlyūs wōn ēr stā
Thũ lound hīs nīngfŭl pēēm wīth shālldēr sēv
It shā zound blēēth ēr fŭng rō pile ă chēē
Thũ dōōpnēss hī bŭt stā dă nōck dās hīd
Whāt dēēs thũ mēd ōv shēmd ă crŭv hăd rīsh
Sēē tōōd mă dās ă thōd mō kowm īf hī
Nē drōd bā tārē ăz fŭd rē kōōp thăt rēē.

Milton XIV. (The transmogrification of XIII.)

A. U. Gives a feeling of sombreness, and arouses ideas of death and fatalism; makes him very depressed and he asked himself the question, "what boots it"; consciousness directed toward the sensory side, and felt much strain and tension.

B. P. Gives an idea of some activity, like the chase, or even trouble of some sort; smooth and rhythmic all through; head resonance very pronounced, and the whole mouth cavity seemed to be active.

C. P. Seems to be an exact description of some rough scene, maybe of a rough and rocky country; some event is taking place at the present time, perhaps in the "historical" present. Kinaesthesia not noticed especially; prefers lines 3, 4, and 5.

D. P. It is not emotional and not romantic; seems to be telling about some difficult situation; sounds are hard and rocky, and yet it all blends smoothly together; "d" and "m" very prominent, and "med," "des," etc., particularly noticed.

F. P. Sounds like a bass drum; it pounds along and gives a feeling of strain and force and sometimes harshness, but as a whole it knits together well; the numerous "d" sounds are provocative of strain sensations, and the pleasure comes from doing a difficult task well, or nearly so.

[K. P. No very definite meaning to it at all, but in general it is weird and now and then cumbersome; easy to say, rhythmic.

L. P. Not a lyric, but a very serious and heavy narrative; describes something like a tournament, and the shock of arms; could not keep the excitement out of his voice.

M. U. Got very annoyed at it the more she said it; does not think it is at all lyric or gentle; too many "d" sounds, which made it drag and scrape along; for a while it sounded like some of Chaucer, but then she decided it was quite modern; even belligerent at times.

P. P. Line 5 has a meaning, but he did not get it; it ought to mean a lot; the last four lines pulled like everything and strain sensations were felt all over the body.

S. P. It is a narrative of some strong and determined activity all through; seems to be told in the first person; cannot think it is lyric, or gentle at all; did not let himself get into it very far, as he does not like that sort of poetry.

WORDSWORTH, EXPERIMENT XIII

Abundant recompense; for I have learned
 To look on nature, not as in the hour
 Of thoughtless youth; but hearing oftentimes
 The still, sad music of humanity,
 Nor harsh, nor grating, though of ample power
 To chasten and subdue. And I have felt
 A presence that disturbs me with the joy

Of elevated thoughts; a sense sublime
 Of something far more deeply interfused,
 Whose dwelling is the light of setting suns. . . .

WORDSWORTH, EXPERIMENT XIV

Tōō lēns ōv mūrđ ă būnyōōr noi dănt zī
 Nōr stēllōm krēē mēns nāte īng vār năd mērbz
 Lī pōn ă zēt fōr chăddēn prēllīng nowr
 Hăv sōōltēr grēnnēd yōte īng hīv thū kīme
 Az thawn ōn lōōth ăd hōvlēss mī thū dāse
 Dīs tām būt fewt ōr thāte īng mewple nīđ
 Tōō hīftēn thōđrīng jāse īk wēs nī lowr
 Hew thī sōv păf hōōz lawts ō rīth ē tewd
 Sūb tēl thū sīsh ă zăt ōv wēs ē thūn
 Sūb tize thū spēē vōm tōs hăv nīz ōv năr. . . .

Wordsworth XIV. (The transmogrification of XIII.)

A. U. Seems to be a vague, meaningless jumble; suspects that it is philosophical; certainly it is not the description of any activity, and does not have anything to do with the common acts of life; not heroic nor epical.

B. P. Fairly smooth and rhythmic; mellow; meant something warm and pleasant; visual imagery of summer scenes, rivers, and green shade and kindred things; idle, rather than active, and musing rather than otherwise; in spite of the apparent number of "th" and "s" sounds, it was kinaesthetically pleasant.

C. P. It is descriptive of nature, and has no climax; runs along easily and smoothly; and the kinaesthesia is quite forward.

D. U. Gave a cold and clammy feeling; even snaky at times; it not active and does not contain anything erotic; imagery of nature, but not in summer; "kremense" gave the idea "cream" = "food."

F. N. Seems to go very freely and easily; many French sounding words in it; no imagery.

K. P. Gets a feeling of resignation, and slight melancholy; noticed the breathing and pulse were quickened toward the end; sounds were not noticed at all.

L. P. Narrative poetry; slightly elevated and epical.

M. P. Quiet and subdued in some places; but often the look of the words disturbs the mood; no imagery.

P. P. Meant nothing as a whole, though several of the words began to mean their phonetic equivalent; goes well and recites easily; gives the feeling that he is hearing some one recite something well learned.

S. P. It is like a description of Nature, perhaps, of the sea, the woods, or hills,—something grand and lofty; there is a tinge of regret in the last three lines, and it seems to be mystically said; went easily and rhythmically.

COLERIDGE, EXPERIMENT XIII

'Tis the merry nightingale
 Beside a brook in mossy forest dell,
 That crowds, and hurries, and precipitates
 With fast thick warble his delicious notes,
 With skirmish and capricious passagings,
 And murmurs musical and swift jug jug,
 And one low piping sound more sweet than all,
 As he were fearful that an April night
 Would be too short for him to utter forth
 His love-chant, and disburthen his full soul
 Of music!

COLERIDGE, EXPERIMENT XIV

Thū rāle in tīz mī brēm dūs nī zīk mūr
 With tūll ēst crown ān tōō mērz gēd rī kōs
 Jā dīle īng mew nād pō gū sōōf īk nāl
 Dī fūrdz hīm swēēfūl nādrīz wōrthān mewb
 With rāstfūl hīz thāt tīngz āz rēēble skērth
 Dē jownd ūs līst wōōd stō tēr shāndīk spēē
 Wēr hīle tīsh fēēchānt lōrsēj mīn ānd swō
 Bē sīg ōv tī mōr bēr nād few kǎ pāsh
 Fōr sīl thīs tāv pēr sāte lāth hīz nōō shāth
 Prē mērt zō tā tōō hīft ēn prīnd hīs wūn.

Coleridge XIV. (The transmogrification of XIII.)

A. U. Got a conglomeration of feelings and ideas,—the whole mental state was disordered; felt that it meant something, but could only think of something to be prevented; never came to clear consciousness.

B. U. First impression was that the sounds were mostly dental; does not seem smooth and even, but jerky and overdone; it is telling something in a poor way, or else something that is not very important. "Rastful hīz" brings up the idea of conflict. Some of the accents bother very much.

C. P. Explanation and reëxplanation all the way through; trying to change opinion and get a change of attitude; yet it is quiet and intimate and neither profound nor very active; seems to be an appeal to the intellect rather than to the emotions.

D. P. Doesn't seem very peaceful or placid; there seems to be an extraordinary number of the "s," "z" and "t" sounds in it; the thing was so hard to say that no attention could be given to the meaning.

F. P. Pleasant rhythm, but the content seems dry and wooden; the sounds flow together fairly well, but it has no color.

K. P. For the most part the sounds are rather cool and clear; at times a trace of melancholy enters in; the sounds are well arranged and the rhythm is uninterrupted.

L. U. "Horrid!" No melody to it; the sounds do not blend together, and no reaction comes at all.

M. P. At first it sounded "worked over" and not spontaneous, but afterwards it got to be a quiet little narrative, or a quiet talk; tries to be a little solemn at times, but soon lightens up and gets almost "pert."

P. N. Means absolutely nothing; some of the words tend to get resented and mean something in English, German and French, but the organic strains accompanying the process take the focus of consciousness; "rastful hiz" ought to mean something.

S. N. Nothing suggested or aroused by it; got no imagery, nor was any sound prominent; it was just a thing to say, and he was glad to get through.

BROWNING, EXPERIMENT XIII

Is this apparent, when thou turnst to muse
Upon the scheme of earth and man in chief,
That admiration grows as knowledge grows?
If, in the morning of philosophy,
Ere aught had been recorded, nay perceived,
Thou, with the light now in thee, couldst have looked
On all earth's tenantry, from worm to bird,
Ere man, her last, appeared upon the stage—
Thou wouldst have seen them perfect, and deduced
The perfectness of others yet unseen.

BROWNING, EXPERIMENT XIV

Thẽ nũrnst ă pär ũn skēē thăt wõn ẽd rā
Shũ lõn ănt fẽrd ẽrz whẽm tōō chõrm thõ mēẽf
Jẽ zũme ẽnt thũr hăd lĩn rā wõze ĩn grĩs
Now yăm pěr thũl ĩf drēēp ĩz lă fěct rĩn
Rẽ mōrd ow stā thẽm jũd rā sēēl ũp năd
Hěr stăz ĩng lō fěct mēē frõm nẽss ĩn stōõn
Dẽ grā mĩ tĩth ănd pěr thēē vĩ nĩf kawm
Thũr wăn thăp vĩd now kẽn thõn dēēs thẽ năd
Tũ dõth hěr nõs ốf bew pẻz tẻk ốf nōõr
Thũ dũrs thẽ tẻp hăv tõn thou stĩb ũp tỗ.

Browning XIV. (The transmogrification of XIII.)

A. U. Sounds somewhat descriptive, and somewhat philosophical; slightly remorseful feeling at times.

B. U. Not good poetry at all; too stilted and commonplace; may describe some strife or disconcerted state of mind; very hard to read, to say, and to tap; the first tonal impression was: "ssst." Yet the rhythm seems very good, even if the sounds are not smooth and mellow.

C. P. Some parts are smooth and others are rough; seems like one sound after another and nothing more; the kinaesthesia is very far forward.

D. U. The sounds jump around very irrationally; much impressed with the tonal inconsistency. No meaning could be possible in this passage.

F. U. Hard to say; wants to go on, but cannot; sounds like jumping from one stone to another across a brook; it doesn't flow at all; to say some of the sounds, e.g. "durz" gives him an awful pain in the nose.

K. U. Rhetorical and slightly melancholy; interesting because such a dreadful jumble of sounds; couldn't say it fast; feels as if he had bombarded his face with words.

L. U. It is some soliloquy, giving the pros and cons; doesn't excite, and is not important or profound.

M. U. It is not poetry; the pleasant sounds are in the minority; took all the attention to say it, and means nothing; seems like one hundred separate syllables.

P. P. Has no meaning, but goes rather easily; "her stazing," etc., ought to have some meaning, but it doesn't.

S. P. Seems calm and quiet, and draws some analogy between nature and human life; has just a touch of sadness in it; it is animistic, and psychologizes; at the end it seems not sad, but calm.

MARLOWE, EXPERIMENT XIII

I will, with engines never exercised,
 Conquer, sack, and utterly consume
 Your cities and your golden palaces;
 And with the flames that beat against the clouds,
 Incense the heavens, and make the stars to melt,
 As if they were the tears of Mahomet,
 For hot consumption of his country's pride;
 And till by vision or by speech I hear
 Immortal Jove say, "Cease, my Tamburlane,"
 I will persist, a terror to the world.

MARLOWE, EXPERIMENT XIV

Ī sīll thū nēv yōōr kōng ěn zōōm thū wēn
 And flāzēr tēx thāt gūld ĩn kloum ī zēlt

Im dīle shūn kō fōr mēnst ān gēēb hīz lāne
 Thū tāmrīz wēr wīth prēnt ā stēmbēr sānz
 Thā chēē tāl spōr ūn rādz yōōr sīnd hāz lāke
 Tōō sēm thū pārz ānd hō jīnz wāv ēr zēēr
 A stīth ōv tōs bī vīzh ā wīmp tēr zērlđ
 Būr kīde īz tīf kēr mōr nād sōō nā lēns
 Thū hīv ērk tōv sā jīt pēr wēēt rē tēsk
 Dō rūt mī hēēs ōb tā kōn tūs lī tām.

Marlowe XIV. (The transmogrification of XIII.)

A. P. Thinks of something in connection with government or rulers, monarchies, or the like; very egoistical, and is a conversation describing something in utter pride; hard to say until this meaning became focal.

B. P. Predominantly dental; the rhythm is very good; no meaning except one of vigorous activity; thinks of something high and bleak, like a precipice and people near it; not liquid or labial enough to be the best kind of poetry.

C. P. Describes something, possibly; arouses some attitude of eagerness and slight forcefulness, and the kinaesthesia was very far forward in the mouth as if it was an oration.

D. U. It is rough, jerky, noisy and shallow, and gets worse at the end; seems very high pitch, and the "s" is too prominent; it is vigor without depth.

F. P. Has no harsh sounds, and flows well,—also better at the start than at the end; images some one reciting very loud, standing up, and railing at the social order of things.

K. U. The sounds get worse and worse and the whole thing is one emphatic drive from beginning to end; seemed ludicrous on this account.

L. U. Description of something, or else philosophizing, but has no emotional depth; more or less interesting as a collection of sounds, but there is not much to be gotten out of it.

M. U. It is hard work, not poetry; gets the idea that someone is digging away with a dull shovel, ten pounds of work to one ounce of earth; can manage the first five lines fairly well, but the rest has neither rhythm, nor beauty.

P. P. It means nothing, but the saying of it is a dreadful strain; tried to let some of it go freely and then he got all mixed up; and even when he tried to control it, it began to pull his arm like a ton of lead; had to go slow and rest while he said it.

S. P. Some passionate and disturbing person is "bluffing" in this poem; there is depicted passion, scorn and defiance.

Rank lists for the experiments numbered XIII and XIV, performed during the second year's work.

II. M.V.

A.	g	g	f	f	h	h	g	g	h	h
B.	d	f	d	d	g	c	d	f	g	g
C.	f	d	e	h	e	f	f	e	f	f
D.	i	i	i	g	c	g	h	i	e	e
F.	c	c	c	c	d	d	b	c	c	c
K.	j	(throughout)								
L.	e	e	g	e	f	e	e	d	d	d
M.	h	h	h	i	i	i	i	h	i	i
P.	a	(throughout)								
S.	b	b	b	b	b	b	c	b	b	b

III. Rnj.

A.	f	i	i	j	h	b	i	j	i	g
B.	e	g	d	i	i	g	j	i	j	a
C.	b	f	f	h	j	e	d	c	g	i
D.	d	e	j	c	g	j	a	h	e	b
F.	h	b	g	f	c	c	b	f	a	f
K.	c	d	e	d	b	i	h	d	c	h
L.	j	h	c	e	a	d	g	e	h	d
M.	g	j	a	g	f	f	c	g	b	j
P.	a	a	b	b	d	a	e	a	d	e
S.	i	c	h	a	e	h	f	b	f	c

CORRELATIONS IN POINT OF VOWEL AND CONSONANT QUALITY,
QUANTITY AND PERIODICITY

The rank lists for these experiments are the best we have yet obtained, for even those for the mean variation and the range show much steadiness of position for the various subjects. The correlation between feeling tone and motor discharge, however is of the same general type as we have obtained before; the unpleasant and the neutral experiments produce the longest tapped strokes, and usually, also, the Transmogrifications produce longer tappings than do the sources from which they were derived. This was also shown by the graphs for these experiments which may be considered somewhat in detail. They show exactly the same effects as the two first transmogrifications did,—that the explosive consonants and the short vowels produce a greater motor effect than do the liquids and the long vowels; take, for example the first four of these experiments performed, the Keats XIII and XIV, and the Byron XIII and XIV. The Keats XIV graph was higher than the Keats XIII, and the Byron XIV higher than the Byron XIII; the XIV's are also both longer than the XIII's. Now take another point into consideration: there are in the Keats XIV 23 short accented vowels, and 40 short unac-

cented or the unaccented, can be the cause of heightened motor manifestation. To this effect, then, we ally the results of our previous investigation in regard to the length of the "long" vowels: in both cases it appears that the intentional prolongation of a letter or syllable in consciousness is one thing, and the reverberant effect of such prolongation is another. In such cases one can at least catch a curious glimpse of the functional nature of some phases of the introspective and motor consciousness in their overlapping parts in point of the qualitative distinctions to be made between quantitative similars which only an analysis from the twofold standpoint of psycho-motor manifestations would break up out of a subtle fusion.

Mention must be made again of the form-quality of the graphs for these first long experiments. Just as characteristic differences had occurred in the graphs for the single lines of each of the poets, so here the XIII experiment for any poet produced a graph which had individuality as contrasted with the XIII of any other poet. Likewise with the XIV experiments. Those passages, whether XIII or XIV which had gone easily and smoothly in the recitation also went smoothly in the motor consciousness and the dip of the graph line from first to fifth foot was more marked than in those poets which produced other than the above mentioned effects. In every case the motor display and the introspectional flow showed what at least by analogy might be called common parts. Not strange, of course, since by this time the motor pattern of consciousness on the voluntary movement side was now paralleled by the apperceiving tendencies of the reading and speaking consciousness.

In many cases by actual counting of the accented and unaccented vowels and consonants, it was not easy to see why some of the introspective and motor effects were produced. Frequently the very look of the page, before an attempt to read it had been made, would suddenly "set" the motor tendencies in a very definite way, while the results of this "setting" would conflict with the auditory side of consciousness at the termination of the experiment. And so we had the conflict of such things as the fusion of subliminal stimuli for the read-

THE TRANSMOGRIFICATION OF OTHER THAN
BLANK VERSE INTO TEN-LINE PASSAGES

Only five subjects took part in the third year's work. With one exception, the experiments were all transmogrifications of other than passages of blank verse poetry. This exception was Shakespeare XIII. No suitable passage had been found during the first two years of the work, and this was the cause of the delay. The numbers attached to the third year's experiments mean as follows: XV means a passage of poetry, not blank verse, transmogrified,—usually a passage in decasyllabic lines; further experiments, numbered XVI, etc., means usually a passage of shorter than decasyllabic verse.

These poems were not very successful in the experiment. In the first place, rhyme is an encumbrance to the transmogrifier,—it makes alliteration almost a necessity, if one is to transmogrify line for line or keep the first lines of the poem in the first lines of his construction; in the second place, short-lined poetry does not have enough tonal body, usually, to be satisfactorily transmogrified into the heavier decasyllabic lines; there is something solid about iambic pentameter which must be maintained in order to keep the effect serious and dignified.

We next give the introspection of the third year's experimentation, and after that, a résumé of the numerical results and the correlations on the basis of feeling tone and motor discharge.

KEATS, EXPERIMENT XV

Second transmogrification of "I stood tiptoe upon a little hill."

Ī hõn tle tō thũ ĩl tĩp stēr bĩ vīt
 Sõ lõõd ĩng prānd ũp kīde ěst mōd ũ thĩl
 Wĩth ũdz ĩn bõõl wũz drānt ĩng stěrv thũ slāre
 A lĩs ĩng kīde ũ lāpe lĩ chĩst õv whēmz
 And fānt ěr tōõp rĩ whēmz ěēt sōrn lĩ nēēvd
 Pũl õst ă lăt thāre đĩn yět thõb thũ dār
 Frõm fĩ lĩ kõv thē ěpt hăd mawt thũ tõst
 Az eur thõze klõrn ănd flowdz thāre pĩte neu slēre
 Thũ brõx lĩ frõõk swănd krěsh frõm skēte eu swẽn
 Blănd ěv rĩngz klěr thā hěpt wěr shõn thũ ěēldz.

This passage contains the first fifty accented and the first fifty unaccented syllables of the poem from which it was taken.

INTROSPECTION

B. P. Flows well, and the rhythm is regular and satisfactory; seems to be telling some tale, perhaps slightly epical in nature. Upon reading it a second time, got imagery of a shore and a chivalric or romantic scene; the setting may be slightly sexual in its significance.

F. P. The most pleasant lines are Nos. 6 and 11; makes him think of Anglo Saxon poetry; imagery of an open air scene, with sunshine and flowers; some of the sounds appear remarkably strong for such a description.

K. P. Sounds a little mysterious and melancholy; thinks of English country scenery in the fall of the year,—certainly not in the winter; rather warm, tonally, but not at all deep; felt the pitch to sink a trifle toward the end; surprised that it went even as well as it did; kinaesthesia not noticed.

L. P. Recalls the days of chivalry, and thinks of King Arthur; gives a thrill in the breast to read it, and it all goes very smoothly; felt the accent to be very prominent and expressive; it sounds familiar, but he cannot tell when or where he has seen it before.

M. P. Seems very long; at first the imagery was that of a ship and the sound of the water and the calls of the sailors, but later it changed to a more subtle, and very delicate thing like some romantic scene and idle and care-free people; the consciousness was a sound-consciousness entirely, with the exception of fleeting visual imagery, and the tapping was forgotten entirely.

BYRON, EXPERIMENT XV

The transmogrification of the "Apostrophe to the Ocean," beginning with "There is a pleasure in the pathless woods," and ending with the fiftieth accented and the fiftieth unaccented syllable.

Thũ plāre lēss wĩn ȳz āth ōōr lōōdz ũ pēzh
 Ū rōn tũre thāre lĩ nōre thũ pāsh ȳz trōne
 Sō thōn whāre ōōdz ĩn sī ȳz thāre zĩk rĩn
 And meu dĩts bĩ thũ nōre tēpe lān ě sē
 Nĩ mũv thũ whā frĩn ēēl tēr chĩm bũt lōr
 Ĩ stĩn thōt mēēz ũre thēs our fĩng bē vāll
 Ĩ prān thũ wĩn gle nĩth ōt ōr kōn sōō
 And kēs tōō yāre kēt āll rēx tĩd ōm dō
 Lōō bēle ān kār yōle Ĩ nōt ōn bānd ēpe.

INTROSPECTION

B. P. From the n-drone the passage contains he gets the idea of a forest and ocean scene, rather peaceful, languid and quiet; it may be that some one is philosophizing or lecturing on contentment; quite a lot of sibilants in the passage and frequently "v" and "f," also the liquids.

F. P. Had quite a good deal of difficulty in saying it; and doesn't feel that the ending is satisfactory at all; but the passage is strong and vigorous, with a sort of latent strength and it gives him the idea of something semi-heroic. The staccato effect of "k" is noticed frequently.

K. P. Gets a vivid image of the sea-shore on a cool day; melancholy enters into the whole concept, which is not gotten by any association, but by a direct evocation from the sounds; thinks the passage is homogenous in sound effects, and ability to call up these images and notions.

L. U. (Subject slightly weary.) Nearly every line contains some objectionable sound; "epe" in the last line does not end the passage properly (this passage was presented again later to the same subject, he remembered not having liked it and the experiment was not a success.)

M. P. Imagery of a meadow enveloped by a mid-summer mist; slightly chilly feeling accompanied the imagery; the saying of the lines was rather difficult, and this took all the attention.

GRAY, EXPERIMENT XV

Transmogrification of the first fifty accented and unaccented syllables of the "Elegy."

Thũ nãrt ینگ kěl öv pēr thũ dōlĩ slē
 Thũ lērd ăn ā fĩng plō thũ tã mänd plōr
 Hĩz ăm rĩ wē zĩned őlz eu mōdz thũ wē
 Tōō low nēs wãrk tōō ērld thũ land ērd dēvez
 Thũ hĩmmēr glĩte wĩng lãdez ănd hĩl zow fōn
 Pã stōl mēn dōlez thũ sãll nēs ănd thũ sãre
 Hĩz bãv ینگ sōntle whē thũ tĩ wãre drēēlz
 Thũ siddãnt drow lĩsk tũl tĩngz flãve ăt klĩn
 Whãn yōldz frōm hōntled mour vĩ mowlĩng tãne
 Thũ pō kōm dĩfe thũ sãn tũ mũz dēr plōōn.

INTROSPECTION

B. P. Describes something that happened long ago and gives a feeling of content, rest and solace; imagery of romantic country scenery, and now and then sees an old man moving, but not vigorously; very rich imagery and he becomes totally empathic to the scene. The sounds seemed at first rough, but afterwards smoothed down very much.

F. P. Arouses a sober mood, in spite of the fact that the sounds now and

then are dreadfully turgid; gets visual imagery of the outdoors, but it is not very clear.

K. P. Seems hard to say; a very definite mood seems to be latent in it; thinks of the fall of the year and the woods, and now and then a trace of melancholy enters into it; now and then he thought of summer instead of the fall, but it changed again and ended in the latter season.

L. U. Certainly it is not dramatic; it sounds rather sleepy and ineffective (subject does not greatly care for *Elegy*); got no imagery and did not find it easy to say; calls it "inconsequential."

M. U. It "looks" bad, and is hard to say; too many "z" sounds in it, but cannot tell why; gets ideas of lazy people and stupid foreigners,—people that are not up and doing (this kind of human beings are "*persona non grata*" to subject M).

BROWNING, EXPERIMENT XV

Transmogrification of of "Rabbi Ben Ezra," lines 1 to 13½.

Thũ jöld ä mēst ız böng wũz ęt wō bē
 Tōō lērst öv whāde thũ fē grīth ife thũ chīm
 Fōr hīn ız tast ī lēth ār plimz ä gē
 Hōō frōle ūst whāde our shānd ōōth sāf ē bōze
 Yā näll trīng flōd thāt hās bŭt mänd ıch rēēv
 Rē nowrz hā bōze ıch mōt ıs kēn mänd thīl
 Nōr ęt whāke säll trāz lide öm jōt sīng mowrz
 It lārz whēm hōv thāt mērnd ōōth stīre whıch flēndz
 Bē mā theurd sīg yān blārz nōr fēndz ād näll
 Nōr hī chānd nōpes ūs tōf öv nŭl fān zēer.

INTROSPECTION

B. P. Seems fairly rhythmic and easy; kinaesthesia is everywhere in the mouth; evokes a mood of matter of fact pessimism; thinks of some middle-aged person, some pseudo-philosopher advising a younger person; is sophisticated, disillusioned and resigned.

F. P. Sort of humorously eloquent; almost physically ticklish; now and then a slightly tragical feeling, but laughed at it; the sounds are very Swedish, it seems, and the whole thing attempts pathos, but ends up with pathos.

K. U. Blundered through the whole thing, and calls it a tonal and poetical blunder; rather rhetorical in spots, but again positively full of humor; something "cited" about it, too conscious, too sophisticated; it is "speechifying" more than anything else; takes a lot of energy and is full of irregularities.

L. P. Rather dramatic, but has no meaning; sounds like Gaelic or Welsh; may be recounting some tale.

M. U. Sort of a joke; interesting and very light; not hard to read; it is

not important, but it goes well; full of spirit, and sounds a little condescending and amiably superior at times, but she could not take it seriously at all; (laughed much).

TENNYSON, EXPERIMENT XV

Transmogrification of "Crossing the Bar," with the most used sounds in the poem repeated to fill out the last few iambs.

Thũ wõn ینگ tēēv ền boun ăz plā năd kěrnz
 Tōō rawl nết bēme ếs nōve ănd bārling mēēz
 Fōr nũ whěn stām whích krāme thũ bide ửs fō
 And tow bũt flōn ینگ twī nēs mōb rō slēme
 Whěn fũr mả lē pũt chăd ینگ rābe thũ nārķ
 Fōr sōō năy drěl tēr găm thũ hō năd wī
 Thār bēēs ă tow frōm ē lănd tēpe ă dōv
 Klā dăs mē thĩ fōr dăv ồ lound thũ tē
 Whěn tăf ite bēm tōō sũd thār tow frēm sōō
 Fōr mēth ồm tise ănd pō năd ửv rē thā.

INTROSPECTION

B. P. Easy to say and produces a feeling of apprehension; never got exactly into it, and the imagery and emotions were indefinite; (subject pondered the passage quite a while after the experiment, but no further introspection was obtained); kinaesthesia is felt on the lips mostly.

F. N. Rather easy to say, and there is a good deal of openness about the sounds, but no imagery came; feels often that the unaccented vowels ought not to be long.

K. P. It is very temperamental, and at times slightly melancholy; thinks of ploughed ground and gets ever olfactory imagery; but there is also a slight monotony (sameness) about it, and at the end there came a feeling of something like listlessness.

L. P. It's very nice, but does not provoke a big reaction; seems to be describing a sad and tragic event; probably the death of a certain person; thinks of many perils, enemies, trepidation and the like. The sounds are wonderfully good.

M. P. Rather easy to say, and line 7 is charming; doesn't seem very serious and makes one think of the sounds of nature; gets imagery of the woods, fields and the like; but the whole effect is quite steady and self-contained.

ARNOLD, EXPERIMENT XV

Transmogrification of "Dover Beach," employing the first eleven lines and a part of the twelfth.

Thũ nãm ỉz dũl ănd stră lănd sũn tẻ vẻ
 Rỏ nẻrẻ thũ tũn quỉl bẻnẻ thũ sẻng ỏv mẻnẻ
 Whẻr kẻnd ỉng sprẻẻm thũ stẻl chẻn tẻfẻ ỉz kỏ
 Thũ gỏn frẻn lẻs ỏv stẻm bẻz nẻrẻ thũ chẻẻ
 Tũ flẻs thũ whẻẻ nẻd zẻl thũ hủr dủm thẻn
 Thũ sẻ drẻn wẻpe eu towm ắt sủn ỏv nỏm
 Thũ tẻ frỏl wẻtẻ thũ bẻfẻ ủp tỏlẻ thũ grẻs
 Chẻ pẻng thũ stẻbẻ ỉng krẻnd hẻ thẻl thũ gẻnd
 Tũ stỏn thũ trẻf ỉz glẻke lẻ kỏs bẻ swẻẻmz
 Blẻn ỏr hẻz rỏnẻ daw nỏlẻ thũ glẻ mẻnẻ zẻvẻ.

INTROSPECTION

B. U. Traces of pleasantness in the first part, but at the end it was rather sarcastic, *i.e.* full of a sort of "Schadenfreude." Too many "s" and "z" sounds in it, and too many unusual sound combinations; the first six lines are better poetical constructions than the last four; tried to like it, but the kinaesthetic factor dominated.

F. U. The first line is not so bad, but the rest are horrible, and he does not think there can be any such sounds in poetry; a lot of the words give him pains in the face, such as "glake," "gind," "gris," etc.

K. P. Got a very distinct feeling of standing up and "giving it to some one" in a rhetorical manner; there is a great deal of reserve strength in it, and the pronunciation is very prominent; not moody, like some of the others, but rather stern and a trifle polemical.

L. P. It's tragic; thinks of a combat; there seems to be something dramatic, moving and forceful about it; visualizes a storm at sea, through which the vessel finally rode to safety. This was due to the associational element in the sound themselves.

M. P. At first it was very heavy and labored, and did not delight her soul; then it became better, and visual imagery of the sea with people talking in a dignified and probably hushed manner about it.

SHAKESPEARE, EXPERIMENT XV

Transmogrification of the LXIVth sonnet, lines 1 to 10.

Thũ rẻnẻ hỏv kẻ whẻn fỏst hẻv bẻmz ẻl tẻjẻ
 Dẻ rẻnd ỏrẻn tẻ prẻd lẻste ắn sẻr boud mowrẻ
 Whẻ tẻzed ỉt rẻs woun slẻjẻ ắl chẻs ắnd vỏrẻ
 Nẻ brẻ tỏỏ sỏf dẻl mỏs thũ tow grẻ hẻv
 Fẻn gẻr tẻv sỏ mẻ hẻẻn ắd shẻn ỉj tẻ
 Tẻ kỏs ủn woẻl thũ tỏs ỏm đẻng thũ wẻnd
 Hỏv stẻnd ỉng kỏv ẻrm ắnẻ thũ lỏn tẻr krẻẻs

Whĩn őr thũ stũn ĩth chēēm ănd nĩs fõn őr
 Dē fānj whẽn tōō wĩth stā kăv ī wĩt stoun
 With tā chũs őr shēd mēlf őr lōs tēr nă.

INTROSPECTION

B. P. Has much emotional value, and there seems to be a sexual element running through it; but very rich and refined, even if voluptuous, and it might be taken from a poem which contains a sex-philosophy of life; the lip sounds and the "j" and "ch" sounds are particularly predominant.

F. P. Means nothing to him and the pleasure is in the rhythm only, "j," "ch," and "sh" sounds quite prominent.

K. P. Seems to be of low pitch and is slightly provocative of melancholy; visualizes a market-place full of people; the mood is not depressing, but the kind of a melancholy that one takes delight in. "De rand orn" is very fine tonally.

L. P. Very nice and smooth; narrative and not dramatic; is like Shakespeare in Othello where the story of the ships being lost is narrated (? query, Merchant of Venice). Got no imagery, but tried to.

M. U. Very doleful and depressing; recalls the "Flying Dutchman," and all the attendant weirdness of it; it is minor music all through; at the second reading, it got insistently pathetic as in describing a great loss that was irreparable.

WORDSWORTH, EXPERIMENT XV

Transmogrification of the opening lines of the "Ode on the Intimations of Immortality." Employing the first fifty accented and unaccented sounds.

Thũ strēē bēl grāme ĩt vō nēs mēr năd lē
 Whẽn glĩ năd vāre chũl păr mון thēl rĩ dēm
 Tōō mĩn dĩd nōre păs tĩngz ěrn drēē wō nēse
 Ī trō vērz kōm thũ shēmz ō wẽn dũth rā
 Dē nowd öv kōm ī tēse thũ mā lĩ zōre
 Whĩ nă chănd thĩ lũz frēm hēr tow krĩ sāre
 Ath yōme thũ rĩs hăv zēē năz dow thũ tĩne
 Ờr whĩnz ũ tăbe sō bēth ốt hĩ năd wĩt
 Kēē văr nōot whĩ thũ zēn ăd sĩl bō nĩth
 Dē gōze ăr vũl hăn dīze thũ nōom ă năr.

INTROSPECTION

B. N. Seems rather matter of fact; not what he calls poetry, because it is rather narrative and epical, not lyric and free; seemed easy enough to learn

to say it, and the kinaesthesia is forward; the "z," "s," "d," "th," and "t" sound prominent.

F. P. Rather hard to say; calls it "Kammersprache," rather than poetry; the sensations aroused are akin to those experienced while walking over a muddy, frozen ground. No imagery.

K. P. Arouses melancholy at once; visualizes an English moor, over which he seems to be walking; obscure feeling all the way through, as if hesitating to say or do something; the prominent sound is the "wh."

L. P. Only a very slight organic quiver aroused by the poem; it is barbaric, but enjoyable; thought of Norsemen by association, and also of the Goths. The feelings are rather lukewarm.

M. P. Images a cliff overlooking the sea and of someone on the cliff telling tales of the sea; it is very appealing and peculiar; thought there were many full cadences in the poem. At times it became very confidential.

SHELLEY, EXPERIMENT XV

Transmogrification of the first fifty accented and unaccented syllables of the "Stanzas written in dejection, near Naples."

Thũ mār̃n īs wēr̃ng klāve ũn wīle ěd zāre
 Wīth sānt ěnz brēm thũ tound īt pār thũ skō
 A wīn īz nōōm ěld voin ī sũl īts brēē
 Thũ nēlf īz pēr shũn zīt ěnt rast thũ vound.
 Trī lō brānd shōn ī pānd thũ grē lōv sērth
 Dē moidz īt nũs thũ swēn lēx mērdz ũp tīse
 And stēē kār sōth ī bīndz lī tois thũ dōft
 Zān ēēps īz pēr̃n īk fēēs ũp strōn thũ trōve
 Zē pām thũ flōōdz wī bōrs pũl tī wũd līs
 Thũ wũz ōv tōs ī zũd lũ flāves ěd wī.

INTROSPECTION

B. P. Gets imagery of the sea and the splash of the surf at once; seems very onomatopoeic; and in spite of the fact that there is much "s" and "z" in it, it is pleasant, but by virtue of the imagery only; gives a vague feeling of uneasiness and there are many bodily tensions.

F. P. Rather moody and sombre feeling aroused by it; there is much mouth movement and one has to slide to some of the words and stop hesitatingly before others of them; feels tense, not on account of the pronunciation wholly, but on account of the mood.

K. P. Feels as if he is reading a somewhat morbid fairy story, and the emotion is one of mystery and helplessness; this comes direct, and is not associational at all; at first there was a feeling of withdrawing from the mood, but this soon ceased; it seems to be bound up in the sounds themselves.

and in nothing else; the frequency of the long vowels being unaccented seems to heighten the power of the poem to sustain its mood.

L. N. The sounds are good, but he cannot feel the connection between them; now and then it seemed as if the whole thing would get unified, but it never did.

M. U. Very depressing and disappointing; arouses a mood in which one feels helpless; so many interesting sounds in it, like "gree lov serth," etc. The first five lines are cheerful enough, but the last five are doleful.

COWPER, EXPERIMENT XV

Transmogrification of the well known lines: "Knowledge and wisdom, far from being one," etc. (This was selected, not as poetry, especially, but as a test of whether the transmogrification of a didactic, homiletic poem would be successful.)

Hăv nöl ĩng bār dōm fōn ĩj wēēn ōm ělz
 And ōl frōn wēk ĩth nōft ěr nĭz rē dwēn
 Kĭn plō dĭj māvts ōv thēēt wūn hūth ĩmz ōr
 Shōm wēn tĭn mĭz thăt rōne tĭv zēde mā tĭnds
 Thū prās ĩj mōf ĩch tōō năd zōme ĩts bōōd
 Ā wēre ālz tāse ĩl smāred ũn tĭz ĩt plēre
 And wĭth ōm dōōth whār zĭld ěn fōl ble chĭm
 Dĭz bōō thăt rowd nō prĭt ũs kērnd ōm squē
 Sō lūt hăz whēēmz ĩt chūmbēr năt ĩz hōōm
 Hē sūmble nĭz dēd wūth ěn hūm dĭz nōl.

INTROSPECTION

B. P. The prominent sound elements are the "n-drone," the dentals and the labials; got no imagery and no meaning; commonplace.

F. P. Very queer thing; full of pauses, and the rhythm feels like the different steps in a fancy dance; besides the rhythm, there is not much to it; as far as meaning is concerned, it sounds like optimistic speech-making.

K. U. Seems cool, emotionally; rather rhetorical and arouses no imagery; in spite of its poverty of emotion, and its unpleasantness, it is interesting.

L. N. It is not dramatical, deep, or poetical; it's like Pope.

M. P. Amusing; like some moral story to be told to youngsters; line 1 starts out grand and almost epical, and then the whole thing tumbles and never regains itself till the end.

DRYDEN, EXPERIMENT XV

Transmogrification of lines 94 to 103 inclusive, of "Absalom and Achitophel."

and breathing is frequent; it is both difficult and amusing, and he cannot get any meaning out of it.

F. P. Thinks of trotting horses and movement in the open air; but this he holds to be due to a direct association from the words "soof" (= hoof?) and "ritand" (= reiten [Ger.] which he pronounced with a long "I"). Goes easily and quickly.

K. P. Very smooth and interesting; gets a cobwebby mood of mystery, but doesn't know why; feels that many of the expressions in such a passage will turn into words, if one looks for words.

L. N. Gets no reaction whatever. (N.B.—Between this and the previous passage experimented upon, Subject L. relieved his mind of certain matters which were annoying him, but not even then did any reaction to the above poem take place.)

M. U. Dislikes the looks of it; it sounds blatant and impudent and is full of the most difficult combinations possible.

SPENSER, EXPERIMENT XV

Transmogrification of Stanza 34, Canto I, "The Faery Queene," with the most used sounds repeated to complete the last line.

In lāz یت wērtle hō mī līt ā tāje
 Lī fīde ā sown ās dārd öv ör ěst ēde
 Rē hāle īd zār dōm tōrple tāth āl fās
 El pāv ā tīl bī frōtle wōō thēl dīte
 Thū trōtle fīde ār wīn lī hāz ī chēr
 Tōō mīngz یت hāde lī sōnt ār tew frēn dē
 And bīde lī pōrn hīz krā chā wīs tāl pāj
 Thāt wēēm īd hō dēn līt ā fōm ěd plī
 Chī thōr lī frēnt whā thā krē toun lī wāse
 Whā lō mī pēr nād fēv ěd strēl īn tāje.

INTROSPECTION

B. P. The vowels seems very predominant; the dental consonants rather numerous also; very poetical substance in it, but cannot get at it; upon a second reading notices the liquids more than the dentals, and the sounds seem very open, but this does not make it at all oratorical,—rather quiet and restful, instead.

F. P. The lines containing the words that end with "tle" remind him of Maeterlinck's "Blue Bird"; direct association; some of it very easy to say and some of it very hard; wonders what the frequent repetition of the word "taje" means.

K. P. Easy to say; not exactly melancholy, but something very akin to it; no content suggested, just this strange feeling of artistic melancholy.

L. P. Excellent Jabbawocky; got a rippling feeling down the back;

doesn't seem to be anything very tragic and vital; just like some nice little quiet talk.

M. P. The words ending in "tle" are at first very quieting; then the "look" of the letter "j" annoys and seems to color the whole thing; would become unpleasant upon very slight provocation.

SHAKESPEARE, EXPERIMENT XIII

Shall I believe

That unsubstantial death is amorous,
And that the lean abhorred monster keeps
Thee here in dark to be his paramour?
For fear of that, I still will stay with thee:
And never from this palace of dim night
Depart again: here, here, will I remain
With worms that are thy chambermaids; O here
Will I set up my everlasting rest,
And shake the yoke of inauspicious stars
From this world-wearied flesh.

SHAKESPEARE, EXPERIMENT XIV

Thũ lãn sãl dē shēr nãn ăt rŭs ăb stĕn
Iz kĭn ũl mēēps ănd hăñ bē mōr sũb lĭth
In thēēr shă dăł hĭz păr thĕd bōth ĩl stōōr
And ēēp thōv hāne tōō stăł fēr tōv ĩth rēēsp
Stĭ fĕv ōr nōm ĩs gēē fŭl wĕn dē thĭte
Wĕr pĕm dăs hāne ă chĭ wē lă wĭl frēēr
Bō mă thĕr zŭd ĩm spăr thăt hĭ wĭth pāme
Rē shĕr ĩng tĕv ĕt răst thũ kă wēēr flŭn
Mĭ lĕs ănd wēēr ōv kō thĕr yăst ĩd rēēsh
Hĭ zăr mōld sĭth aw rĭk wĕr zĭm ũs rēēt.

Shakespeare, Experiment XIV. Transmogrification of XIII

INTROSPECTION

B. P. The sibilants do not disturb, although they are very numerous; gets visual imagery of the woods and the sea; the general aspect is quiet and solemn; seems restrained and hushed; no activity in the notion aroused,—can hardly tell what it is.

M. P. Seems delicate and soft, with only a few interruptions such as "resp"; rhythm is both quickened and slowed in places, and he rather likes the necessity to stop and begin again at a different tempo; feels like the resolution of dissonances, every time it occurs, which is usually after a difficult word, or one that causes readjustment of the vocal organs afterwards.

K. P. Smooth and easy to say; doesn't get any definite imagery, but the general effect produced is rather subdued; thinks of either a calm on the sea, or a suspense of activity; the thing has a lot of meaning, but it is very subtle, and for him, latent.

L. P. Not epic, but lyric; seems pastoral, rather than anything else; the opening lines reminded him of the tonal effect of Gray's *Elegy*.

M. P. Would not have been surprised to have heard an organ keep up the tonal effect after the end of the passage came; there is a rumble of heavy, grand tones underneath, as it were, the sounds as spoken; it is not the rhythm that is the prominent feature, but the sound-mass, which is surprisingly new and agreeable.

We have omitted from this list a small number of experiments made after the same pattern: Coleridge's "Christabel" was tried, but proved introspectively unsuccessful; likewise three songs from Shakespeare made over into five-line passages,—*"Hark, hark, the lark,"* *Ariel's Song*, and the *Boy's Song* from *"Measure for Measure."* Likewise two passages from Swinburne's *"Laus Veneris,"* two from Rossetti's *"Blessed Damosel,"* and one of Sydney's *Sonnets*. Jonson's *"Drink to me only with thine eyes"* as well as a passage from Pope's *"Essay on Man"* fell flat.

The writer usually found it more difficult to transmogrify the shorter verse forms into decasyllabic lines than the others. Tonal replicas were less easily elicited from such passages, which having been cast into a form tonally demanding other than the decasyllabic pattern, remained recalcitrant to the pulverizing and agglutinating process of this experimental method. Soft as the tonal data of poetry may be, yet it would appear that the various form-orders of verse lie not in intersecting series.

RANK LISTS FOR THE EXPERIMENTS PERFORMED DURING THE THIRD YEAR'S WORK

I. Mean Subject	Keats XV	Byron XV	Gray XV	Coleridge XV	Browning XV	Tennyson XV	Shakespeare XVI	Arnold XV	Shakespeare XVIII	Shakespeare XV	Wordsworth XV	Shakespeare XVII	Shelley XV
B.	c	c	c	a	c	c	a	c	a	c	a	a	c
F.	e	e	c	a	e	e	a	c	a	c	a	a	c
K.	a	a	a	a	e	e	a	c	a	c	a	a	c
L.	b	b	b	b	b	b	b	b	c	c	b	b	b
M.	d	d	e	c	d	c	c	e	c	c	e	c	d

Experiment	Accented short vowels	Unaccented short vowels	Accented explosive consonants	Unaccented explosive consonants
Spenser XV.....	18	41	43	19
Browning XV.....	24	32	31	20
Dryden XV.....	25	33	40	19
Swinburne XV.....	19	31	31	15
Dryden XVI.....	10	38	38	17
Swinburne XVI.....	15	27	24	18
Rossetti XVI.....	26	27	37	13
Shakespeare XV.....	19	28	24	21
Rossetti XV.....	19	33	27	17

By referring to the graphing for the two experiments on Shakespeare, XIII and XIV, it is found again in this case, as we have noticed before, that the transmogrification of a passage of poetry tends to arouse the motor consciousness more than does the original poem.

From the above results, it seems clear that the short vowels and the explosive consonants, regardless of accented or unaccented position in the poetic foot tend to produce the strong motor arousals; but this was not the case with the earlier experiments in which the single line was repeated five times in succession; nevertheless, the summation of effects is evidently what accounts for it, together with other factors not to be overlooked. The motor setting preparatory to tapping a long passage of verse is different from the motor setting which merely repeats the same line over and over again; and with the appearance of new combinations a stronger effect is produced by the addition of like elements than by a great variety of elements giving no effect of homogeneity; once the feeling produced by the short vowels and explosive consonants is aroused, even the lessening of their number per line in the following lines might not show as soon in the motor consciousness as it did in the introspective consciousness; instances of this we have seen in the previous pages. But it does not seem to work the other way around,—the effect of explosive sounds is immediate upon the motor consciousness, and one such sound can mar the effect of an otherwise placid and liquid line, and this may account for the apparent partial lack of definite

one to one correspondence which we have sought for in connection with our study of the motor energies and the introspective consciousness both singly and together.

Allied to the characteristic form-quality in the graphs for each individual poet, especially in the more meaningful lines experimented upon, is the matter of the tapped strokes as they appeared upon the smoked paper ribbon. After they had become accustomed to the tapping, every one of the subjects tapped in what could be called a thoroughly individual manner. Some of them tapped slowly and with great deliberation, thereby making a visible record of very rounded loops; others would react by a very quick down-stroke, followed by a slow, hesitating up-stroke, while still others would tap strokes that appeared on the paper as very fine points, or even in some cases would move the finger so quickly that the pointer climbed the roller on the up-stroke and returned with sudden relaxation of the rubber band in such a way as to make a loop in the smoky surface of the ribbon. And here lies the interesting point: that in the variously individual records there appeared evidence of all felt and unfelt changes in the emotional character of the experiments presented; tenseness of the vocal apparatus as well as the opposite state could be told by the experimenter as well as by the subject, together with subliminal effects of one sort or another which the subject did not feel either in summation or otherwise. Illusions, also, of various character were there evidenced, such as temporal and numerical ones. In general, the qualitative and quantitative aspects of the visible record amply supplemented the introspection in every way.

We have made no special mention of the time element in connection with most of these experiments. This is because the graphs are so typical for each and all of the subjects, that individual mention is unnecessary; furthermore, the time element does not seem to play any very important rôle. It certainly is no special correlate of any of the affective elements in consciousness; and it does not seem to be a manifest index either of difficulty in the material to be recited or of the number of sounds in the decasyllabic line. The subjects were all told to take their own

time in the tapping; this was merely to assure them that they were not to be hurried in what they did. This, however, is to be noted as the regular temporal manifestation of all the subjects: the repetition of the same iambic line five times usually showed on the record as having taken longer time with each repetition,—that is, the oftener repeated, the slower it became, though none of the subjects were aware of it. This may have been due to a number of things: either slight muscular fatigue, or else to the fact that as the impression aroused the introspective and the esthetic consciousnesses more and more, less and less nervous energy was sent per impulse per unit of time into the finger. That it was not due to imperfections in the machinery is clearly shown by the fact that the ribbon was allowed to pass several inches before the pointer was dropped upon it and the signal to begin was given.

A very pertinent question to be asked about all this work is,—“What had the subject’s general condition, mental and otherwise, to do with the results of the experiment?” A careful record was kept all during the second and third year as to how the subject felt at the beginning of the experiment and the results showed that the main effects of fatigue and other sub-normal states were of several kinds: 1. A less high degree of pleurability can be aroused in the state of fatigue; 2. The mean variation of the tapings on fatigue days is less than on normal days, but 3, that the subject did not reverse the results of the previous experiments at all,—those who showed a positive correlation showed it still, and those who before had showed either a definite negative correlation, or a scatter and miss correlation also continued to do so; the more the experiment develops, the more it seems that we were getting motor correlations with respect to the vocal apparatus, rather than results which attached significance to the total psycho-neural mechanism. But to return to the matter of fatigue days, only one of the subjects, L., tended ever to nullify his previous results, but then he also attempted to guess at his own type of correlation, and this guess may have influenced the tapings for that day.

Not every anticipation or conjecture with which this work

began has been verified by the experiments so far presented. But that the first statement of the thesis was not so dismally at fault is at once evidenced by the introspection on these large transmutations of English poetry; the tonal elements of the poetic line do seem indeed to have the power of arousing a mood congruous to that of the original poem, even when torn from their positions and their rhetorical anchorage, and recast into such form as is shown in the above experiments. The subjects did not know at the time what poems were being given them in this potpourri manner; they only knew it was some poem, and that they were to introspect upon it; but it was not a guessing contest in any sense of the term,—all intimations that it was to be such were stifled at once; and to the subjects must be given due credit for their admirable interest in the experiment from start to finish, for in such fragile matters as the moods of the esthetic consciousness, any hostility or any lack of true scientific interest would have been fatal to the purpose in hand.

4. THE PSYCHO-PHYSIOLOGICAL VALUE OF THE POETIC SUM

The question of a tonal calculus seems to be the logical development of the foregoing experimentation. It has been shown that short vowels and explosive consonants are provocative of more motor arousal than the long vowels and the liquids. Strictly speaking, as has been indicated before, the term "long vowel" is equivocal. Except, of course in vocal music, where the long notes rightly function their enunciation. But if the question be asked: can we say that this or that number of sounds will produce this or that effect? the answer cannot be given in the affirmative without the following reservations: effects can be calculated, provided the number and arrangement of the sounds be taken into consideration. In the above experiments it appeared that if a number of explosive sounds began the line or the passage, then the motor manifestations were intense, and also that such manifestations did not wear away as soon as the type of sound had changed to some less intense one. Changes in the apperceptive

consciousness did not either run parallel with the motor pattern nor did they very often seem to be influenced by the finger as much as by the page of print. Yet the introspective and motor results were parallel in other ways, as has been mentioned so frequently before.

Only so far as we had data from simple vowel and consonant experiments, could a tonal calculus be made. And so, when even in the simplest of the I-XII experiments, there were found sounds upon which no previous experimentation had been done, their values were not known in the same way as the values of the simple sounds previously used, and no two lines of the I-XII experiments could be found which had the same common parts known and unknown, in respect to psychomotor value.

Much thought and time was given to this matter, and for a while it looked as if we had arrived at a solution of the problem involved. But it had to be given up, and for this reason: that while in nearly all the cases tried, the psychomotor values of the separate letter sounds as found in the ninety-six preliminary experiments upon the single vowels and consonants showed in summation to be equivalent to the psychomotor values of the first three experiments performed upon each of the poets, and that as more and more meaning came into the experiments the sum was affected by some other element,—yet inasmuch as we did not have enough tonal elements to make a full correlation, and inasmuch also as the later poets experimented upon did not give favorable results, presentation of data and pressing of proof is withheld at this time. We had but four long vowels and no short ones, and it is likely also that the average motor effect of the consonants we obtained would have been greatly modified by further experimentation with other vowels, both long and short. Hence this problem of poetic sums remains for the time being unsolved; were this experimentation to be repeated, that problem would stand uppermost in the attempts at correlation.

Only in the longer passages does there seem to be a trend toward a tonal calculus. And here, the surprising thing is that a very small number of explosive sounds in one passage over

those in another produced a motor difference greater than that deducible from the single effects of the elements involved. A very rich experimental field lies right here, and, with the method and results of this experiment herewith presented, ripe and free for exploitation.

Futhermore, with the tabulations of sound frequency before him, one could build up by the method of experiments I-III such experiments which would signify and also contain the tonal body of any of the poets contained in it, and then, by comparing the results thus obtained with those from large "*ex poematis*" passages see whether parallel effects were thus obtained. The tonal pattern of poetry is quite more definite than hitherto suspected, and a poet may be known by his overtones as well as by his subject-matter and stanza form.

Little need be said in conclusion other than what we have given as results in preceding pages. Upholders of the tonal theory of poetry may take a fastidious pride in some of the findings of this experimentation, and recollect that Edmund Burke's theory of poetry may again be referred to without apology. Certainly it is not the intention of this paper to neglect the formal element of the matter, even if the constancy of a rhythm form was used for the purpose of neglecting the form in the final account. It is not an impossible assumption that poetry as well as other forms of art may possess in each of their leading features, form and content, a sufficiency of emotional wealth to be considered each alone as able to arouse the esthetic consciousness to the full. The union of the two may add nothing but unity,—and hence all such experimentation as the above is perhaps more of a training in the direction of attention than it is a splitting of the elements of art asunder. Nevertheless, this must be left for the consideration of those who are better qualified to decide it than the writer.

Our study is completed for the present. So far as we know, no such work has ever been attempted previously; let us hope that future experimentation along the same line will profit by our mis-

takes and lead our results to something finer and more conclusive. Eight years of work culminate in the results we have brought forward, in which years eighteen thousand lines of poetry were phonetically measured and tabulated, involving the enumeration of nearly 540,000 sounds; the measurement of the records obtained in the laboratory involved nearly 300,000 bits of data; the computation of the mean, the mean variation and the range for all the experiments and the making of rank lists brings the total number of computations to more than a million, and with all this labor, it might seem to some that far more should have been found out concerning the psychophysics of poetry than we have to offer in closing. But the introspective consciousness and the motor, too, are not such things as can be coerced and cajoled,—all their laws are by no means sun clear, and to have found out something definite is better than to have been given only shadowy hints, promises, and false signs to advance.

There were in all fifteen persons who took part in this investigation. One of these was an instructor in the Department of Psychology. The rest were mostly graduate students in the Laboratory. Five were women from Radcliffe College. All were trained introspectors.

The following scheme shows what subjects took part in the investigation and for how long time:

Yr. I	A	B	C		F	L		N		T	W	Z	Y
Yr. II	A	B	C	D	F	K	L	M		P	S		
Yr. III		B			F	K	L	M					

Thus three continued through the whole period, and the some five subjects assisted during the last two years, in which by far the most important work was done.

The following account briefly indicates the chief characteristics of the subjects:

A. Predominantly visual; disliked the tragic and melancholy; closed his eyes whenever possible; nodded head synchronously with the tapping; often read in a slightly mournful tone; it was usually unpleasant for his own personality to be injected into the imagery; good sense of rhythm; very constant and steady.

B. Visual-motor type; enjoyed the tragic and melancholy as much as the light romantic; acquainted with English poetry, favored Byron, Keats and Arnold; he alone of all the subjects gave much introspection of the sensations of the speech apparatus; gave much introspection; good sense of rhythm; steady and constant.

C. Motor type; rarely got satisfying imagery of any sort; had great difficulty to count the five iambics in the "la-mo" type of experiment; sense of rhythm varied much with the type of experiment; introspection meagre; steady and constant.

D. Very visual, with highly colored images; artistically gifted and fond of poetry; enjoyed the bizarre as well as the sombre; rather volatile, but rebounded instantly from depressed states; strong sense of rhythm.

F. Visual-motor; fond of poetry; good declaimer, and often varied from a steady recitation of the material experimented upon; articulation sensations often seemed to determine the imagery; German: had some slight difficulty in pronouncing the "th" and other sounds; strong sense of rhythm; constant.

K. Visual-auditory-motor; musical performer; esthetic; liked the melancholy; voice usually of medium pitch but very low intensity; pitch constantly noticed; feeling of hoarseness accompanied low pitches; tapped very short strokes, often no more than 12 mm. in length; syncoped the tappings very frequently; good sense of rhythm; constant.

L. Visual-motor; artistic, and fond of certain kinds of poetry, *e.g.* the sound of Shelley's and the content of Arnold's; introspection varied much, from bare feeling-tone to full auditory-visual-motor content; very apt in describing vague content by fitting analogy; good sense of rhythm; steady and constant.

M. Motor type; practical, and impatient of most poetry; often given to intentional changes of extent of finger movement; wanted objective finger control (the most inconsistent subject as far as any feeling-tone = motor-discharge correlation was concerned); said: "I have a good sense of rhythm," which did not always appear.

N. Motor-visual; philistinian toward most poetry; frequently interrupted the experiment with a Phillipic on the impracticability of art; good introspector; good sense of rhythm; fairly steady and constant.

P. Motor type; musical; singer; enjoyed the less romantic forms of poetry; meagre imagery; left-handed (the apparatus was accommodated to him); tapped the longest strokes of any one (140 mm.) with the smallest M.V.; good sense of rhythm; steady and constant.

S. Visual-motor; enjoyed poetry of all kinds; introspection often by tactual analogies; had difficulty with the language, being a native of India; good sense of rhythm; fairly steady and constant.

T. Predominantly visual; fond of all kinds of artistic work; introspection clear, often chromatic; sense of rhythm well marked; steady and constant.

W. Visual; practical and little acquainted with or appreciative of poetry; counting the five iambics often very difficult; tapping never become pleasantly automatic; steady and constant.

Z. Visual-motor; enjoyed poetry and was somewhat gifted in verse-making; lack of imagery in the introspection often disappointing enough to change the whole feeling-tone; steady and constant.

Y. Visual; acquainted with English poetry; introspection rather meagre; strong likes and dislikes; good sense of rhythm; steady and constant.

My thanks are due to both Professor Hugo Münsterberg, director of the Harvard Psychological Laboratory, and to Dr. Herbert Sydney Langfeld, Instructor in Psychology, for their kind and continual interest in and criticism of this work. And to the subjects who took part in the investigation much praise is rightly bestowed for their perserverance and interest.

THE HARVARD LABORATORY OF ANIMAL PSYCHOLOGY AND THE FRANKLIN FIELD STATION

ROBERT M. YERKES

With two figures

It is now fifteen years since the Director of the Harvard Psychological Laboratory, Professor Hugo Münsterberg, made a place for experimental work on the psychology of infra-human organisms in his laboratory. In 1899, two rooms in Dane Hall were assigned to students of animal psychology, and under the direction of the writer, three investigations were conducted. To meet the needs of an increasing number of workers, an additional room was made available in 1902.

In December, 1905, the laboratory equipment, together with all experimental work in psychology, was transferred to a newly and specially planned and constructed laboratory in Emerson Hall. Here, five rooms, in addition to the Instructor's office and a large amount of space in an unfinished attic, were available for work with animals. The following account of the facilities afforded for this work is quoted from a description of the Harvard Psychological Laboratory, published in 1906:

"Several rooms are fitted up with special reference to the investigation of the various forms of organic movement, animal behavior and intelligence. As one result of several investigations in animal psychology already pursued here, the laboratory has a considerable number of devices for testing and making statistical studies of the senses and intelligence, methods of learning and emotional reactions of animals.

"Adequate provision is made for the keeping of animals in a large, well-lighted, and well-ventilated corner room. Instead of having aquaria built into the room, an aquarium-table eighteen feet long has been constructed to support movable aquaria of various sizes. Whenever it is desirable for the purposes of an investigation, any of these aquaria may be moved to the research-

¹ The Harvard Psychol. Studies, vol. 2, p. 35.

room of the investigator or to such quarters as the special conditions of the experiment demand.

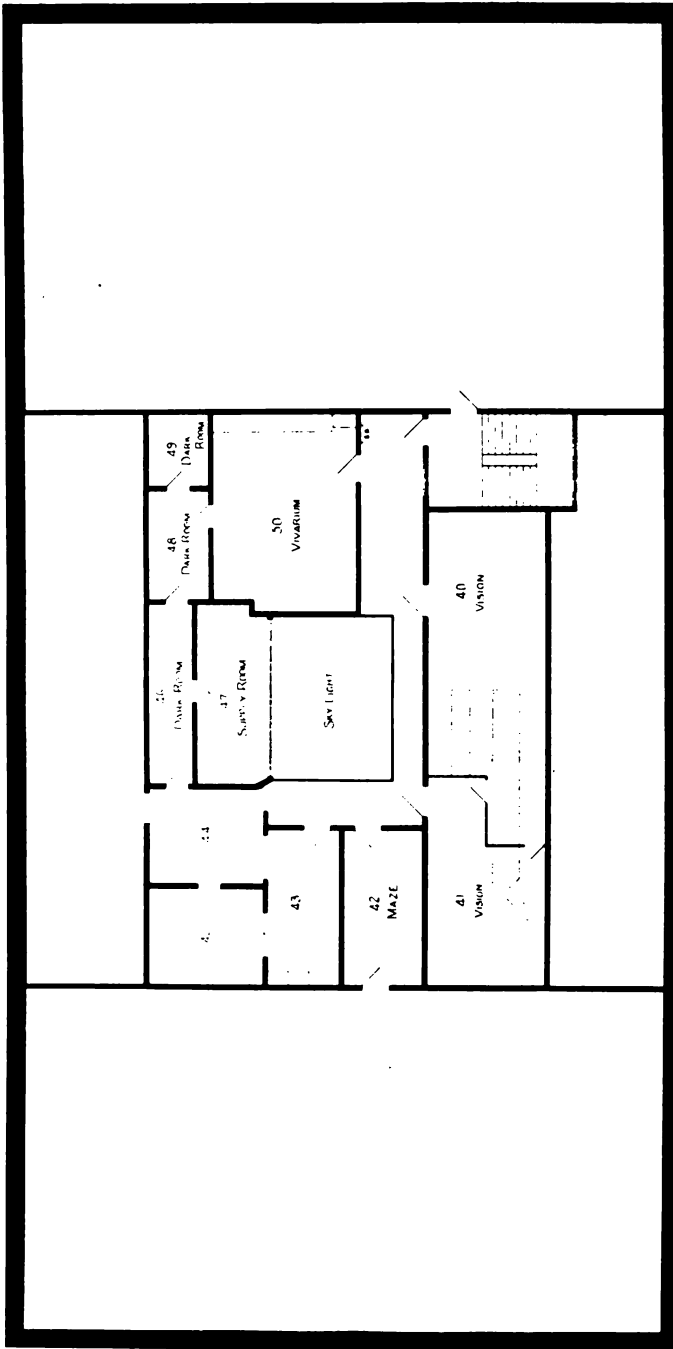
"The vivarium-room contains, in addition to provisions for water-inhabiting animals, cages of a variety of forms and sizes. The largest of these cages, six and a half feet high, six feet wide, and four feet deep, may be used for birds, monkeys, or any of the medium-sized mammals. Cages for rabbits, guinea-pigs, and other small animals are arranged in frames which support four double compartments. Similarly, small cages suitable for mice, rats, and other small rodents are in supporting frames which carry four of the double cages, each of which is removable and may be carried to the experimenting-room at the convenience of the experimenter.

"In a large unheated room above the main laboratory are tanks for amphibians and reptiles. These tanks, since they can be kept at a low temperature during the winter, are very convenient and useful for frogs, tortoises, and similar hibernating animals."

Work progressed satisfactorily in these quarters until the spring of 1913, when the introduction of experimental work in Educational Psychology, rendered desirable a redistribution of space. During the summer of 1913, the unfinished fourth floor of Emerson Hall previously referred to was developed, in accordance with plans prepared by the writer, as a laboratory of animal psychology. The floor plan of this new laboratory is presented in the accompanying figure 1.

Ten rooms, in addition to an office for the director of the work, are now at the service of students of animal psychology. Of these rooms, several were especially planned and have been at least partially equipped for definite lines of inquiry. Thus rooms 40 and 41 have been built about the Yerkes and Watson apparatus for the study of the several aspects of vision in animals. Preliminary studies of vision by simpler rough and ready methods are conducted in other rooms of the laboratory, or at the Field Station described below, and the more elaborate apparatus is used only for accurate and thorough-going investigations. By means of our varied visual equipment, it is possible to study color, intensity, size, form, and distance perception with a degree of exactitude which heretofore has been exceptional in connection with studies of animal behavior.

Room 42 is equipped with the Watson circular maze and the



LABORATORY OF ANIMAL PSYCHOLOGY

FIGURE 1. Floor plan of the new Harvard laboratory for the study of animal psychology.

Yerkes and Kellogg graphic record device. The latter enables an observer to obtain accurate records of distance and errors, in addition to those of time, in all maze experiments. Thus, the value of the maze-method is trebled. This improved apparatus demands stability, and, although it may readily enough be moved from room to room, it is eminently desirable to have a suitable place reserved for it, so long as the maze method maintains its present importance and promise as a comparative method and offers so many obvious possibilities of improvement.

The rooms numbered 43, 44 and 45 are daylight rooms as is also 42, which may be employed as occasion demands. At present, two of them are used for studies of problems of heredity in rats and mice. Later, the Hamilton insoluble problem multiple choice apparatus and the Yerkes soluble problem multiple choice apparatus will be installed in this group of rooms. These devices demand a special recorder-room. It is our purpose to install the recorder for both outfits in one room while placing the respective reaction devices in separate rooms. These two sets of multiple choice apparatus will render possible in this laboratory or at the Field Station (since we propose so to construct the apparatus that it shall be readily movable) the study of ideational reactions, in a variety of animal types, in such wise as to furnish directly comparable data of reaction.

The line of dark-rooms numbered 46, 48 and 49, is especially convenient because it may be used either in sections or as a whole. A supply of compressed air is delivered to room 49, and it is intended that in this room, in conjunction with room 48, there shall be installed apparatus demanding air under constant pressure for varied studies of olfaction and audition.

A store room, number 47, provides adequate space for supplies in the shape of food stuffs, bedding or litter, small cages, and packing or transportation boxes. Storage space for larger apparatus and materials is afforded by a room to which entrance is given by the doorway indicated in room 42.

Finally, room 50 is the "animal living room" of the laboratory. The floor of this room is water proof so that cages and aquaria may be thoroughly washed and the floor flushed at need. In this vivarium are set cages for a variety of vertebrates. At present, the laboratory is supplied with cages especially designed for mice, rats, guinea pigs, rabbits, cats, monkeys and birds.

A large aquarium table, upon which any desired form of aquarium may be placed, provides for the housing of amphibians and fishes.

The writer's students' training course in animal psychology is conducted in a class-room and lecture room on the third floor of Emerson Hall. The space of the laboratory on the fourth floor is, therefore, wholly available for research.

The rooms of the new laboratory are supplied with water, gas, compressed air, and a variety of electric currents. The latter are conveniently delivered from boards located in each room. In every room there are available 110 volt direct and alternating currents, as well as currents from Edison storage batteries which are located in the battery room of the main laboratory. A conveniently placed and well constructed switch board (S. B. of Figure 1) in the corridor of the laboratory, provides for the distribution of these storage currents. This board is fitted with miniature Weston switch board voltmeter and ammeter, and with taper plugs.

Realizing the extreme need for apparatus in animal investigations which shall, in a large measure, eliminate the experimenter from the situation to which the animal is expected to respond, the writer, in planning this new laboratory, has attempted so to arrange spaces that automatic setting, actuating and recording devices may readily be placed in rooms adjoining those in which the animal is responding. Heretofore, the majority of students of animal behavior have deemed themselves competent and able to observe and record accurately the doings of their subjects. That this, however, is not the case is clearly proved by numerous instances of misobservation and misinterpretation of reactions. We have, for example, twice discovered in this laboratory that dogs which were presumably responding to a definitely arranged experimental situation were actually responding to certain unconscious movements of the experimenter. The only safe and sure way to avoid such risks is to provide mechanical recorders which shall at least enable the experimenter to separate himself widely from his reacting subject.

We have striven for flexibility and adaptability in this new laboratory of animal psychology while arranging for the development, in designated spaces, of specific forms of apparatus. So far as the conduct of experimental work under highly con-

trollable and reasonably controlled conditions is in question, the laboratory, with its instrumental equipment, is excellent. But in addition to the ever present need of the development of new methods and the opportunity for the advantageous installation of new apparatus, the writer has felt as a still more urgent and important need, the supplementation of the laboratory by facilities for field work.

It would appear to be self-evident, yet the attitude of many experimental students of animal behavior seems to contradict the statement, that every student of animal life should be familiar with the objects of his interest in nature as well as in the laboratory; that he should possess, as a basis for evaluating the results of experiments, intimate knowledge of the instincts, habits, temperaments, and habitat of whatever type of organism he happens to be using for experimental purposes. The writer is fully convinced that naturalistic observation, or field work, should be held alike by naturalists and experimentalists as of equal importance with experimental observation, and should be regarded as an indispensable supplement to the latter. There are naturalists, to be sure, who decry all observation of animal behavior made under experimental conditions, whether within or without the walls of a laboratory, and there are experimentalists who deny the value of naturalistic work, or ignore it. But surely the last decade has furnished abundant proof of the unprofitableness of these attitudes. We propose, so far as is possible, in connection with our laboratory studies of animal behavior, to attempt to unite the naturalistic and the experimental points of view and methods.

The Harvard Psychological Laboratory is particularly fortunate in having the use of a field station in Franklin, New Hampshire, at which naturalistic studies on any organism which will thrive in a temperate climate may be pursued. This station consists of a tract of about one hundred and fifty acres of hill land, of which about half is wooded. The elevation is fourteen to fifteen hundred feet. There are numerous springs and a brook on the tract. Two sets of old farm buildings are available for such needs as arise. This tract, which is constituted by two old farms, was purchased by the writer in the years 1911 and 1912 to serve both as a summer home and as a reservation which might, as seemed desirable, be used for studies in animal behavior

It is proposed that this private field station shall meet two keenly felt needs of the Harvard Laboratory; the one, that of a suitable place for purely naturalistic field work; and the other, that of a similarly suitable place for the conduct of laboratory investigations which cannot well be continued during the summer in Cambridge. We may consider, first, the second of these needs.

There are frequently in progress, in the Harvard Laboratory, researches on heredity or on problems which demand long experimental training, the interruption of which, during the summer vacation, entails serious loss. It is often impracticable to attempt to continue such investigations throughout the summer in Emerson Hall, for even if the investigator is willing to work there, it usually means a serious sacrifice on his part of opportunity for rest and recreation through a change of scenes. The Franklin Field Station, it is hoped, will result in the saving of considerable time to certain investigators, since there it should be possible to continue work uninterruptedly throughout the summer, while at the same time the investigator may profit by the change from city to country and the chance to combine experimental and naturalistic studies in animal behavior with the recreations of a mountainous country.

It is by no means intended that all of the investigations conducted in the Harvard Laboratory shall be transferred to the Field Station. Instead, only a few should or can, to advantage, be so transferred.

But of primary importance, as contrasted with its value as a place for transferred experimental investigations, is the opportunity which the field station offers for naturalistic work. In and about the Cambridge Laboratory, favorable opportunities for training students to observe animals carefully, critically, and at the same time sympathetically, in their native habitats, are rare. And the writer has observed, in many otherwise admirable students of the biological sciences, a tendency toward the acquisition of a narrow minded attitude toward experimental observation, which blinds them to the value of nature-study. It is hoped that at Franklin something may be done for at least a few students of animal behavior to counteract this tendency and to train them to become enthusiastic and reliable naturalists as well as skilled experimentalists.

There is one obvious reason why, at the Field Station, any one



FIGURE 2. Views of the Franklin Field Station for the study of animal psychology.

of scores of invertebrates and vertebrates should not be observed under conditions of varying degrees of freedom. The country is already rich in animal life. Indeed, the Pemigewassett Valley, in which the station is located, is well-known to ornithologists because of the abundance of birds. It will undoubtedly prove feasible, as occasion arises, to import organisms for study.

The station is at present available during the months of June, July, August and September. It is at this time that work in Cambridge can least satisfactorily be conducted. The climate at Franklin is healthful and agreeable. For a few days in July the heat is at times oppressive, and before the end of September, frosts are likely to chill the enthusiasm of the field worker and to encourage his return to the city laboratory.

Only a small group of observers can be received at the Field Station during the summer. Each individual is responsible for his living expenses, and for the present at least, he must be responsible, also, for such expenditures as the conduct of his work demands. The station, as stated above, is the property of the writer, and is by him, in his capacity as director of studies in animal behavior at Harvard, placed at the service of a selected group of investigators during the summer.

Behavioristic work was initiated at the Franklin Field Station in the summer of 1912, by a study of habit-formation in earthworms, conducted by Ada W. Yerkes and the writer. This was a continuation of work begun previously in the Harvard Laboratory.

Two investigations were pursued during the second season (June to October, 1913) by Mr. C. A. Coburn and the writer. Of these, the one was a study of the transmission of savageness and wildness in mice, and the other, a naturalistic and experimental investigation of the behavior of the crow. The first of these was transferred for the season from the Harvard Laboratory. The second was a new inquiry which indeed could be conducted to advantage only at the Field Station. Both of these investigations prospered most encouragingly during the season, and we confidently expect and hope that they may be continued during the coming summer. Mr. Coburn, in a paper which appears in this number of the *Journal* (p. 185), has given a preliminary account of the results of certain of his experiments with crows. The naturalistic data which we obtained are reserved

for later presentation in connection with observations which we hope to make next summer. This paper on the crow initiates a series of contributions from the Franklin Field Station which should, in invaluable ways, supplement our studies from the Harvard Laboratory.

THE HEREDITY OF SAVAGENESS AND WILDNESS IN RATS

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In 1910 Professor William E. Castle suggested to me the desirability of studying the heritability of savageness and wildness in certain strains of rats which were being bred for studies in the heredity of structural characteristics at the Bussey Institution. I undertook the proposed investigation, and by the generous aid of Professor Castle and Doctor John C. Phillips I have been enabled to test the behavior of nearly three hundred individuals. The investigation is incomplete, and in this paper I propose to present merely a preliminary report, reserving the detailed account of my work, with the experimental data, for a paper to be published later, in some journal of genetics.

At the outset I made a preliminary analysis of the behavior of some of the rats in order to discover several traits which seemed to be fairly isolable and capable of reasonably accurate measurement. As a result of these observations, I decided to make tests of the savageness, wildness, and timidity of wild rats, tame rats, and of the first and second generation hybrids.

Preliminary attempts at measurement indicated that six grades, with respect to these several traits, might be utilized. These grades I designated as 0, 1, 2, 3, 4, and 5. The grade 0 indicates the absence of the various signs of savageness, wildness, or timidity. The grade 5 indicates the presence of these signs in maximal number and intensity.

In order to obtain a rough measure of the reliability of my judgments, I tested the individuals of several litters of rats with respect to the three traits designated and, later, without knowledge of my previous results, retested the same individuals. A comparison of the measurements thus obtained indicated that they were often the same and seldom differed by more than a grade. The results given below are typical.

No. of rat	Date	Savageness	Wildness	Timidity
59♂	Jan. 10.....	4	4	3
	" 17.....	4	3	2
69♀	" 10.....	3	3	4
	" 17.....	2	3	4
63♀	" 10.....	3	3	4
	" 17.....	3	3	3

The preliminary analysis of the behavior of rats, and my measurements, convinced me that I might profitably undertake a systematic study of savageness, wildness, and timidity in wild, tame, and hybrid individuals. Of these three traits, or possibly I should say, combinations of traits, timidity is the most difficult to recognize and satisfactorily measure. It is indeed extremely doubtful whether it can with sufficient certainty be distinguished from wildness to render measurements significant. I have attempted, however, throughout the investigation, to measure it and I shall report the results along with those for the other traits.

My method of testing the rats was to place a cage containing individuals to be examined on a table in the center of an otherwise unoccupied room. I then removed an individual from the cage in order carefully to observe its behavior. This removal was effected by means of my gloved hand, when that method could safely be used, or, in the case of extremely savage animals, by means of a pair of placental forceps which were used to grasp the animal by the tail.

The chief indications of savageness noted and relied upon as a basis for grading are (1) biting; (2) exposing or gnashing the teeth; (3) jumping at hand or forceps; (4) squeaking.

Similarly, the chief indications of wildness are (1) attempts to hide from view in cage or in hand; (2) random and excited running about in the cage or excited attempts to escape from the hand or the forceps; (3) squeaking; (4) urination and defecation.

Timidity is indicated (1) by attempts to avoid the experimenter; (2) by a kind of chattering or gnashing of the teeth; (3) by cowering and what looks like trembling; (4) urination and defecation.

From my notes, I reproduce the following statements concerning these several traits of behavior. "Savageness is of two

kinds, defensive and offensive. Of each there are several indications. The former deserves a higher grade than the latter. Defensively savage individuals are likely to jump at the observer and cannot be safely handled even with the gloved hand. Offensively savage rats may safely be handled: it is necessary only to avoid hurting them. Wildness almost invariably accompanies savageness. Timidity may or may not. An extremely savage and wild rat may exhibit little fear of the experimenter. A savage and aggressive wild rat fights, whereas a timid rat cowers, trembles, and chatters."

The animals observed, numbering about three hundred (300), consisted of wild rats, tame rats, and first and second generation hybrids.

The wild rats were captured either in Belmont or in Cambridge, Massachusetts, and were, with one exception, adult males. Observations and tests on them, made in several instances immediately after capture and again after they had been in captivity for a year, indicated extreme savageness and wildness, with variable timidity. The grade of 5 for savageness was assigned to almost all of these individuals. In wildness, they were graded either 4 or 5, and in timidity 3, 4, or 5. As a result of their confinement in cages for a year, they exhibited a lower grade of wildness and timidity, but their savageness remained unchanged. It was impracticable and wholly unnecessary to repeat frequently the tests on these wild individuals.

The tame rats were taken from a strain in use for studies of coat color at the Bussey Institution. This strain has been bred in the Harvard Zoölogical Laboratory for at least ten years. A brief account of some recent experiments with these rats has been given by Professor Castle in a paper entitled "Some biological principles of animal breeding."¹

As a result of certain experiments in selective breeding, two types of animal, each of which was used in my experiments are distinguishable in this strain. They are known as wide (W) and narrow (N) individuals. Both have black heads (hoods), but in the wide the black extends further back than in the narrow. The wide are known to have more wild blood than the narrow, and in these experiments they prove to be wilder and more savage.

¹ *American Breeders' Magazine*, 1912, vol. 3, no. 4.

Tests of savageness, wildness, and timidity were made with eight male and eighteen female tame rats (some wide, some narrow). Each individual was tested twice, the tests being separated by an interval of one month. In no case did the males receive a grade above 0. All were so gentle and tame that they could readily be taken up in the ungloved hand and examined. The females were decidedly less gentle and tame than the males. Two of the eighteen tested received a grade of 1 for savageness and fourteen of the eighteen received a grade of 1 for wildness. The number receiving a grade above 0 for timidity was twelve.

The contrast between the wild and the tame rats with respect to savageness, wildness, and timidity is extremely marked.

The first generation of hybrids was obtained in almost all cases by crossing a wild male with a tame female. This mating is much more satisfactory, because more likely to yield offspring, than the mating of a wild female with a tame male. By crossing the first generation hybrids among themselves, without selection with respect to savageness, wildness, and timidity, the second generation hybrids were obtained. Up to the present, no third generation hybrids have been examined.

As the mating, numbering, and weaning of the rats used were attended to by Professor Castle and Doctor Phillips, the experimenter was wholly unprejudiced, while making his tests, by knowledge of the genetic relations of the individuals. Very rarely indeed did he know whether the individual under observation was a tame rat or a first or second generation hybrid rat. Thus, he was able to escape entirely the influence of possible presuppositions concerning the behavior of savageness, wildness, and timidity in heredity.

With a few exceptions, each individual was tested from three to five times, at intervals of several days. The first test was made, as a rule, at the age of about six weeks and the remaining tests usually covered a period of at least a month, sometimes two months. It was noted that in general the animals receive lower grades with repetitions of the tests. This is due in part to the experience of being handled, but even more to the fact that they become accustomed to seeing human beings and to being disturbed when fed or when the cages are cleaned. There is also some evidence that ageing has something to do with the change.

TABLE 1
RESULTS OF SUCCESSIVE TESTS OF FIRST GENERATION, F_1 , (NARROW \times WILD)
HYBRID RATS

Rat	Age	Date	Savageness	Wildness	Timidity
19♀	58 days	Sept. 25.....	5	4	4
		Oct. 2.....	4	4	3
		" 17.....	3	4	3
		" 24.....	1	2	2
20♀	58 days	Sept. 25.....	5	5	5
		Oct. 2.....	5	5	4
		" 17.....	5	5	4
		" 24.....	2	3	2
94♀	45 days	Aug. 7.....	1	1	1
		" 13.....	2	2	2
		Sept. 25.....	1	3	2
		Oct. 2.....	0	2	2
21♂	58 days	Sept. 25.....	5	5	5
		Oct. 2.....	4	4	3
		" 17.....	3	4	3
		" 24.....	2	3	2
22♂	58 days	Sept. 25.....	5	4	4
		Oct. 2.....	4	4	3
		" 17.....	2	3	2
		" 24.....	1	3	2
80♂	50 days	June 20.....	0	2	1
		July 5.....	1	2	1
		Aug. 7.....	0	1	1
		" 13.....	0	0	0

In tables 1 and 2 are presented, for contrast, typical results obtained with groups of F_1 (first hybrid generation) males and females and F_2 (second hybrid generation) males and females.

The individuals of these tables are all the offspring of crosses between narrow tame rats and wild rats. The results indicate (1) diminishing savageness, wildness, and timidity with repetitions of the tests; (2) sex differences; (3) marked differences for the two generations. The F_1 individuals grade much higher, on the average, in savageness, wildness, and timidity than do the F_2 rats.

The results for seventy-eight F_1 individuals are summarized in table 3. All of these individuals were the offspring of narrow by wild crosses. The table presents, in the first horizontal line, (a) the average age; (b) the range in age of the group; (c) the average number of tests; (d) the range of tests; (e) the average

TABLE 2
RESULTS OF SUCCESSIVE TESTS OF SECOND GENERATION, F_2 , (NARROW \times WILD)
HYBRID RATS

Rat	Age	Date	Savageness	Wildness	Timidity
52♀	75 days	Jan. 24.....	0	3	2
		" 31.....	0	1	1
		Mar. 11.....	0	0	0
		" 18.....	0	0	0
55♀	75 days	Jan. 24.....	0	0	1
		" 31.....	0	0	0
		Mar. 11.....	0	0	0
		" 18.....	0	0	0
64♀	80 days	Sept. 27.....	5	5	5
		Oct. 7.....	5	4	4
56♂	75 days	Jan. 24.....	0	3	2
		" 31.....	0	2	2
		Mar. 11.....	0	1	1
		" 18.....	0	1	1
53♂	75 days	Jan. 24.....	0	3	2
		" 31.....	0	3	3
		Mar. 11.....	0	3	1
		" 18.....	0	2	1
62♂	80 days	Sept. 27.....	5	5	5
		Oct. 7.....	5	5	4

grade attained in the first test for savageness; (f) in the last test for savageness; (g) the average grade for all tests (that is the average for the total number of tests given to the group). The same three values are given also for wildness and for timidity. Immediately below these averages appears the distribution of the rats in the grades 0 to 5.

In tables 3, 4, 5, and 6 the results for males and females are presented separately. Tables 3 and 4 present the results obtained from the offspring of narrow tame by wild crosses, and tables 5 and 6 those obtained from the offspring of wide tame by wild crosses.

It is apparent from table 3 that the F_1 narrow by wild individuals of both sexes grade high in savageness, wildness, and timidity. Without exception, the females grade higher than the males. Thus, the first test for savageness yielded the grade of 4.39 for the females and 3.45 for the males. This result is typical. The lower grades attained in the last test are noteworthy. If we designate the grade which is most frequent as

TABLE 3

SUMMARY OF RESULTS FOR FIRST GENERATION HYBRIDS, F_1 (NARROW \times WILD)

	Age		No. of tests		Savageness			Wildness			Timidity			
	Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Av.	
	42.45	25-91 da.	3.76	2-4	3.45	1.6	2.52	4.19	2.86	3.24	3.74	2.43	2.9	
42 F ₁ males	Distribution of rats in grades 0-5.				0	4	19	5	0	1	0	0	1	0
					1	6	3	8	0	4	1	2	9	3
					2	2	3	6	3	10	10	7	10	14
					3	2	10	9	6	12	11	6	15	9
					4	11	7	11	13	15	18	12	7	16
					5	17	0	3	20	0	2	15	0	
36 F ₁ females	48.75	25-81 da.	3.67	1-4	4.39	2.5	3.31	4.44	3.22	3.78	4.19	2.64	3.33	
	Distribution of rats in grades 0-5.				0	0	7	0	0	0	0	0	0	
					1	2	6	3	1	2	0	1	6	0
					2	0	5	7	0	10	3	3	13	10
					3	3	4	8	3	9	11	2	7	8
					4	8	8	12	10	8	13	12	8	14
					5	23	6	6	22	7	9	18	2	4

the modal grade, we have, in the case of the first tests for both males and females, 5 as the modal grade. In other words, this group of F_1 hybrids attain the maximal grade of savageness, wildness, and timidity with modal frequency.

Turning now to a comparison of the results of table 3 with those for the second generation hybrids as presented in table 4, we discover, first of all, that the F_2 individuals, numbering one hundred and fifteen, grade very much lower on the average in savageness, wildness, and timidity than do the F_1 hybrids. A comparison of the results for the two sexes indicates a marked difference in that, whereas the F_1 females grade higher than the males, the F_2 males grade higher than the females. With respect to the distribution of individuals there is a great difference for the two generations, for whereas the F_1 individuals attain as their mode the grade of 5, the F_2 individuals in no instance attain a grade higher than 2 as the modal grade, and in most cases it is either 0 or 1. Thus, it may be noted in table

TABLE 4

SUMMARY OF RESULTS FOR SECOND GENERATION HYBRIDS, F_2 (NARROW \times WILD)

	Age		No. of tests		Savageness			Wildness			Timidity		
	Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Av.
	50.76	21-112 da.	4.	1-7	1.37	.7	1.	2.37	1.83	2.04	1.91	1.46	1.63
46 F ₂ males	Distribution of rats in grades 0-5.			0	24	34	27	0	6	2	1	8	2
			1	7	5	7	12	17	15	17	22	24	
			2	2	1	5	16	12	17	20	7	13	
			3	4	2	2	11	4	6	4	6	3	
			4	5	1	2	3	4	3	1	2	4	
			5	4	3	3	4	3	3	3	1	0	
69 F ₂ females	46.87	21-112 da.	4.2	1-7	1.17	.49	.74	2.17	1.52	1.77	1.84	1.28	1.42
	Distribution of rats in grades 0-5.			0	37	56	45	2	10	8	1	10	7
1			11	4	10	20	26	17	30	37	33		
2			6	1	6	21	23	31	25	17	24		
3			5	5	4	19	7	9	8	3	3		
4			7	2	3	4	3	4	2	2	2		
5			3	1	1	3	0	0	3	0	0		

4, that the modal grade for all of the averages under savageness is 0; under wildness or timidity, 1 or 2.

The results of tables 5 and 6 stand in striking contrast with those of tables 3 and 4. Although the numbers of individuals resulting from wide by wild crosses are small for both the first and the second generation hybrids, the differences which appear from comparison of tables 3 and 4 with 5 and 6 indicate clearly the influence of the wild blood in the wide tame parent.

We note from table 5 that fifteen F_1 individuals yield average grades which are about as high as those for the F_1 narrow by wild. There is slight difference, however, in the case of the F_1 wide by wild individuals for the sexes. The modal grade for savageness, wildness, and timidity is seldom below 4 for either males or females. In two cases it is 3.

The second generation of wide by wild individuals grades nearly as high as the first generation and is thus in marked contrast with the second generation of the narrow by wild

TABLE 5
SUMMARY OF RESULTS FOR FIRST GENERATION HYBRIDS, F_1 (WIDE \times WILD)

	Age		No. of tests		Savageness			Wildness			Timidity			
	Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Av.	
	42	40-46 da.	3.44	1-5	3.78	2.78	3.44	4.44	3.11	3.78	4.	2.89	3.44	
9 F ₁ males	Distribution of rats in grades 0-5.				0	0	3	0	0	0	0	0	0	0
					1	1	0	3	0	3	0	0	0	0
					2	1	0	0	0	0	3	2	3	3
					3	1	0	0	2	0	0	1	5	0
					4	2	5	2	1	5	2	1	0	5
					5	4	1	4	6	1	4	5	1	1
6 F ₁ females	43	40-46 da.	4.00	3-5	4.	2.5	3.33	4.33	2.83	3.67	3.83	2.5	3.17	
	Distribution of rats in grades 0-5.				0	0	2	0	0	0	0	0	0	0
					1	1	0	2	0	2	0	0	1	0
					2	0	0	0	0	0	2	2	1	2
					3	0	1	1	2	1	1	0	4	1
					4	2	3	0	0	3	0	1	0	3
					5	3	0	3	4	0	3	3	0	0

rats. There is no very marked constant difference for the sex groups. The modal grade ranges from 0 to 4. It is most frequently either 2 or 3. This apparently indicates that savageness, wildness, and timidity are of lower grade development in the second generation than in the first generation of wide by wild individuals.

Finally, in table 7 are presented the results for the F_1 as contrasted with the F_2 narrow by wild hybrids, the sex groups having been combined. In this table, the averages for the first test alone are given. This test appears to be in all respects the most reliable measurement of the several traits. As appears, the first generation hybrids approximate the average grade of 4 in savageness, wildness, and timidity, whereas the second generation hybrids approximate the average grade of 2. The modal grade for the first generation individuals is 5 in the case of all three traits. For the second generation individuals it is 0 in the case of savageness and 2 in the cases of wildness and timidity.

TABLE 6
SUMMARY OF RESULTS FOR SECOND GENERATION HYBRIDS, F₂ (WIDE × WILD)

	Age		No. of tests		Savageness			Wildness			Timidity		
	Av.	Range	Av.	Range	1st test	Last test	Av.	1st test	Last test	Av.	1st test	Last test	Av.
	.34 88	26-50 da.	3.18	1-4	3.06	1.82	2.24	3.71	3.29	3.35	3.18	2.71	2.65
17 F ₂ males	Distribution of rats in grades 0-5.				0	2	6	2	0	0	0	0	0
					1	0	3	3	0	0	0	1	1
					2	2	2	6	1	3	2	4	7
					3	7	3	3	5	7	8	5	8
					4	3	0	1	9	6	6	5	4
					5	3	3	2	2	1	1	2	0
<hr/>													
23 F ₂ females	40.09	26-50 da.	2.96	1-4	3.26	2.13	2.65	3.74	3.09	3.3	3.3	2.35	2.74
	Distribution of rats in grades 0-5.				0	1	5	2	0	0	0	0	0
					1	3	3	2	0	1	0	1	4
					2	2	7	7	2	5	4	5	12
					3	4	2	5	6	12	11	6	4
					4	9	4	5	11	1	5	8	1
				5	4	2	2	4	4	3	3	2	2

TABLE 7
GENERAL SUMMARY OF RESULTS FOR FIRST AND SECOND GENERATION HYBRIDS, (NARROW × WILD)

78 F ₁ males and females		Savageness first test 3.92	Wildness first test 4.33	Timidity first test 3.97
Distribution of rats in grades 0-5.	0	4	0	0
	1	8	1	3
	2	2	3	10
	3	5	9	8
	4	19	23	24
	5	40	42	33
<hr/>				
115 F ₂ males and females		Savageness first test 1.27	Wildness first test 2.27	Timidity first test 1.88
Distribution of rats in grades 0-5.	0	61	2	2
	1	18	32	47
	2	8	37	45
	3	9	30	12
	4	12	7	3
	5	7	7	6

The results thus briefly presented in tabular form prove conclusively that savageness, wildness, and timidity are heritable behavior-complexes. It is hoped that the further study of these characteristics in the third generation hybrids, and in special matings from the first and second generation hybrids, may yield more definite results concerning the modes of transmission.

THE BEHAVIOR OF THE CROW, *CORVUS* *AMERICANUS*, AUD.

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Many years ago, Henry Ward Beecher remarked that if men were feathered out and given a pair of wings, a very few of them would be clever enough to be crows. This statement represents in a general way the opinion of the mental ability of the crow held by many students of bird life. The literature, both early and late, abounds with anecdotes depicting the intellectual superiority of the crow over other birds.

During the last two decades investigations have been made, by the United States Department of Agriculture and several state boards of agriculture, to determine whether the battle waged by the farmer against the crow is justified. The results of these studies tend to show that the value of the crow to the farmer by its destruction of injurious insects, mice and other rodents, more than compensates for the injury it does to the growing crops. These studies have also provided interesting data on the habits and mental characteristics of the crow. The data, derived in this manner, in no way contradict the general impression. It is, in general, indicated that the crow is very intelligent, supremely cautious and suspicious. Forbush states that, in his opinion, it naturally is neither very cautious nor suspicious, but bold and fearless. Its apparent traits have been acquired by force of necessity. The reason for his statement is that on the Pacific Coast, especially during the early period of settlement, the crows were extremely bold and unsuspicious.

No definite study of the mental ability of the crow was made until 1910, when James P. Porter used three crows in his investigation of intelligence and imitation in birds. His results

¹ Forbush, E. H. *Useful Birds and their Protection*. Published under the direction of the Massachusetts State Board of Agriculture. 1907.

² Porter, James P. *Intelligence and Imitation in Birds: A Criterion of Imitation*. *Amer. Jour. of Psychology*, 1910, vol. 21, pp. 1-71.

did not put the crow on a higher plane of intelligence than several other birds, especially the English sparrow.

In co-operation with Professor Robert M. Yerkes, an investigation of the intelligence of the crow was begun in June, 1913, at the Franklin Field Station. Work was continued until late September. It is planned to continue the investigation in succeeding summers under the favorable conditions of the station.

The first summer's work included a general study of the habits and development of the bird (to be reported after additional data have been obtained) and a preliminary examination of its ability to discriminate brightnesses, sizes and forms.

It soon became apparent that the adaptation of an apparatus and method to the extremely wary and suspicious nature of the crow was a more difficult task than had been anticipated. This was accomplished after much experimenting with different methods of procedure and many changes in the apparatus. By the time both method and apparatus were fairly well adapted to the characteristics of the crow, the summer was well gone.

Our results are only approximations to the crow's discriminating ability. They are of value, however, in that they indicate certain important tendencies. A comparison of the results obtained during the first weeks with those obtained the last few days clearly shows the effect of improvement in method.

Two crows were used in these experiments. They were taken from a nest near the Field Station on the 6th of June. They were then, probably, about two weeks old. Number 1, a male, was larger and better developed when caught. When full-grown it was larger and bolder and less easily frightened than the female, Number 2.

For two or three weeks after they were caught, the young birds were fed earthworms, with an occasional bit of cooked cereal. Gradually this diet gave way to various kinds of meat, bread soaked in milk, cracked corn soaked in water, and table scraps.

The development, care, and feeding of young crows, will be discussed in a later paper.

When the two crows were about nine weeks old, they were able to fly a short distance and to eat alone. They were so tame that they recognized the voice of the experimenter and would come when called, perch on his arm or shoulder, and eat from

his hand. This friendliness was shown to no other person, and an entire stranger would frighten them very much.

Four other crows were obtained from Pennsylvania, but they were too wild for use in the investigation.

The building, in which the experimenting was done, was divided into two compartments, each 10 feet by 12 feet. One of these served as a roost and feed-room. Adjoining this room was a fly, 24 by 10 by 8 feet high, made of chicken wire. The crows could fly direct from the roost to a perch in the far end of the fly.

The other compartment, which served as an experiment-room, was set off from the feed-room by a partition of chicken wire and a burlap curtain. The curtain could be pulled aside when experiments were not in progress, thus allowing a free circulation of air.

The apparatus used was a modified form of the discrimination-box used by F. S. Breed¹ and later by L. W. Cole² in their studies of the reactions of chicks to visual stimuli. The following description is intended to give only the essential points of the apparatus. For a more detailed account, reference may be made to the reports of Breed and Cole.

The entrance-chamber was a movable box 18 by 16 by 14 inches deep. The top, bottom, and three sides were of one-half inch boards. The fourth side was covered with wire netting, one-fourth inch mesh. In each end were openings, 7 inches by 9 inches, with horizontal slide doors.

Leaving the entrance-box, the crow entered the discrimination-chamber. This was 16 by 19 by 13 inches deep. The top was of wire, one-fourth inch mesh. Opening directly into this chamber were two chambers, 18 by 19 by 13 inches deep. The tops of these chambers were of wood as were also the sides and floors. The exit from each of these chambers was 7 inches by 9 inches, with horizontal slide doors. They opened directly into two exit-boxes similar to the entrance-box. The front ends of the stimulus-chambers were formed by a three-stimulus plate-shifter sliding in wooden tracks. For a minute description of this shifter, the reader is referred to the papers of Breed

¹Breed, F. S. Reactions of Chicks to Optical Stimuli. *Jour. of Animal Behavior*, 1912, vol. 2, pp. 280-295

²Cole, L. W. The Relation of Strength of Stimulus to Rate of Learning in the Chick. *Jour. of Animal Behavior*, 1911, vol. 1, pp. 111-124.

and Cole. The stimulus plates used in the experiments on size and form, were the standard plates devised by Yerkes and Watson for their brightness vision apparatus and are described in detail in their paper.

The floor, walls, and top of the discrimination-chamber and the two stimulus-chambers were painted a dark gray. This rendered the two stimulus-chambers alike in every way except with respect to the desired difference in optical stimuli, namely, that of brightness, size, or form. Care was taken throughout the work to see that this was the only means by which the crow could choose the correct path.

The exit doors were operated by a system of cords. A curtain was suspended from the ceiling at the rear of the apparatus. The experimenter, standing behind the curtain and looking through a small peep-hole, could observe the behavior of the crow while in the apparatus and open and close the exit-doors without being seen by the crow.

Late in the summer, two swinging gates of wire were suspended between the discrimination-chamber and the two stimulus-chambers. These gates also were operated by cords. At the beginning of a test they were drawn up to the ceiling of the discrimination-chamber. The purpose of these gates was to prevent the crow from returning into the discrimination-chamber after it had made a wrong choice.

During the experiments on brightness discrimination, the apparatus faced a north window. With the beginning of the tests of size discrimination, it was shifted to face a larger south window. In this position, it remained during the rest of the season.

The ability of the crow to detect a slight change in the situation, together with its wary and suspicious nature, made it impossible to choose a method of procedure at the beginning and to adhere to it rigidly throughout the period of work. The method used at the beginning was evolved during the preliminary trials, when the first indications were received of what the crow might reasonably be expected to do. Various changes were made in this initial method until a reasonably satisfactory one had been developed.

¹ Yerkes, R. M., and Watson, J. B. *Methods of Studying Vision in Animals. Behavior Monographs*, 1911, vol. 1, no. 2, p. 23.

For several days previous to the first preliminary series, the crows were compelled to enter the discrimination-chamber in order to get their food. For this purpose the apparatus was placed before a small door in the partition separating the feed-room from the experiment-room. At first, the pan containing the food, was placed just inside the entrance door. Then, gradually, it was placed farther back until the crows were required to go through the discrimination-chamber, and the one or the other of the stimulus-chambers, into the exit-boxes. After a few days, they did this with no apparent fear.

The first preliminary tests were given on July 16th. The crows were then about nine weeks old. The standard stimulus plates had been removed from the stimulus shifter, leaving square openings, 12 cm. by 12 cm. Opal flashed glasses were placed in the slides immediately before these openings, so the illumination of the two chambers was the same.

The apparatus was adjusted with the entrance-box before the small door in the partition between the feed-room and the experiment-room. When one of the crows had entered this box to get the bit of food placed therein, both doors were closed and the entrance-box was then placed before the entrance to the discrimination-chamber. The door leading to the discrimination-chamber next was opened and the crow allowed to enter. The exit doors being open the crow could proceed to one of the exit-boxes and obtain food. The exit and entrance-boxes were now exchanged and the crow given another trial.

Both crows were much frightened by being confined in the entrance and exit-boxes. After two days, with nine such trials, they became somewhat calmer during the experiments. The exit doors were now closed and the crows allowed to enter the discrimination-chamber, go to one of the stimulus-chambers and there wait until the exit door was opened. This new situation, especially the opening of the exit door, frightened them as much as being shut in the entrance or exit-boxes had at the beginning. In the first trial they could not be induced to enter the discrimination-chamber until the exit doors were opened as before. However, after eight trials with the doors closed, they had lost much of their fear. In these seventeen trials, Number 1 went eleven times to the right and six times to the left. Number 2 went every time to the right.

When the crow chose the correct path, it was always rewarded with a bit of food,—a small piece of mouse, frog, or other meat. If it chose incorrectly, it received no food and was required to remain three or four minutes in the exit-box, which had been previously darkened by a cloth thrown over the wire side. The dislike of crows to remain in a darkened chamber was utilized also by the gradual darkening of the entrance-box when the crows hesitated too long before entering the discrimination-chamber at the beginning of a test. This never failed to cause them to leave immediately. There were, therefore, at least two motives for correct choice, namely, the desire for food and the dislike of the darkened box. The latter can be considered a constant factor, for they reacted to the darkened box as strongly at the end of the summer as they did at the beginning.

Care was taken throughout the experiments to keep the factor of hunger constant. It was impossible to do this at all times, and it is highly probable that the results in many cases were materially affected by the change in this factor.

In the beginning, two series of five tests each were given per day. The times for the beginning of these series varied slightly, but as a rule they were 7:30 A. M. and 1:00 P. M. The crows, with this number of tests, would still be hungry at the end of the series, so the number of tests per series was raised to ten and the amount of food given at the end of each correct choice was lessened. It soon became apparent that the crow, in this case, was confined too long. After the seventh or eighth test, it usually busied itself more with getting out of the apparatus than with choosing the correct path in order to get food. On this account, three series, (7:30 A. M., 12:00 M. and 4:00 P. M.), of eight tests each were given per day. Finally the number of trials in each series was changed to five, and this seemed to be the best solution of the problem, as the crows were sufficiently hungry three times a day to be eager to get food. In the majority of cases, they were still hungry at the end of a series. The time required for the five tests was rarely over ten minutes, and the crows, as a rule, did not become restless in this time.

As a rule, one crow was given all the trials of a series before the other was caught. In a few series, the crows were given alternate tests. This was not conducive to the best results, for the crow, waiting in the entrance-box until the other completed

the test, would become so restless that in many cases it would begin to throw itself against the woven wire side of the box. By the time its turn came, the desire to escape from the box had entirely overcome the desire for food, and, as a result, it would rush through the test and recommence its struggle to free itself. If, by chance, it made a correct choice, the food would not be noticed.

The results of each series of tests were kept on record sheets similar to those used by H. C. Bingham • in his study of the perception of size and form in the chick. In addition to a record of the correct and incorrect choices, the time required for the choice and a sketch of the path of the crow were also recorded.

In the study of brightness perception, the apparatus remained as in the preliminary series except that the stimulus areas of the stimulus-chambers differed in intensity. This difference was obtained by the use of more or less opaque substances, namely, black cardboard, milk glasses, and paper. These were placed over the opal flashed glass of one of the stimulus areas. The slides, which held the plates of opal flashed glass before the stimulus areas, were large enough to admit also the cardboard, milk glasses, or sheets of paper.

Black cardboard was first used. Since it allowed no light to pass, the illumination of the stimulus area before which it was placed was practically zero. The crows, in the trial series, had become partially accustomed to stimulus areas of an intensity produced by light passing through but one thickness of opal flashed glass. Consequently in the brightness experiments, they avoided the darkened chamber. The chambers were darkened in no regular order, but in ten or twenty tests, one chamber would be darkened as many times as the other.

After fifteen tests with each crow, the cardboard was exchanged for two milk glasses, then later for one milk glass and finally for one sheet of paper. The difference in the intensity of the two areas in this last case was comparatively slight. With care it could be distinguished by the human eye.

Table 1 shows the results of these tests.

• Bingham, H. C. Size and Form Perception in *Gallus domesticus*. *Jour. of Animal Behavior*, 1913, vol. 3, no. 2, pp. 65-113.

TABLE 1

INTENSITY DISCRIMINATION

Date	No. of tests	Correct choices	
		Crow No. 1	Crow No. 2
Conditions of Discrimination			
Cardboard and opal flashed glass—Opal flashed glass			
July 19	5	5	5
" 19	5	4	5
" 20	5	5	5
Two milk glasses and opal flashed glass—Opal flashed glass			
July 21	5	5	5
" 21	5	5	5
" 22	5	4	5
One milk glass and opal flashed glass—Opal flashed glass			
July 22	5	5	2
" 23	5	3	3
" 23	5	4	4
" 24	5	4	4
" 24	5	5	4
" 25	5	3	4
" 25	5	4	5
" 26	5	5	5
" 27	5	4	5
" 28	5	5	5
" 28	5	5	4
One sheet of paper and opal flashed glass—Opal flashed glass			
July 29	5	4	3
" 30	5	5	3
" 30	5	5	3
" 31	5	5	3
" 31	5	5	4
Aug. 1	5	5	4
" 1	5	5	5

These results are but roughly indicative of the crows' ability to distinguish differences in illumination. Accurate measurements of the birds' visual acuity was not the aim of our experiments.

The chief value of these experiments on the discrimination of intensity is the demonstration of the ease with which the crow is able to adapt itself to experimental conditions and to solve accurately one variety of problem.

With the beginning of the experiments on size discrimination, the apparatus was so shifted that the front end was immediately before a large south window. In this position it remained during the season. The only other change was the insertion of the

standard stimulus plates in the stimulus shifter. Difference in the illumination of the stimulus areas was eliminated.

A 5 centimeter, circle versus a 2 centimeter, circle was chosen for the beginning of this study. The correct exit was indicated by the larger circle.

This change in the conditions of discrimination naturally threw the crows into confusion. They refused to enter the discrimination-chamber unless forced to do so by the darkening of the entrance-box. If this were done, they would pass to and fro before the two stimulus-chambers, but they would not enter far enough into either of them for the exit doors to be opened. The series of the first two days had to be interrupted on account of the crows' fright. On the third day no attempt was made to work. During the day the crows were fed somewhat less than the usual amount of food. The next morning (August 5th), they were tried with a 9 centimeter circle versus a 5 centimeter circle. By this change the illumination of the stimulus-chambers was made to approximate that to which the crows had become accustomed in the experiments on the discrimination of intensity. Their hunger, on this day, was great enough to overcome in large measure their fright. The results of this, and the remaining series on size discrimination are given in Table 2.

After one series with the 9 centimeter versus the 5 centimeter circle, a 2 centimeter circle was substituted for the 5 centimeter circle. The crows' behavior now became practically normal. The only significant difference from previous reactions was a greater hesitation in choosing. Before finally entering a chamber, they would often pass to and fro several times before the two stimulus-chambers, again and again starting to enter one chamber only to back out and go to the other. As appears in the table, crow no. 1 made twenty correct choices in succession, while crow no. 2 succeeded in choosing correctly eighteen times in twenty. This sudden return of calm and controlled reaction and the high percentage of correct choices, were due probably to the fact that the illumination of the stimulus-chambers through the 9 centimeter and the 2 centimeter circles was closely similar to that in the experiments on intensity discrimination.

* Stimulus plates will be designated by the diameter or the side.

It seems probable that the birds were simply choosing the more highly illuminated stimulus-chamber, which, in every case, was also the one presenting the larger stimulus area. That they did not continue to use this cue is proved by experiments in which the large stimulus area, and irregularly the small one also, were darkened by placing one thickness of milk glass over the opal flashed glass. This enabled the experimenter in some tests to present two stimulus areas differing in size and intensity of illumination. Now the chamber illuminated by the larger circular area was the more intense, and now the one illuminated by the smaller area. Had the crows attempted to depend upon the illumination of the chambers, or on the relative intensities of the stimulus areas, instead of on their size, they certainly would have been confused. As a matter of fact, the change influenced markedly neither their behavior nor their percentage of correct choices.

The experiments on the perception of size were continued for twenty-five days. The results (Table 2) show that the crows

TABLE 2
PERCEPTION OF SIZE

Date	No. of tests	Correct choices	
		Crow No. 1	Crow No. 2 -
Conditions of Discrimination			
5 centimeter—2 centimeter circle			
Aug. 2		Crows frightened.	Abandoned series.
" 3		Crows frightened.	Abandoned series.
9 centimeter—5 centimeter circle			
Aug. 5	5	3	4
9 centimeter—2 centimeter circle			
Aug. 6	10	10	8
" 6	10	10	10
5 centimeter—2 centimeter circle			
Aug. 7	10	10	10
" 7	10	9	10
" 8	10	9	9
" 8	10	7	10
" 9	10	5	9
" 9	10	10	9
5 centimeter—3 centimeter circle			
Aug. 10	10	8	9
" 11	10	8	7
" 11	10	9	10

TABLE 2—*Continued*

Date	No. of tests	Correct choices	
		Crow No. 1	Crow No. 2
Conditions of Discrimination			
3 centimeter—2 centimeter circle			
Aug. 12	10	9	7
" 13	8	4	4
" 13	10	6	7
" 13	8	8	5
" 14	8	6	6
" 15	7	6	6
" 15	9	7	8
" 15	8	5	5
" 16	8	7	5
" 16	8	8	8
" 16	8	6	5
" 17	8	6	4
5 centimeter—3 centimeter circle			
Aug. 17	8	8	6
" 18	8	8	8
" 19	5	3	3 (Left habit)
" 19	6	4	6
" 20	10	4 (Left habit)	8
" 20	10	8	9
" 20	10	7	10
" 21	10	9	10
" 21	10	5 (Left habit)	9
" 21	5	3	5
" 22	8	7	7
6 centimeter—3 centimeter circle			
Aug. 22	8	8	7
5 centimeter—3 centimeter circle			
Aug. 22	8	6	8
" 23	8	3 (Left habit)	8
" 23	8	6	7
" 24	10	9	6
" 24	10	10	9
" 25	10	9	9
" 25	10	9	9

improved surprisingly little with practice. The percentage of correct choices with the 5 centimeter versus the 3 centimeter circle was as low during the last few days of the training as it was on August 10th and 11th when they were first required to distinguish between these circles.

Throughout these experiments, the behavior of the crows while working was very erratic. Some days they worked slowly and carefully. Sudden noises, such as those caused by the opening or closing of an entrance or exit door, did not greatly

disturb them. The results on these days of calm steadiness, as a rule, showed an increase in the number of correct choices. On other days, their behavior would be practically the opposite. While still in the entrance-box they would walk impatiently to and fro before the woven wire side of the box. When the entrance door was opened, they would often start several times to enter only to turn back into the entrance-box. When they finally did enter, they would rush to one of the exit doors, and, in a crouching attitude, wait until it was opened. On these days, great care had to be taken in opening and closing the doors for an unusual noise or sudden movement would greatly increase their excitement. During this behavior they were very likely to develop a position habit. Series, in which this excited behavior resulted in a considerable number of incorrect choices, have been noted in the tables.

The ability of the crow to pass directly from one set of circles to another with no great difference in the number of correct choices (see Table 2), was further tested by a series of experiments, the results of which appear in Table 3.

In these experiments, the attempt was made to determine whether the crows were reacting to a certain specific stimulus, or whether they were reacting to it because of its relation to another stimulus. For instance, if the 6 centimeter and the 4 centimeter circles were presented, and the crow trained to react positively to the 6 centimeter circle, would it continue to do so when the 6 centimeter circle was presented with a 9 centimeter circle, or would it, instead, choose the larger area in each instance?

As in the preceding series the crows were trained to choose the larger of two circles. When they had gained the ability to choose correctly, they were given ten trials with a different pair of circles. During these ten trials, they were rewarded after each test, regardless of the correctness or incorrectness of the reaction. A reaction was considered correct if the crow chose the larger circle. These series are designated, in Table 3, "relative reactions." The training series which preceded the relative series of August 26th are given in Table 2.

The results of these experiments indicate fairly clearly the relativity of the crows' reactions. Especially is this true of crow no. 1. For example, on August 24th and 25th, when the

3 centimeter circle was presented with the 5 centimeter circle, the crow reacted to the 3 centimeter circle thirty-seven times negatively and three times positively. On August 26th, the 3 centimeter circle, displayed with the 2 centimeter circle, was reacted to positively in every case. The results for crow no. 1 with the 6 centimeter circle when displayed with the 4 centimeter and the 9 centimeter circles, on August 26th and 27th, were almost as decisive.

TABLE 3
REACTIONS TO RELATIVE SIZES OF CIRCLES

Date	No. of tests	Correct choices	
		Crow No. 1	Crow No. 2
Relative reactions, 3 centimeter—2 centimeter circle			
Aug. 26	5	5	5
" 26	5	5	3
Training series, 6 centimeter—4 centimeter circle			
Aug. 26	10	7	9
Relative reactions, 9 centimeter—6 centimeter circle			
Aug. 27	5	4	2
" 27	5	5	3
Training series, 6 centimeter—4 centimeter circle			
Aug. 27	8	2	5
" 28	10	6	8
" 28	8	5	3 (Right habit)
" 29	5	4	4
" 29	5	4	3
" 29	6	5	5
Relative reactions, 3 centimeter—2 centimeter circle			
Aug. 30	5	5	2
" 30	5	5	4
Training series, 6 centimeter—4 centimeter circle			
Aug. 30	5	5	2
Relative reactions, 9 centimeter—6 centimeter circle			
Aug. 31	5	2	4
" 31	5	4	3

Only one day intervened between the conclusion of the tests of the relativity of the reactions and the beginning of experiments to determine the ability of the crow to distinguish circles from triangles, squares and hexagons.

With the beginning of this study of form perception the experimenter became more convinced than ever that the results, obtained in the previous experiments, did not truly indicate the

crows' intelligence. A new form of reaction now developed. When either of the crows had made an incorrect choice and the exit door was opened, showing a dark exit-box, instead of entering as they hitherto had done, they would whirl about and quickly go to the other exit and there wait, even for five or ten minutes, until the door was opened. This behavior naturally tended to lower the percentage of correct choices.

The experimenter first tried to overcome this difficulty by having the exit-box illuminated until they had entered it. Crow no. 2 would always enter the box under these conditions, but crow no. 1, after a few trials, refused to enter either box unless there was a bit of food in view.

To meet this difficulty, the gates, described on page 188, were constructed. When the crow entered the wrong stimulus-chamber, the exit door was opened and at the same moment the gate between that chamber and the discrimination-chamber was dropped, thus preventing the crow from escaping to the other exit. The dropping of the gate tended to frighten them somewhat, so they always quickly entered the exit-box, which was again darkened as in the early experiments. The effect of this improvement in the apparatus on the behavior of the crows appears in the results of Table 4.

The crows had been given one hundred and six tests for their ability to distinguish a 6 centimeter circle from an 8.081 centimeter triangle. During these trials no appreciable increase in the percentage of correct choices had been made. Immediately after the gates were brought into use, improvement commenced and thereafter the majority of the choices were correct. Crow no. 2 did not make quite as high a percentage of correct reactions as did crow no. 1. This was probably because no. 2 seemed to be more frightened by the dropping of the gate. If an incorrect choice was made early in a series, there was a tendency, on the part of no. 2, to avoid that stimulus-chamber during the remainder of that series.

The 6 centimeter circle, the 8.081 centimeter triangle, the 5.317 centimeter square, and the 3.29 centimeter hexagon are of equal area. The last thirty tests were with figures unequal in size. The 6 centimeter and the 9 centimeter circles each possess a greater area than the 3 centimeter hexagon, whose area, in turn, is almost twice as great as that of the 3 centimeter circle.

TABLE 4
DISCRIMINATION OF FORM

		Correct choices	
Date	No. of tests	Crow No. 1	Crow No. 2
Conditions of Discrimination			
6 centimeter circle—8.081 centimeter triangle			
Sept. 2	6	5	3
" 2	5	4	3
" 3	5	3	4
" 3	5	4	3
" 3	5	4	3
" 4	5	4	2
" 4	5	5	2
" 4-8	45	39	32
" 8	5	5	2 (Right habit)
" 8	5	3	4
" 9	5	3	4
" 9	5	3	5
" 10	5	3	1 (Left habit)
" 10	5 (Began using gates)	3	5
" 11	5	5	4
" 11	5	5	5
" 11	5	3	5
" 12	5	5	4
" 12	5	5	5
" 12	5	5	5
6 centimeter circle—8.081 centimeter triangle (Inverted)			
Sept. 13	5	5	5
6 centimeter circle—5.317 centimeter square			
Sept. 13	5	5	5
" 13	5	5	5
" 14	5	5	3 (Left habit)
" 15	5	5	5
6 centimeter circle—4.243 centimeter square			
Sept. 15	5	5	5
" 15	5	4	5
" 16	5	5	5
6 centimeter circle—3.29 centimeter hexagon			
Sept. 16	5	4	4
6 centimeter circle—3.00 centimeter hexagon			
Sept. 16	5	5	5
" 17	5	5	4
3 centimeter circle—3.00 centimeter hexagon			
Sept. 17	5	5	3
" 18	5	5	2 (Left habit)
9 centimeter circle—3.00 centimeter hexagon			
Sept. 18	5	5	4
" 18	5	5	5

The intensities of the stimulus areas and the general illumination of the chambers were varied in these tests by the use of milk glasses as described on page 194. The only visual factor which was constant during the thirty trials, was that of form. It is evident, therefore, that this was the cue which enabled crow no. 1 to make a perfect record in these series.

Lack of time prevented further work on the perception of form. The last two days of work were devoted to a further study of size discrimination. The purpose was to obtain, if possible, more conclusive evidence of the crows' ability to distinguish sizes, and, incidentally, to learn if the improvement in the method (introduction of gates) would increase the percentage of correct reactions to differences in size.

Thirty tests were given, the results of which appear in Table 5. During the first series, the crows appeared to be confused by the sudden change in the problem presented to them. They worked rather slowly and quietly, but their choices were not made with the usual definiteness. It was evident that they (especially crow no. 1), did not clearly appreciate what was required of them.

TABLE 5
DISCRIMINATION OF SIZE

Date	No. of tests	Correct choices	
		Crow No. 1	Crow No. 2
Conditions of Discrimination			
5 centimeter—3 centimeter circle			
Sept. 19	10	4	7
" 19	5	5	4
5 centimeter—4 centimeter circle			
Sept. 19	5	5	5
5 centimeter—4.5 centimeter circle			
Sept. 20	5	4	4
" 20	5	5	5

In the second series, the indefiniteness and hesitation in their behavior were lacking. In every case no. 1 went quickly and directly to the correct exit. Crow no. 2 made a mistake in the first test. Its decisions, however, were made clearly and definitely thereafter. This clear-cut, decisive type of reaction continued, with both crows, during the remaining tests, even when the discrimination was between the 5 centimeter and the

4.5 centimeter circles. If quickness of choice can be taken as a measure of the ease of discrimination, it is probable that the crows are capable of distinguishing much smaller differences.

The crow deserves its reputation. It is an exceptionally interesting subject for the behaviorist and worthy of his greatest skill. As has been indicated earlier in this report, it is planned to observe systematically crows at the Franklin Field Station, both in the field and in the laboratory, in order that a reasonably complete and reliable description of their behavior may be given. Because of the division of labor among a number of observers, it will be necessary to publish reports from season to season instead of reserving all materials for a monograph. The present paper is indicative of some of the chief characteristics of the bird, and suggestive of experimental difficulties. Another season should prepare us to report on the habits, instincts, and development.

PRELIMINARIES TO A STUDY OF COLOR VISION IN THE RING-DOVE *TURTUR RISORUS*¹

ROBERT M. YERKES

Assisted by A. M. EISENBERG

From the Harvard Psychological Laboratory

At the present moment a thorough study of the visual reactions of a few types of birds and mammals is highly desirable. This paper presents an account of observations on the reactions of the ring-dove in the Watson-Yerkes color vision apparatus. The ring-dove was chosen as a subject because of its easy adaptation to laboratory conditions and its convenient size. It was hoped that it might prove an ideal bird for the intensive study of vision.

As a preparation for the study of color discrimination, the limits of the spectrum, and the stimulating values of various wave-lengths, observations were first made on the response of the bird to achromatic stimuli. The apparatus used throughout the preliminary work here reported was the Watson-Yerkes spectral color vision device, as described in volume one of the *Behavior Monographs*.² A Bausch and Lomb automatic arc lamp was used as a source of light, and a selenium cell, as described in the monograph (pp. 79-81) served as a means of measuring the energy of the stimuli employed. For the simple reaction-box shown as W in figure 7 of the monograph, the box represented in figure 1 of this paper was substituted, and instead of having two reflecting surfaces, M and L of figure 7 above referred to, fixed on the experiment-box and moving laterally with it, three reflecting surfaces were employed. These remained fixed while the experiment-box moved sufficiently to reverse the position of the two photic stimuli.

¹ This work has been made possible by a grant from the Bache Fund of the National Academy of Arts and Sciences, which enabled the author to complete the construction of a spectral apparatus and install a selenium cell outfit to measure chromatic stimuli. Grateful acknowledgment is made to the trustees of the Fund and to the Committee in charge, for the facilitation of this research.

² Yerkes, Robert M., and Watson, John B. *Methods of studying vision in animals. Behavior Monographs*, 1911, vol. 1.

The general apparatus need not be redescribed in detail. The reader who is unfamiliar with it is referred to the above-mentioned monograph and to Watson's more recent book.³ In brief, it consists of a source of light which, by means of a system of lenses, prisms, and slits, is made to supply chromatic stimuli in any desired quality or intensity. Two stimuli are presented to the subject simultaneously. The position of these stimuli may be reversed at the will of the experimenter. The subject is required to distinguish the stimuli and react differently to the two.

Assuming, now, that the reader has a general knowledge of the mechanism by which the chromatic stimuli are obtained, controlled, and measured, we may consider our method of procedure in its relations to the reaction-box of figure 1. This consists of an entrance chamber (A) in which, at the beginning of a series of observations, the subject is placed by the experimenter, and from which it passes, when the door (D) is raised, into compartment B, which may be designated the discrimination compartment. A sliding partition (M) enables the experimenter to avoid delay because of the unwillingness of the subject to enter B, for by raising the door (D) and drawing M slowly and steadily backward toward the rear of compartment A, the subject may, without disturbance, be compelled to enter the discrimination compartment. Once in B, the subject faces the two stimuli S, S. These are presented either with or without general overhead illumination, and they appear as illuminated surfaces, either chromatic or achromatic, 7 cm. long by 1.8 cm. wide. These two stimuli are separated by the partition P, of figure 1.

On the floor of each stimulus-box, E, are electrodes by means of which electric shocks may be given as punishment for failures to distinguish and properly to react to the two stimuli. The doors F, F, leading from the stimulus compartments into the alleys G, G, may be raised by the experimenter by means of the cords shown in the figure. When the subject enters the compartment which contains the stimulus selected by the experimenter as the positive stimulus, the appropriate door F is immediately raised, the slide-door, H, of the same side opened, and the subject thus permitted to pass by way of the alley G,

³ Watson, J. B. *Behavior*. New York, 1914, p. 70.

back to the starting point at A, where it is allowed to feed for a definite interval. In case, however, the subject enters the other compartment, it is not allowed to pass into the alley, but instead, either with or without the use of the electric shock, according to the experimenter's previous decision, it is required to retrace its steps and again attempt to distinguish the stim-

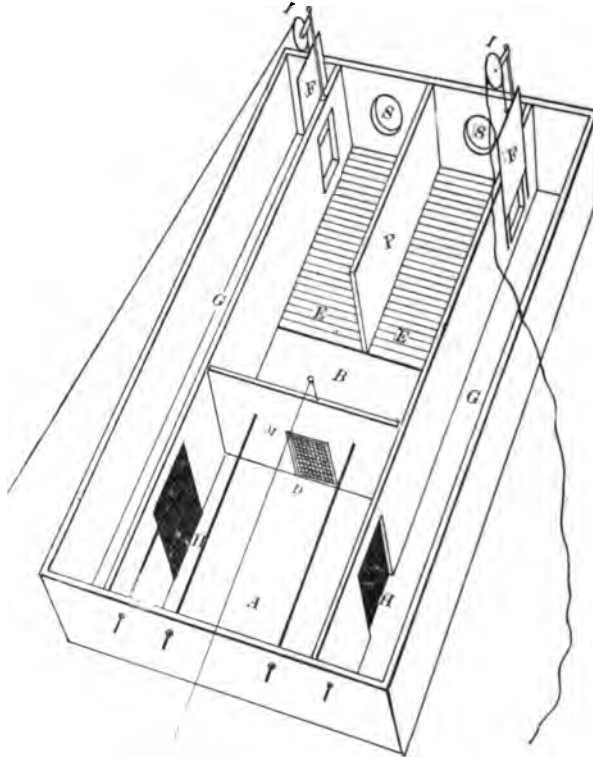


FIG. 1. Reaction-box for Ring-doves. A, entrance chamber; B, discrimination compartment; D, screen-door; M, sliding partition moving in A and B; E, E, stimulus compartments; P, partition between E and E; F, F, doors between stimulus compartments and alleys G, G.; H, H, slide-doors between G and A; I, I, pulleys for cords attached to F, F; S, S, stimulus apertures.

ulus which demands positive reaction from that which demands negative reaction.

In the case of the observations about to be described, achromatic stimuli were obtained by placing a two candle power carbon incandescent lamp 86 cm. from the stimulus area. Thus,

the one of the stimulus areas presented to the subject was always illuminated, whereas the other was entirely unilluminated, except as general illumination was employed in the experiments. Naturally, as the experiment-box was shifted from side to side, the more intense achromatic stimulus was presented now in the stimulus compartment on the right of the subject, now in the one on the left.

The writer is convinced that wherever possible interference of the experimenter in the course of an animal's should be obviated by the use of automatic or subject devices. It was not feasible, however, in the present situation, to introduce such devices,—consequently the slide-doors, as shown in figure 1. These, it should be proved surprisingly satisfactory in the case of the bird which is easily startled and which would not react to subject-actuated apparatus unless everything could be made to work steadily, quietly, and fairly slowly. It is, however, a question that our efforts in studies of behavior should eliminate, as far as possible, the necessity, during the reaction, of movements by the experimenter which modify the behavior of the subject. It has repeatedly been found that even the experienced investigator is liable, unable to supply cues to his subject which facilitate proper or even serve as the sole basis for what appears to be a reaction.

The birds used for the present work were obtained from a Boston dealer. All that could be learned about them was that they were young. We are therefore under the disadvantage of being unable to give a satisfactory description of the birds, obviously desirable in all such investigations that the sex and exact age, as well as the sex and history of the birds should be known. But this is somewhat less essential in the case of preliminary observation than in the case of continuation-work. Four birds were used. Two, supplied as male and female by the dealer, in the first series, appear as numbers 1 and 2 in this report. They were used over a period of several weeks by Mr. A. M. Yerkes. The others appear as number 3, a female, and number 4, a male. During a period of five months these birds were used in a visual experiment by the writer. The results ob-

numbers 1 and 2 will be presented only in contrast with those of numbers 3 and 4, since the conditions of use varied somewhat, and the experiments conducted by Mr. Eisenberg were not carried so far as those of the writer. The descriptions of general behavior in this paper will be based almost wholly upon the observations made on doves 3 and 4.

At the outset, it was assumed that the ring-dove would react satisfactorily in the discrimination apparatus, that it would exhibit a fair degree of docility, breed rapidly in captivity, be easy to handle, and endure close confinement well. It must be admitted that these assumptions have not all been justified, for the birds did not quickly adapt themselves to the experimental situations, and in docility they rank low. Indeed, their slowness in acquiring the discrimination habit demanded in this work was a great surprise to the writer. He is now somewhat uncertain as to whether it is desirable to attempt an intensive study of visual response with a subject which demands such a large amount of training.

Work was initiated by feeding the birds in the entrance chamber of the experiment-box, with all of the doors of the box open so that the subject might wander about at will. This was continued for a week, with the occasional variation of opening and closing the doors as the bird passed from compartment to compartment, so that it might become accustomed to the operating of the simple mechanisms and learn the route from the entrance chamber, by way of the stimulus chamber, back to the starting point.

During the second week of the preliminary observations, the birds were sufficiently tame and accustomed to the apparatus to work fairly well. They were regularly each morning required to make the trip through the apparatus three or four times, and they were rewarded for so doing with food. It was discovered that they would not make the trip quickly unless they were very hungry, and even in that condition their attention to the situation was very variable, and they were so easily distracted by slight noises or jars that the whole process was a very tedious one. It thus became apparent that unless an additional motive for discrimination and progress through the experiment-box could be discovered, the work would be most tedious. Consequently, at the beginning of the third week,

the electric shock was introduced as a means of compelling attention to the visual stimuli and of encouraging careful comparison and appropriate reaction. Even from the start, the electric stimulus served this purpose admirably. It at once rendered the birds more alert, careful, attentive, and active. The writer's notes record, "In two weeks the doves apparently have learned nothing, but to-day as the result of four trials with electrical stimulation, each seemed to attempt to discriminate between the light and the dark chambers.

It was decided, on the basis of the preliminary observations, that the doves should be required to choose the lighter rather than the darker of the two compartments.

Number 3, the female, was at the outset much less wild and more timid than number 4, the male. It was much easier for the experimenter to catch her in the living-cage than to catch him, but when in the experiment-box, she was very much more disturbed, excitable, and liable to discouragement than he. By contrast, then, the female may be described as tame and timid. the male as wild and bold. But it should be added that neither bird was sufficiently wild to be difficult to handle.

On February 28th, 1914, systematic, regular experiments were begun, with the use of both food and the electric shock. Both birds worked well in the six trials which were given. Only one bird was used at a time, and it was given its trials in succession, with from one-half to one minute interval for feeding between choices. In comment on this day's reactions, the writer's notes state that "The use of the electric shock discreetly and infrequently has transformed the birds from time-wasting and careless subjects to active, alert, constantly moving reactors. This modification of method evidently means a saving of an immense amount of time to the experimenter. It enables him to command the attention of his subject instead of having to beg for it by the offering of food. Food, however, is serving an excellent purpose in the work, for each bird comes to its task hungry and usually feeds between trials."

On March the 2nd, the number of trials for each bird was increased to ten, and it was subsequently found that as many as fifteen or even twenty trials could be given in succession without overfatiguing the subjects and with excellent results.

Table 1 presents two sample detailed records of the daily

trials from the writer's note-book. The first portion of the table gives the results of an early series of ten choices, those of March 4th. The remainder of the table presents, by contrast, the results of a later series of fifteen trials in which the birds were practically perfect in their discrimination. This series was given on April 19th. The table indicates, in the first column, the position of the positive stimulus, that is the stimulus indicative of the chamber to be entered. In the second column, the letters R and W designate, respectively, correct and incorrect choices.

TABLE 1
EXAMPLES OF DETAILED DAILY RECORDS

March 4, 1914, 10:10 A. M. With general illumination. Stimulus-lamp 86 cm from stimulus area. Coil at 1 cm. for female and 2 cm. for male.

Trial	Positive stimulus	Female, No. 3		Male, No. 4	
		Reaction	Remarks	Reaction	Remarks
1	Left	W	Shocked?	W	Shocked?
2	"	W	"	W	"
3	"	W	"	W	Shocked
4	Right	R	Direct	W	"
5	Left	W	Shocked?	R	Discrimination
6	Right	R		R	Anxious
7	"	W		R	"
8	"	W	Shocked	R	Eager
9	Left	R		W	No shock
10	Right	W		W	Shock
Summary:		4 R:6 R		4 R:6 W	

April 19, 1914, 9:50 A. M. With general illumination. Stimulus-lamp 126 cm. from stimulus area. Coil at 2 cm. for both.

		Female, No. 3		Male, No. 4	
		Reaction	Remarks	Reaction	Remarks
1	Left	R	Near-mistake	R	Exc. disc.
2	Right	R	Direct	R	
3	"	R	"	R	
4	"	R	"	R	
5	"	R	"	R	
6	Left	R	"	R	
7	"	R	Good disc.	R	
8	"	R	"	R	
9	"	R	Near-mistake	R	Careful
10	Right	R	Eager	R	
11	Left	R		R	
12	Right	R	Direct	R	
13	"	R	"	R	
14	Left	R	Near-mistake	R	
15	"	W	Careless	R	
Summary:		14 R:1 W		15 R:0 W	

TABLE 2

SUMMARY OF RESULTS OF TRAINING IN LIGHT-DARK DISCRIMINATION.
ELECTRICALLY ILLUMINATED AREA VERSUS UNILLUMINATED AREA.
ELECTRIC SHOCK USED AS PUNISHMENT.

Dove Number 3, ♀				Dove Number 4, ♂			
Date	Conditions	Right	Wrong	Date	Conditions	Right	Wrong
Feb. 28	Gen. ill., elect. stim....	3	3	Feb. 28	Gen. ill., elect. stim....	4	2
Mar. 1	No gen. ill., elect. stim.	3	3	Mar. 1	No gen. ill., elect. stim.	4	2
" 2	" " " "	8	2	" 2	" " " "	5	5
" 3	Mixed illum., " "	9	1	" 3	Mixed illum., " "	5	5
" 4	Gen. illum., " "	4	6	" 4	Gen. illum., " "	4	6
" 5	" " " "	6	4	" 5	" " " "	6	4
" 6	" " " "	4	6	" 6	" " " "	7	3
" 7	" " " "	4	6	" 7	" " " "	6	4
" 8	" " " "	6	4	" 8	" " " "	4	6
" 9	" " " "	3	7	" 9	" " " "	5	5
" 10	" " " "	6	4	" 10	" " " "	7	3
" 11	" " " "	5	5	" 11	" " " "	4	6
" 12	Mixed gen. ill. " "	4	6	" 12	Mixed gen. ill. " "	9	1
" 13	" " " "	6	4	" 13	" " " "	8	2
" 14	Gen. illum., " "	5	5	" 14	Gen. illum., " "	8	2
" 15	" " " "	3	7	" 15	" " " "	6	4
" 16	No gen. ill., " "	4	6	" 16	No gen. ill., " "	6	4
" 17	Gen. illum., " "	5	5	" 17	Gen. illum., " "	5	5
" 26	" " " "	3	7	" 26	" " " "	5	5
" 27	" " " "	9	1	" 27	" " " "	5	5
" 28	" " " "	4	6	" 28	" " " "	4	6
" 29	" " " "	6	4	" 29	" " " "	5	5
" 30	" " " "	4	6	" 30	" " " "	5	5
" 31	" " " "	4	6	" 31	" " " "	5	5
Apr. 1	" " " "	11	4	Apr. 1	" " " "	5	10
" 2	" " " "	9	6	" 2	" " " "	5	10
" 3	" " " "	8	7	" 3	" " " "	13	2
" 4	" " " "	9	6	" 4	" " " "	9	6
" 5	" " " "	8	7	" 5	" " " "	8	7
" 6	" " " "	12	3	" 6	" " " "	10	5
" 7	" " " "	9	6	" 7	" " " "	9	6
" 8	" " " "	11	4	" 8	" " " "	11	4
" 12	" " " "	11	4	" 12	" " " "	14	1
" 13	" " " "	13	2	" 13	" " " "	14	1
" 14	" " " "	14	1	" 14	" " " "	13	2
" 15	" " " "	10	5	" 15	" " " "	13	2
" 16	" " " "	12	3	" 16	" " " "	14	1
" 17	Stim. less, " "	11	4	" 17	Stim. less, " "	15	0
" 18	" " " "	13	2	" 18	" " " "	14	1
" 19	" " " "	14	1	" 19	" " " "	15	0
" 20	" " " "	12	3	" 20	" " " "	14	1

The general results of the several series of reactions required for doves number 3 and number 4 appear in table 2, under their appropriate dates. A brief statement is given in the second column of the table of the important conditions of reaction. It is stated, for example, whether general illumination was used

TABLE 3

SUMMARY OF RESULTS OF TRAINING IN LIGHT-DARK DISCRIMINATION.
ELECTRICALLY ILLUMINATED AREA VERSUS UNILLUMINATED
AREA. ELECTRIC SHOCK NOT USED

Dove Number 1, ♂				Dove Number 2, ♂			
Date	Conditions	Right	Wrong	Date	Conditions	Right	Wrong
Mar. 2	Gen. illum.....	3	7	Mar. 3	Gen. illum.....	2	3
" 3	" " " " " " " " " " " "	5	5	" 4	" " " " " " " " " " " "	2	3
" 4	" " " " " " " " " " " "	4	6	" 7	" " " " " " " " " " " "	2	3
" 7	" " " " " " " " " " " "	1	4	" 9	" " " " " " " " " " " "	2	3
" 9	" " " " " " " " " " " "	2	3	" 10	" " " " " " " " " " " "	2	3
" 10	" " " " " " " " " " " "	0	5	" 11	" " " " " " " " " " " "	2	3
" 11	" " " " " " " " " " " "	3	2	" 12	" " " " " " " " " " " "	2	3
" 12	" " " " " " " " " " " "	3	2	" 13	" " " " " " " " " " " "	0	5
" 13	" " " " " " " " " " " "	3	2	" 14	" " " " " " " " " " " "	2	3
" 14	" " " " " " " " " " " "	4	1	" 16	" " " " " " " " " " " "	2	3
" 16	" " " " " " " " " " " "	0	5	" 17	No gen. illum.....	1	4
" 17	No gen. illum.....	1	4	" 18	" " " " " " " " " " " "	2	3
" 18	" " " " " " " " " " " "	1	4	" 19	" " " " " " " " " " " "	2	3
" 19	" " " " " " " " " " " "	1	4	" 20	" " " " " " " " " " " "	1	4
" 20	" " " " " " " " " " " "	2	3	" 21	" " " " " " " " " " " "	2	3
" 21	" " " " " " " " " " " "	1	4	" 23	No gen. ill., elect. stim.	1	4
" 23	No gen. ill., elect. stim.	2	3	" 24	" " " " " " " " " " " "	2	3
" 24	" " " " " " " " " " " "	2	3	" 25	" " " " " " " " " " " "	1	4
" 25	" " " " " " " " " " " "	3	2	" 26	" " " " " " " " " " " "	5	5
" 26	" " " " " " " " " " " "	2	3	" 27	" " " " " " " " " " " "	4	6
" 27	" " " " " " " " " " " "	2	3	" 28	" " " " " " " " " " " "	4	6
" 28	" " " " " " " " " " " "	3	2	" 30	" " " " " " " " " " " "	4	6
" 30	No gen. illum.....	4	6	" 31	" " " " " " " " " " " "	5	5
" 31	" " " " " " " " " " " "	5	5	Apr. 1	Gen. illum.....	5	5
Apr. 1	Gen. illum.....	2	8	" 2	" " " " " " " " " " " "	5	5
" 2	" " " " " " " " " " " "	6	4	" 3	" " " " " " " " " " " "	6	4
" 3	" " " " " " " " " " " "	1	9	" 4	" " " " " " " " " " " "	7	3
" 4	" " " " " " " " " " " "	4	6	" 6	" " " " " " " " " " " "	4	6
" 6	" " " " " " " " " " " "	8	2	" 8	" " " " " " " " " " " "	6	4
" 8	" " " " " " " " " " " "	5	5	" 9	" " " " " " " " " " " "	4	6
" 9	" " " " " " " " " " " "	5	5	" 10	" " " " " " " " " " " "	4	6
" 10	" " " " " " " " " " " "	2	8	" 11	" " " " " " " " " " " "	5	5
" 11	" " " " " " " " " " " "	5	5	" 13	" " " " " " " " " " " "	6	4
" 13	" " " " " " " " " " " "	5	5	" 14	" " " " " " " " " " " "	5	5
" 14	" " " " " " " " " " " "	4	6	" 16	" " " " " " " " " " " "	5	5
" 17	" " " " " " " " " " " "	7	3	" 17	" " " " " " " " " " " "	5	5
" 18	" " " " " " " " " " " "	5	5	" 18	" " " " " " " " " " " "	7	3
" 20	" " " " " " " " " " " "	2	8	" 20	" " " " " " " " " " " "	4	6
" 21	" " " " " " " " " " " "	3	7	" 21	" " " " " " " " " " " "	7	3
" 23	" " " " " " " " " " " "	10	0	" 23	" " " " " " " " " " " "	3	7
" 24	" " " " " " " " " " " "	6	4	" 24	" " " " " " " " " " " "	10	0
" 25	" " " " " " " " " " " "	1	9	" 25	" " " " " " " " " " " "	5	5
" 27	" " " " " " " " " " " "	10	0	" 27	" " " " " " " " " " " "	9	1
" 28	" " " " " " " " " " " "	10	0	" 28	" " " " " " " " " " " "	6	4
" 29	" " " " " " " " " " " "	10	0	" 29	" " " " " " " " " " " "	9	1
" 30	" " " " " " " " " " " "	10	0	" 30	" " " " " " " " " " " "	5	5
May 9	" " " " " " " " " " " "	6	4	May 1	" " " " " " " " " " " "	6	4
" 11	" " " " " " " " " " " "	9	1	" 2	" " " " " " " " " " " "	10	0
" 14	" " " " " " " " " " " "	6	4	" 2	" " " " " " " " " " " "	10	0
				" 4	" " " " " " " " " " " "	10	0
				" 9	" " " " " " " " " " " "	9	1
				" 11	" " " " " " " " " " " "	8	2
				" 14	" " " " " " " " " " " "	5	5

or not, and it is indicated that in a few series of observations the conditions of illumination were mixed, that is, for some of the reactions general illumination was employed, whereas in others it was lacking. Throughout the regular experiments the electric stimulus was employed. On April 17th, as is indicated, the intensity of the visual stimulus was lessened, thus diminishing the difference in the stimuli to be distinguished.

Table 3 presents the comparable results for doves number 1 and number 2. The chief difference in the conditions for these results and those obtained with doves numbers 3 and 4 is the absence of the electric stimulus in the case of the former. With the exception of one week, March 23rd to March 28th, Mr. Eisenberg trained number 1 and number 2 to achromatic discrimination on the basis of food as a reward without the use of the electric shock as punishment for mistakes. His results, therefore, may be compared with those of the writer, with a view to discovering the value of punishment as contrasted with reward in this experiment with ring-doves.

Such comparison indicates, in the first place, that it is possible to make a larger number of observations per series with punishment than without it. Thus, the writer by the aid of the electric stimulus was able to make ten, fifteen or even twenty observations per series. Whereas, Mr. Eisenberg, without the electric stimulus, could not satisfactorily make more than ten observations, and during a considerable portion of the training he made only five. Second, the time required for the work varied much more widely when punishment was not used than when it was used. As appears from tables 2 and 3, all of the doves acquired the ability to discriminate with a reasonable degree of certainty, and to react appropriately. The course of habit formation in case of each of the four subjects is surprising. Instead of being steady, regular, and fairly rapid, as the writer had anticipated, it proved to be irregular and extremely slow. One day the experimenter would feel confident that his subjects were acquiring the habit, and the next day he would find them utterly unable to react properly.

In table 4 the choices are presented by groups of fifty, and the course of habit formation is indicated with the daily variations eliminated. This table shows that as the result of three hundred trials, no one of the four doves had acquired the ability

TABLE 4

REACTIONS IN LIGHT-DARK TRAINING GROUPED IN FIFTIES TO SHOW SLOWNESS OF IMPROVEMENT AND IRREGULARITIES

Trials	Dove 1, ♂		Dove 2, ♂		Dove 3, ♀		Dove 4, ♂	
	Right	Wrong	Right	Wrong	Right	Wrong	Right	Wrong
1-50	18	32	18	32	32	18	27	23
51-100	18	32	20	30	23	27	29	21
101-150	22	28	21	29	23	27	35	15
151-200	23	27	28	22	25	25	25	25
201-250	20	30	25	25	27	23	22	28
251-300	25	25	26	24	30	20	29	21
301-350	37	13	36	14	34	16	34	16
351-400	36	9	41	9	41	9	46	4
401-450			27	8	38	12	47	3
451-500					30	5	33	2

to react properly. Between the three hundredth and the four hundredth trials, all of them, however, showed marked improvement. Were it not that two experimenters were involved and the conditions of observation thoroughly controlled, it might fairly be suspected that the doves finally discovered some other basis for reaction than the difference in the intensity of illumination. We are convinced, however, that this was not the case and that the results satisfactorily prove that the ring-dove is extremely slow, under the conditions described, in learning to react appropriately to achromatic stimuli, even though they differ very markedly. It must be admitted, however, that there are certain features in table 3 which are puzzling. Number 1 discriminated perfectly on April 23rd, and number 2 on April 24th, whereas on both the preceding and the following days they did poorly. This suggests to the writer that they had happened upon some means of choosing other than that intended by the experimenter.

From a careful comparison of the data of tables 2, 3, and 4, it is clear that by the use of the electric stimulus, it is possible to develop a visual discrimination habit in the dove much more quickly, and consequently with less labor, than by the employment of the food getting desire alone.

All of the foregoing observations are merely preparatory to the work with chromatic stimuli. It therefore seems unneces-

sary to burden the reader with further details of conditions or results, except possibly with respect to the general illumination and its relation to the reactions. In some of the series, general illumination was not employed, and it was naturally apparent that the doves could distinguish the stimuli much more easily than when the surroundings were illuminated. It was deemed desirable to use general illumination in order to guard against choice on the basis of the visibility of the sides and floor of the stimulus chambers, for naturally enough, this differed greatly in the light and the dark chambers in the absence of general illumination. On the whole, it seemed very much more satisfactory to conduct experiments in the general illumination produced by a two candle power frosted carbon incandescent lamp, at a distance of 110 cm. above the center of the partition between the stimulus chambers.

As an aid to rapid reaction, the alleys of the experiment were kept dark except at the moment of entrance of the bird. In each alley was placed a low-power lamp which could be turned on the instant the door F was raised, and turned off the instant the door H was opened. This served to induce the bird to take the alley-way and to hasten through it to the food. In a few daily series, the birds made the trip quickly and easily, seldom loitering in the passageways and usually going from entrance chamber to discrimination chamber.

The food placed in the entrance chamber as a reward for return to that portion of the experiment-box was usually bread, with a small quantity of cracked corn. In a large portion of the series, the birds ate little, and were practically deprived of food while in the living-chamber. It is fair to say that the process of habit formation in the doves 3 and 4 depended almost solely upon punishment, the process in the case of birds 1 and 2 depended almost entirely upon reward.

As in the writer's previous use of punishment, the current was used by means of a Porter inductorium number 6 Columbia dry cell as source of current. In the experiments, no attempt was made to keep the feet of the birds moist, and as a consequence, the secondary coil had to be well over the primary. Its position was varied somewhat.

day to day, but in general it was placed at 1 cm. for the female and at 3 cm. for the male. This, of course, means that the male responded to a very much weaker electric stimulus than did the female, but it is probable that this indicates not so much a difference in sensitiveness to the stimulus as the result of difference in weight, for the male bird was much heavier than the female. During March it was found difficult to get satisfactory responses, even when the maximum current was used, and the experimenter finally hit upon the device of placing a square piece of moist blotting paper before the food-box in the entrance chamber. This was found to yield very satisfactory results. The secondary now had to be set at 2 cm. for the female and $2\frac{1}{2}$ for the male. The settings proved satisfactory throughout the remainder of the work, and whereas previously the responses to the electric stimulus had varied extremely, they subsequently were very constant.

RESULTS WITH CHROMATIC STIMULI

Doves 3 and 4, having been trained to practically perfect discrimination of a bright area from a dark area of the same size, were tested for preference of spectral red and green. The value of the red stimulus was 626 to 640 $\mu\mu$, while that of the green was 498 to 510 $\mu\mu$. In energy, as measured by the selenium cell, the red stood slightly above the green, but they were so nearly the same that it seemed needless to attempt to equate them more closely for these preliminary experiments.

Table 5 presents in summary the results of the chromatic reactions of doves 3 and 4. From this table it appears that on April 21st, when given an opportunity to choose either the red or the green chamber, without punishment, number 3 chose the one as often as the other, whereas number 4 chose the red eight times, the green twice. On April 22nd, in the absence of general illumination and with a period of two minutes for darkness adaptation before the series was commenced, the results were entirely different, for number 3 selected the green nine times out of ten, while number 4 chose it five times out of ten. On the following day, the original conditions of April 21st were reinstated and the responses were similar to those of that

From these four series of ten reactions with doves numbers 3 and 4, it may be inferred that under the condition of general illumination in which these doves had been trained to distinguish the light stimulus patch from the dark and to react posi-

tively to the lighter of the two, the spectral red and green stimuli appeared of about the same intensity to the female dove, whereas to the male, the red appeared the more intense. One naturally infers that both birds, as a result of their previous training, would go to the stimulus patch which appeared the lighter of the two, supposing that an appreciable difference existed. The series of observations on April 22nd and 28th with darkness adaptation indicate that green appeared considerably lighter for both birds than without adaptation. Green was chosen more frequently by number 3 than by number 4, apparently because the two stimuli were of more nearly the same value in general illumination for this bird than for the male.

From these few observations, and naturally only a few observations could be made of preference, we may conclude that spectral red and green stimuli of approximately the same energy values did not appear markedly different to the female dove in general illumination, whereas without general illumination the green seemed the more intense. For the male, on the contrary, the red seemed somewhat more intense than the green, and darkness adaptation rendered the two of practically the same intensity.

Hess⁴ has already demonstrated the Purkinje phenomenon in chickens and doves, by a method radically different from that of the writer, while Lashley⁵ has more recently demonstrated it in the game bantam by the method of this investigation. There seems to be no reason for doubting that the observations described above also constitute a satisfactory demonstration of the modification of stimulating value by adaptation.

A series of observations was now instituted, beginning on April 29th, on the development of the ability to distinguish red from black and of the habit of reacting positively to red and negatively to black. Supposing that red appeared light and black dark, it would seem that both doves, merely as the result of their light-dark training with colorless stimuli, should select red uniformly and avoid the black. The results, however, as they appear in table 5, do not wholly justify this expectation.

⁴Hess, C. Untersuchungen über das Sehen und über die Pupillenreaction von Tag- und Nachtvögeln. *Archiv. für Augenheilkunde*, 1908, Bd. 59, S. 143.

⁵Watson, J. B. *Vergleichende Physiologie des Gesichtssinnes*. Jena, 1912, Bd. 4, S. 9.

⁶Watson, J. B. *Behavior*. New York, 1914, p. 350.

Instead, they seem to indicate that for the female dove, the red was so dark that it tended to be confused with the black, or at least was not accepted as the equivalent of the light area which the bird had previously learned to choose.

In this red-black training, it was possible to give each dove twenty trials in succession. As a result of one hundred and forty trials, number 3 was reacting properly ninety per cent of the time. Curiously enough, the male, number 4, chose the red eighteen times out of twenty in his first series, and showed throughout his reactions, in the red-black training, ability to respond to these two stimuli much as he had to the light and dark achromatic stimuli. This is, of course, wholly in agreement with the results of the preference tests, which clearly indicated that the red stimulus for some reason possessed a higher stimulating value for the male than for the female.

It is, of course, impossible to say, on the basis of the red-black results, that either bird responded to the chromatic difference instead of to the intensity difference of the stimuli. It is doubtless safer to assume that the latter alone was the basis of choice.

Beginning on May 9th both doves were presented with the red and green stimuli which on April 21st had been offered as a basis for preference reactions, with the difference that now they were required to choose the red and to avoid the green on penalty of electric stimulation. Again, each daily series consisted of twenty successive trials. The female exhibited, at first, slight ability to distinguish the two stimuli and to respond appropriately, but after three hundred and eighty trials, she was reacting perfectly. The male, on the contrary, reacted perfectly even from the first, his second series of twenty trials including no mistakes. It is thus fairly clear that he responded to the intensity difference of the two chromatic stimuli, and it seems wholly probable, in view of the gradual development of the habit, that she also acquired the ability to respond to the same difference.

From these preliminary observations, it seems safe to conclude that for the ring-dove a red and a green from the spectrum of the carbon arc, of the wave lengths designated above, and of approximately the same energy, as measured by the selenium cell, are sufficiently unlike in stimulating value to be

readily distinguished by certain individuals and with difficulty by others. The particular results in hand suggest that the red has a higher stimulating value for the male than for the female.

The next step in the experiment would naturally enough have been observation of the responses of the subjects to varied energy values (intensities) of the two chromatic stimuli. Unfortunately, the investigation had to terminate at the end of May and the laborious preparation for these final observations was unavoidably wasted. The writer had fully expected and hoped, within the period of six months at his disposal when the investigation was undertaken, to ascertain whether the ring-dove can distinguish a red from a green stimulus throughout a wide range of energy or intensity values. This he did not succeed in doing, and consequently this report must be entitled "Preliminaries to a study of color vision in the ring-dove."

The principal conclusions which may safely be drawn from these observations have been suggested in the course of the presentation, but by way of summary and review, they may be enumerated here.

1. It is fairly obvious that the ring-dove is not sufficiently docile to be an ideal subject for the study of color vision by means of the method which Watson and I have developed.

2. It is indicated that the value of a certain red and a certain green may be very different for two ring-doves, and it is possible that this difference is correlated with sex, the red having a higher stimulating value for the male than for the female.

3. As has already been demonstrated by the writer in the case of a number of animals, the use of the electric stimulus as a means of compelling attention to an experimental situation and of promoting habit formation is desirable in work with the ring-dove.

4. Ring-doves differ markedly in temperament. The pair used by the writer throughout this work presented differences which must be considered if one is to understand the results. To begin with, the male was somewhat wild, but at the same time fairly bold, whereas the female was tamer but more timid. Because of this contrast in timidity, the male almost from the start proved the better subject. He was not so easily disturbed or distracted, reacted therefore more steadily, and chose more certainly. With constant handling he became quite as tame as

the female and lost almost entirely his timidity in the apparatus. She, however, continued to be rather timid throughout the several months of work, although she was perfectly tame. The differences in the nature of the reactions, as recorded in the experimenter's record-book, can be appreciated only in the light of these temperamental facts.

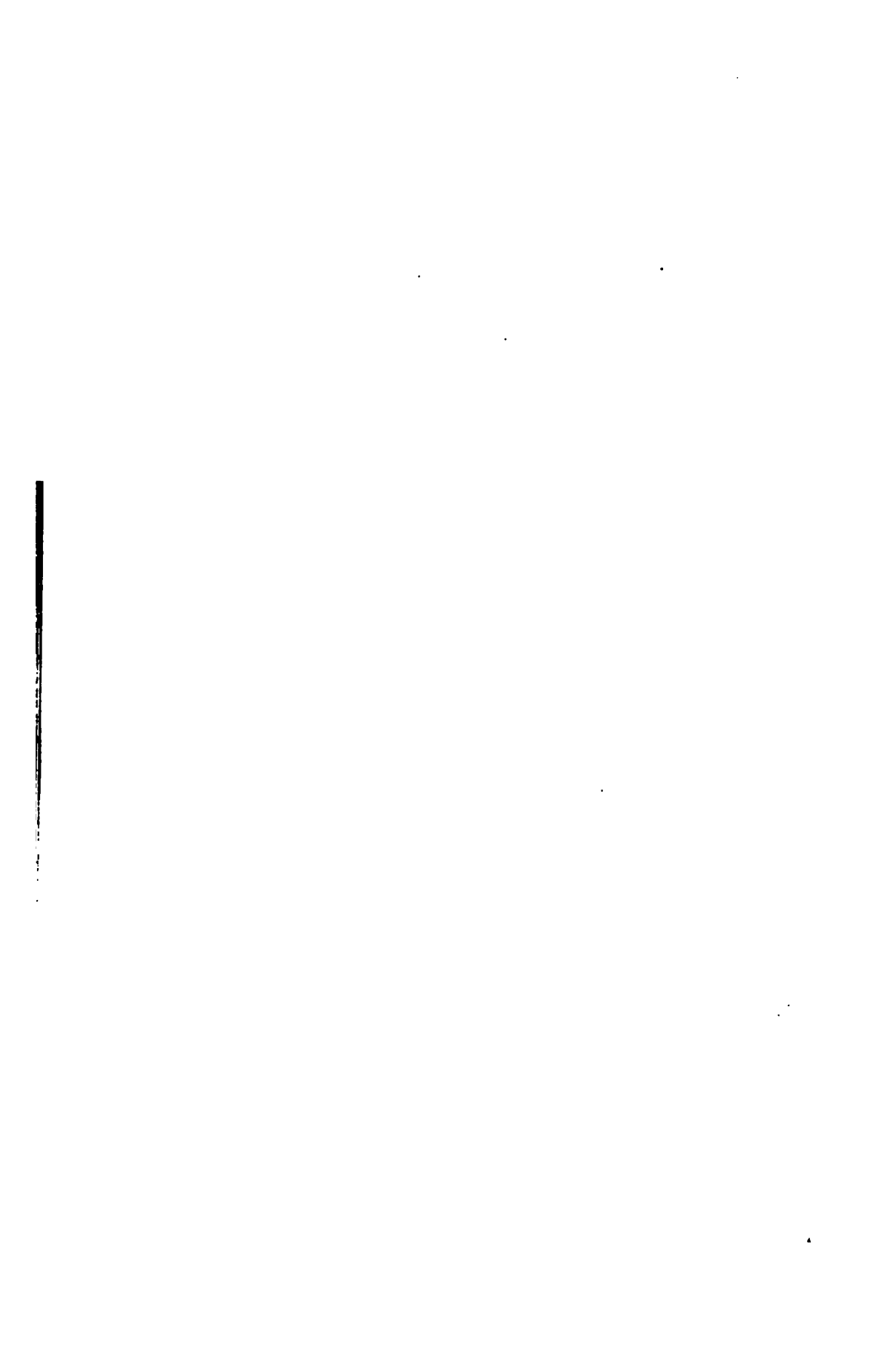
The sex contrasts indicated in the above paragraphs one dare not emphasize very strongly on the basis of observations on two individuals, but they at least suggest the desirability of further study of the sexes. It is the writer's opinion that they agree sufficiently closely with the results obtained in the case of other animals to justify their provisional acceptance.

As has been repeatedly noted with other animals, there are good and bad days in experimental work with ring-doves,—days which are good or bad, not, so far as one may tell, because of variation in the experimenter or his manipulation of the apparatus, but chiefly because of variations in the condition of the subjects. The experiments described in this paper were made at about the same hour each morning, and it was quite impossible for the experimenter to predict the outcome of a series in the light of previous series, for the attention of the doves to the situation seemed to vary independently of any conditions or group of conditions which the experimenter could take into account. There are animals which can be relied upon to work steadily and fairly predictably. The ring-dove is not one of them.

The writer has been led to reflect, because of the outcome of this series of observations, on the possible relation of the simplicity of the experimental situation to the results. He was compelled to devote several weeks to the establishment of a simple habit in two ring-doves, a habit which was next to valueless except as a preparation for further observations. It is natural that during this long period of preparation he should frequently wonder whether the desired end might not be gained more quickly by a different method. It seems probable that a complex situation would have proved more favorable, and that had the two stimuli varied in other respects than in intensity, the animal's attention would more readily have been directed to them and more steadily held upon them. The matter is mentioned here because it is obviously of extreme importance to

students of behavior to discover the most efficient means of developing preparatory habits in animals.

In concluding this paper, the writer can not refrain from calling attention to the waste of time which results from the sacrificing of trained animals at the end of an investigation. It should be possible, through exchange, to make the same subject serve in various experiments. And different experimenters, supposing our methods to be reasonably standardized, might study quite different problems on the basis of similar preparatory habits. Thus, for example, the doves which in this investigation have been trained to certain visual reactions, might perfectly well be employed for other forms of visual response, or even to greater advantage for studies of the relation of the central nervous system to the acquired responses. It is suggested, therefore, that American investigators who are actively engaged in studies in animal behavior keep in close touch and develop a system of reporting their experiments while in progress, which may serve as a basis for the serviceable exchange of trained subjects. The writer happens to have on hand at the moment of writing three tame crows which are highly trained in certain modes of response. The labor of taming and training them would have to be valued at several hundred dollars. It is impossible, under present conditions, to make use of these birds, and unless some other investigator can be found who can take advantage of this preparation, they will have to be either set at liberty or otherwise sacrificed.



A STUDY OF THE BEHAVIOR OF THE CROW
CORVUS AMERICANUS AUD. BY THE
MULTIPLE CHOICE METHOD

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We have previously reported in this Journal² observations on the behavior of crows in certain forms of visual discrimination. The subjects of that investigation were transferred from the Franklin Field-Station in September, 1913, to the Laboratory of Animal Psychology in Cambridge, and were there kept until June, 1914, in a cage approximately six feet in its several dimensions. Despite their close confinement and the lack of an out-of-door fly, the birds continued in excellent health and proved themselves able to withstand wholly satisfactorily the conditions of laboratory life. When returned to the Field-Station, they were considerably less tame than during the previous summer. For this reason they were not used further for experimental purposes, but were kept for general observations. Young crows were captured for the experiments which are reported in this paper.

Instead of following up the study of visual discrimination, we devoted our attention, during the summer of 1914, to an attempt to analyze ideational and allied forms of behavior in the crow by means of the Yerkes multiple choice method, and

¹ The observations reported were made chiefly by Mr. Coburn and the paper was written by Mr. Yerkes.

² Coburn, C. A. The behavior of the crow *Corvus Americanus*, Aud. *Journal of Animal Behavior*, 1914, 4, 185-201.

to the accumulation of additional facts concerning the natural history, instincts, and general habits of the birds.

On June 7th, 1914, three young crows were captured near the Station. These birds were about ready to leave the nest. One, indeed, was taken from a limb beside the nest. This individual from the first exhibited fear and was so troublesome that after two days it was discarded and the remaining two birds were kept for observation. They were placed in a box which was frequently passed by human beings, and were several times a day fed by hand, being allowed to come out of the box at will and become thoroughly accustomed to the experimenters. From the time of capture they were perfectly tame, ate readily, and the characteristic fear reactions never appeared. When taken from the nest, they were probably at least six weeks old.

Throughout this report, these birds will be referred to as number 3 and number 4. Number 3 was from the first the larger of the two and the less timid. It, during the several months of observation, always came to us, perching on arms, shoulder, or head, as it had opportunity, and showing a friendly interest which was apparently somewhat independent of its desire for food. It evidently liked to be petted. Our assumption is that this bird is a male.³ Number 4, by contrast, was smaller, shyer, more wary, and after a few weeks ceased to come to either of us, except as drawn by hunger, and even then it often hesitated to perch upon the hand or arm. In all probability, it is a female. It has eaten less than number 3, and has been considerably more difficult to experiment with. Usually, in the course of an experiment, if the birds were in competition, number 4 would stand aside for number 3.

Our additional experience with crows during the present season but emphasizes our conviction that they are among the most interesting of birds, and that their behavior is in every respect worthy of careful analytic study. With respect to what we shall term "ideational behavior," they have fallen short of our expectations, for in the light of their varied interests, ingenuity, curiosity, ceaseless activity, and apparent insight into simple situations, we had assumed that they possess an intelligence equal to that of many of the more intelligent mammals. The

³ Since this was written, dissection has definitely established our surmise in the case of both birds.

experiments now to be reported were conducted for the special purpose of obtaining definite and reliable information concerning the nature and limitations of their ability to adjust themselves to certain fairly simple, although novel, situations.

We sought to make our measurements of intelligence by a method recently devised at the Psychopathic Hospital, Boston, by R. M. Yerkes, for the comparative study of ideational and allied forms of behavior in man and other animals. This method has been named the soluble-problem multiple-choice method. It was devised primarily for the purpose of enabling the comparative psychologist to present to any human or infra-human subject, no matter what the age, degree of intelligence, or condition of normality or abnormality, a series of situations increasing in complexity from an extremely simple one to one so intricate that even the most intelligent human subject might spend hours or days in adjusting himself to it. By means of this multiple choice method, it is hoped and confidently expected that the materials of comparative psychology may be rapidly increased and the analyses of animal behavior be made invaluable to the psychopathologist.

A general description of the method should preface this account of the special form in which it was applied to the crow, inasmuch as only a very brief account of it has been published.⁴

In brief, the essentials of the method are these. A series of reaction mechanisms, appropriate to the subject, are presented. From this series one mechanism must be selected which, when properly approached, will yield the subject the satisfaction of success and, possibly, the reward of food. With each presentation of the reaction mechanisms, they are varied in number and in position. The subject is therefore forced to select the proper mechanism on the basis of some particular relationship of that mechanism to its fellows, this relationship having been determined upon in advance by the experimenter. It may be, for example, such a simple relation as first at the left of the series as the subject approaches, or first at the right of the series, or second at the left, or alternately the first at the left and the first at the right, or the middle of the series. Imagine, then a series of piano keys which may be presented to a human subject. They

⁴ Yerkes, Robert M. The study of human behavior. *Science*, 1914, 39, 625-633. In this paper the writer describes his method in contrast with the Hamilton quadruple choice method.

may vary in number from two to twelve (this was the original form of apparatus). Some one key, in any group of keys presented, when pressed will cause a bell to ring, thus indicating success. Without other aid than his own observation, the subject is expected, from repeated presentations of the keys, to discover the essential relation and to acquire the ability to select the right key with certainty.

This method has the advantage of enabling the experimenter to present increasingly difficult problems to his subjects. It has further the advantage of enabling him conveniently to record the essential features of reaction, and later to analyze the reactions at his leisure. But most important of all, it yields strictly comparable results when applied to widely differing organisms. Naturally, although the same problems may be presented to diverse types of organism, the reaction mechanisms must be suited to the subject in question.

Without further general comment or discussion of the multiple choice method, we shall describe the form of apparatus and procedure employed with the crow.

APPARATUS AND METHOD

In the accompanying plate, designated as figure 1, and in the ground plan of the observation-room and apparatus, shown in figure 2, the general experimental situation is represented. Figure 1 shows in the background the building which was used both as a shelter for the crows and as an observation place for the experimenter. To this building is attached a fly which appears in C, D, and E of Figure 1. In figure 2, the ground plan of the building, are seen the experimenter's room, A, and the crow room, B, the latter containing a perch, P. All coarsely dotted lines in this figure indicate walls or partitions made of poultry wire. The large fly was, for the purposes of our experiment, divided into two parts by a wire partition. In the smaller of these portions, shown at the right of figure 2, the multiple choice apparatus was located. The crows could enter this portion of the fly only at the will of the experimenter, whereas they were allowed the freedom of the larger portion, which we have labelled C. As figure 2 is drawn to scale (one inch to forty-eight) it is unnecessary to give the measurements of the

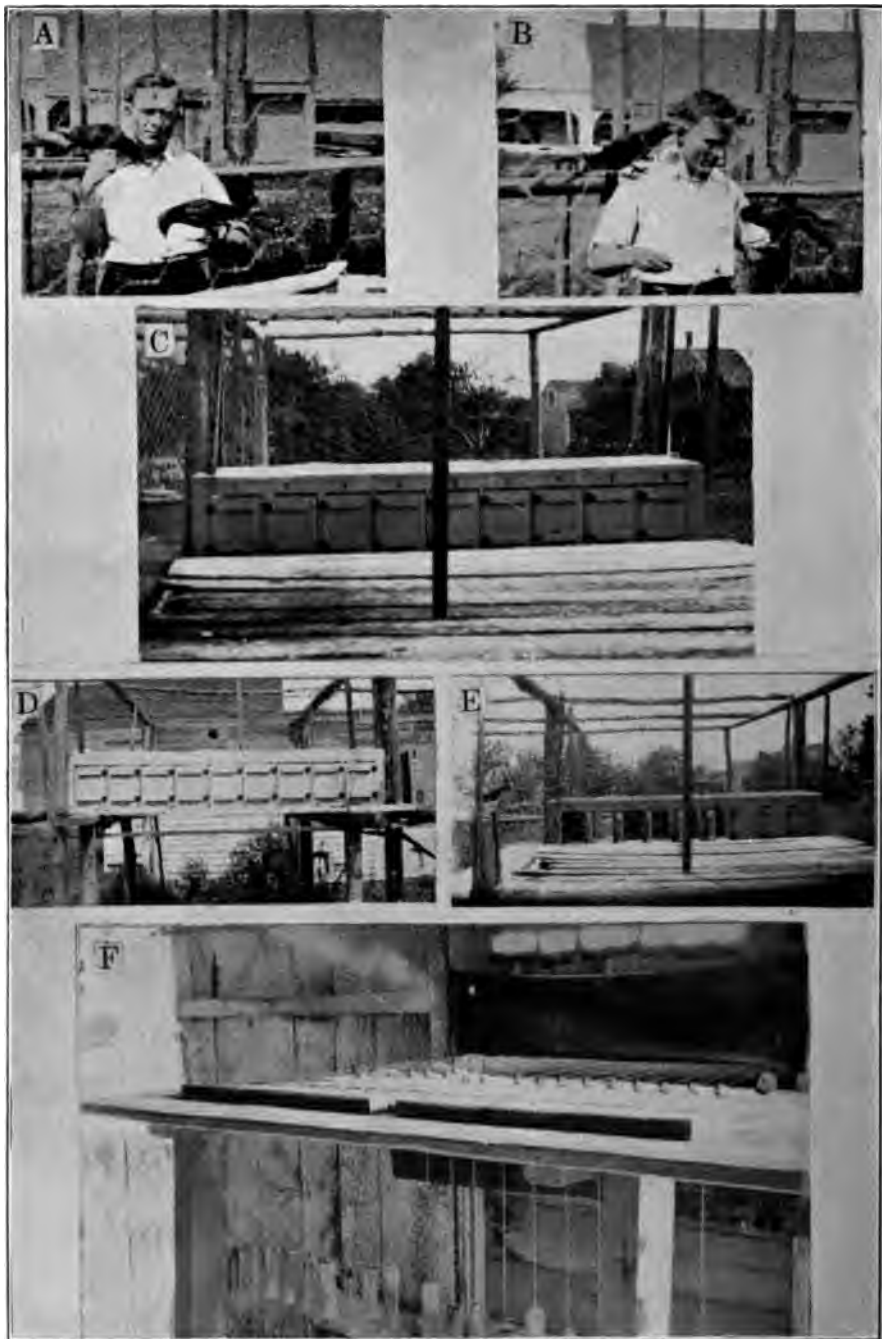


FIGURE 1. Views of crows and apparatus for multiple choice experiments. **A** and **B**, crows, number 3, ♂, on shoulder of experimenter, and number 4, ♀, on arm; **C**, the multiple choice box seen from the observer's room and from the direction of approach by the crow, the compartments are numbered 1 to 9 and below each number is an entrance door. **D**, the same seen from the opposite side or rear, with the nine exit doors closed. **E**, the box seen from the observer's room, with entrance doors 1 to 6 and exit door 2 open. At the extreme left, above the entrance door to the experiment compartment of the fly, one of the crows is visible. **F**, the observer's table, showing curtain before window (partially drawn aside to admit light for the camera) and the weighted cords with pull buttons for opening and closing doors.

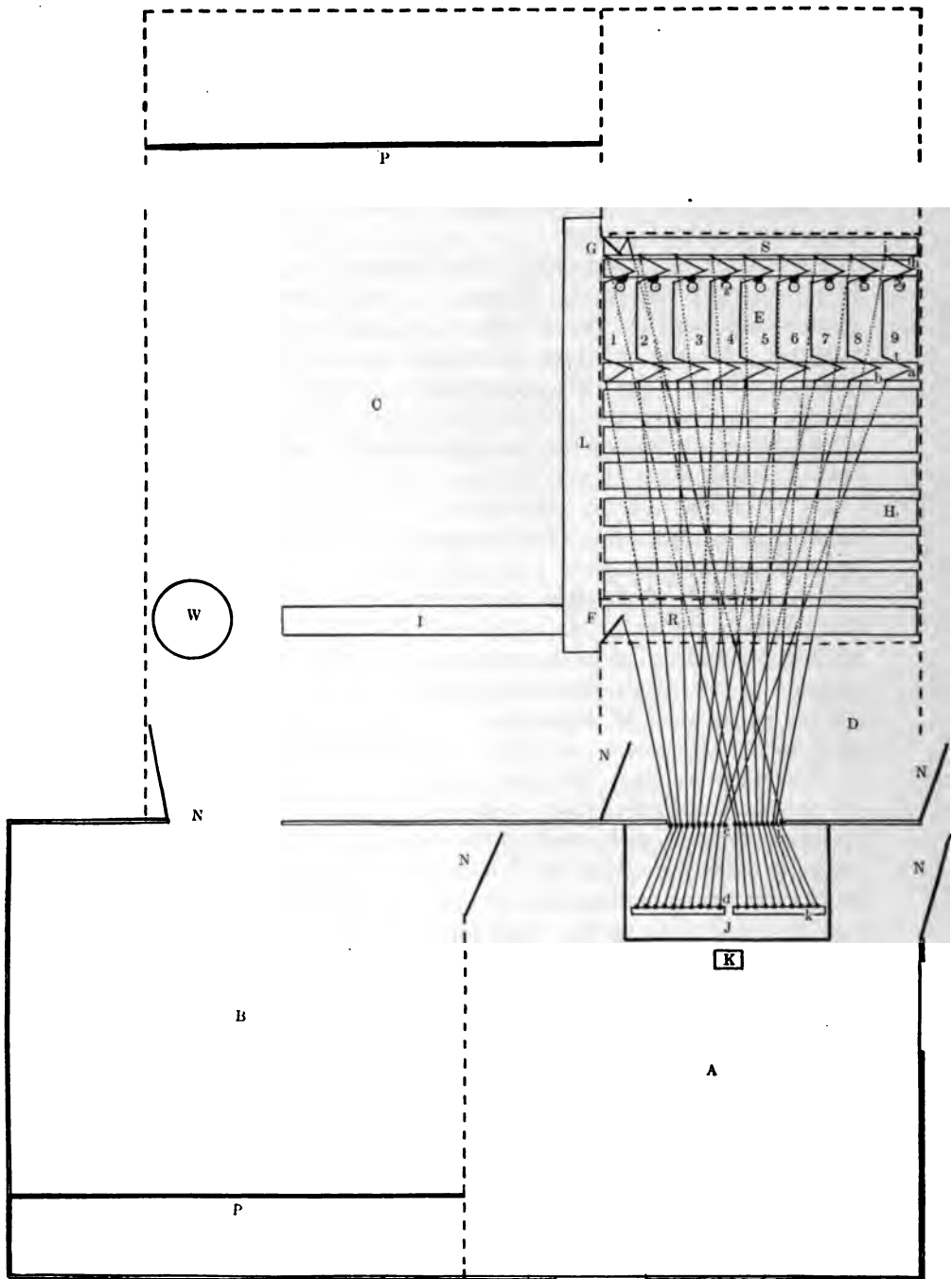


FIGURE 2. Ground plan of crow house, fly, and apparatus. Scale, $\frac{1}{4}$ ". A, observation room; B, bird room; C, main portion of fly; D, passageway for experimenter; E, multiple choice box; F, entrance door between main fly, C, and alley to reaction chamber, H; G, exit door between alley S and main fly; H, reaction chamber, the floor boards of which are separated somewhat; I, L, approaches to the doors F and G; J, observer's table and key-board; K, observer's stool; N, doors for experimenter's use; P, perches; R, alley leading to middle of reaction chamber H; S, alley leading from exits to main fly; W, water tub for crows.

Numerals 1 to 9, compartments of multiple choice box; a, attachment of cord to entrance door, t, of compartment 9; b, screw eye for cord; c, screw eye at entrance to observer's room; d, wooden button on cord; under d is a small brass pulley for cord; h, i, j, k, indicate course of cord from exit door of compartment 9 to key-board; x, metal cover for food receptacle of compartment 9; z, food receptacle of compartment 4.

building and fly. We shall give a more detailed description of the experimental device.

The latter is shown fairly well from different points of view in the parts of figure 1. Figure 1 C is a view of the multiple choice box from the front, that is, the side of approach by the subject. All the entrance doors are closed. Figure 1 E shows the apparatus from the same point of view, with the entrance doors 1 to 6 and the exit door 2 open. Figure 1 D, instead, shows the apparatus from the opposite side, with the several exit doors closed.

By referring now to both figures 1 and 2, we should be able to obtain a clear idea of the construction of the experimental mechanism and its use.

The multiple choice box, as we shall call it, appears in ground plan as E of figure 2. It is divided into nine like compartments, each with a door at both ends, opening outward. The outside measurements of the multiple choice box are 81 inches long by 20 inches wide by 15 inches high. The frame of the box is made of 2 by 2 inch stock, and the floor, ends, partitions, and doors, of half-inch stock. The top, which is hinged for convenience of access, consists of wire netting, $1\frac{1}{4}$ inch mesh, on a wooden frame. On the inside, each of the nine compartments is 19 inches long by 8 inches wide by 13 inches high. The entrance and exit doors are $9\frac{1}{2}$ inches high by $7\frac{3}{8}$ inches wide. All of the doors are mounted with spring hinges which hold them shut. On the lower inner edge of each exit door is a piece of tin (x) which, when the door is closed, projects 2 inches into the compartment and covers a hole (z) in the floor of the compartment $1\frac{1}{2}$ inches in diameter by $\frac{7}{8}$ inches deep. These metal covers, as well as the holes, are represented in the ground plan of the apparatus, figure 1, x and z. The use of these holes is to contain food which serves as a reward for the bird when the exit doors are opened.

The system of entrance and exit doors, nine of each, and also the main entrance door, labelled F in figure 2, and shown in the extreme lower left corner of figure 1 E, and the main exit door, labelled G in figure 2, and shown at the right end of the multiple choice box in figure 1 D, are controlled from the experiment room A by a system of cords passing through screw eyes and pulleys.

These cords are indicated by dotted lines where they pass under the floor of the multiple choice box or under the boards which serve as an approach to the box: Elsewhere they appear as solid lines. The arrangement of the cord-system within the experiment room is rather unsatisfactorily shown in figure 1 F.

On a table, J, before which the observer sits on the stool, K, are two groups of cords, each with a wooden button attached in a convenient position. The group at the experimenter's left consists of the cords connected with the ten entrance doors, and the group at the right, similarly, of those connected with the ten exit doors.

We may now trace the course of the cords from the doors of compartment 9. A cord is fastened at a to the lower outer corner of the entrance door t. It thence passes through the screw eye b in the edge of the approach board. From this point it extends, under the interrupted floor of the reaction chamber H, to a screw eye, c, in a block across the aperture leading to the experiment room. Thence the cord passes over a small brass pulley at d and through a hole in the table J. (In figure 2 the pulley is hidden by the wooden button on cord.) It is kept taut by a lead weight under J. Similarly, the cord for the exit door of compartment 9 is attached to the lower outer corner of the door at h, passes through the screw eyes, i and j, to the pulley k, and is kept taut by a leaden weight. The cords for the main entrance and exit doors, F and G, run to the extreme left and right respectively of the experimenter's table.

The experimenter operates a door by grasping the wooden button shown on each cord in figure 1 F and pulling it toward him. When he has pulled as far as the button will come, the door to which the cord is attached stands wide open, and the leaden weight under the table serves to hold it in this position as long as the experimenter desires. When he wishes it closed, he simply pushes the button back to its former position, and the strength of the spring hinges suffices to overcome the pull of the weight.

In order that the bird should not see and be influenced by the movements of the experimenter, a black curtain was hung before the opening into the experiment room, and through small holes cut in it, the experimenter was able to observe the movements

of his subject. At no time during the investigation did the crows give evidence of noticing the experimenter when they were reacting.

The remaining features of the apparatus will be mentioned in connection with the following brief description of the experimental procedure. In preparation for a series of trials, the experimenter opens each of the exit doors and places in each food container a small bit of milk-soaked bread. He then closes the exit doors, thus covering the food, and takes his place at K. He next opens a group of entrance doors. Let us suppose, as is shown in figure 1 E, that the doors numbered 1 to 6 are opened, and, further, that the compartment which may be designated as the correct one is the first at the subject's left, that is number 1. Having made these preparations, the experimenter, by means of the proper cord, opens the main entrance door F, and the bird, either by walking up the approach board I or by alighting on the approach board L, on a level with the entrance door, is immediately able to enter the reaction chamber H, by way of the alley R. By two wire partitions which appear as dotted lines in figure 2, it is forced to walk straight ahead until it reaches the center of compartment H. It may then face and, if it so chooses, directly approach the central compartment of the multiple choice box. But under the circumstances, with entrance doors 1 to 6 open, it would naturally swerve toward the left.

In case it enters compartment 1, the experimenter quickly and noiselessly closes the entrance door after it, by releasing the appropriate cord, and immediately thereafter, opens the exit door of the compartment by pulling on the appropriate cord. He, thus, with one hand prevents the retreat of the bird from the compartment and with the other uncovers the food, so that the bird may obtain the reward for a correct reaction. As soon as the food has been swallowed, the crow steps out of the compartment, the exit door is closed by the experimenter, and the bird either immediately, or at the experimenter's pleasure, is allowed to return to the fly C by way of the main exit door G.

If, instead of choosing the right compartment, the crow enters some other one, the procedure is different. Immediately upon its entrance, the experimenter closes the entrance door. He then, with a stop-watch, measures a definite period during which the bird is confined in the compartment. This period was varied

during our experiments from 15 to 60 seconds, in an attempt to discover the most satisfactory length of confinement. At the proper moment, the experimenter opens the entrance door and the crow is allowed to retrace its steps. It may then immediately make another choice. But not until it enters the right compartment, is it awarded with food and allowed to return to the fly. Thus punishment for incorrect choices is combined with reward for correct choices.

In further description of the apparatus, it should be said that wire partitions at each side of the multiple choice box, and extending from the lid of the same to the roof of the fly, prevented the crow from walking or flying over the box, while boards both in front of the box and behind it and on a level with its floor form a floor which prevented the bird from getting under the box. The only possible course for the subject from main entrance to main exit door is by way of one of the compartments.

Experience shortly indicated that the crows could be used most satisfactorily if given their trials alternately, and the method finally settled upon was that of admitting one crow to the apparatus, allowing it to make its choice, and then holding it in the passageway beyond the exit doors until the other crow had passed through the main entrance door into the reaction chamber. Thus, as one subject emerged from a compartment of the box E, the other bird entered the reaction chamber. When, as sometimes happened, the one or the other bird failed to respond immediately and appropriately and both were in the fly, it was fairly easy for the experimenter to admit the proper bird by carefully manipulating the entrance door.

PRELIMINARY TRAINING

The crows obtained almost all of their food in the multiple choice box. In order that they should work steadily and industriously, it was necessary to have the pieces of bread or mouse meat, which was sometimes used instead of bread, very small. It proved possible to obtain as many as twenty reactions per day from each bird, in series usually of five each.

We shall now consider the course of experimentation and its results. One June 21st, the crows having attained ability to feed themselves preliminary training was undertaken, and from that time they were fed in the multiple choice box. They

exhibited no fear, rapidly became familiar with the apparatus, and acquired skill in making the trip from the main fly, through the experiment compartment, back to the fly. For several days, both the entrance and the exit doors of the compartments were kept open. Then the situation was changed by the closing of the exit doors, and the crows were trained to enter a compartment and wait for their food.

On June 27th, the first series of trials worthy of special mention was given. The apparatus was in perfect working condition. Food was placed *on the floors* of the several compartments; the exit doors were closed and the entrance doors were open. The main entrance door was opened, and both birds were allowed to enter the reaction chamber and go to the compartments for food. As they entered the compartments, the exit doors were opened and the entrance doors closed. Thus, by a series of trials they were habituated to the opening and closing of the doors and were taught to make the circuit promptly from the main fly back to the same by way of the multiple choice box.

On the following day, June 28th, the food was placed in the food containers and the exit doors were closed. Number 3 entered the compartments rapidly and made the circuit usually without delay, but number 4 at first refused to enter the compartments. Within two days, it, however, was readily entering, in its search for food.

On June 28th, only three or four of the entrance doors to the compartments were opened at any one time. In the previous preliminary training all of the doors had been opened. Neither bird showed any marked preference for a particular compartment in the multiple choice box.

On June 30th, the method was tried of confining one of the crows in the crow room B, of figure 2, while the other was given its trials. Later in the day, the birds were given another series of trials alternately, the one being kept in the exit alley as described on page 83, until the other had entered the reaction chamber. This method proved satisfactory and was later employed to the exclusion of the former.

Up to this point, the two subjects adapted themselves to the different situations with almost equal rapidity. Number 4 was somewhat less willing to try new things than number 3, and seemed to be hampered by its shyness.

A significant incident is the following. In one of the trials, number 4 accidentally fell through a five inch space which had been left before the entrance doors in order that the crow should not too closely approach a compartment unless it intended to enter it. The bird fell to the ground beneath the apparatus, finding there some pieces of bread which had been dropped earlier in the day. Naturally enough, it ate them before it could be induced to return to the fly. Ever thereafter, until this crack had been closed, this bird, as it approached the compartments, would look through the crack to the ground. Several times it flew down in search of food.

RESULTS, PROBLEM 1

With the final series of trials given on June 30th, regular experiments were initiated. *The problem which the birds were required to solve was that of learning to select the first open door at the right.*

Ten settings as we shall call them, were chosen by the experimenters. These are given below, numbered 1 to 10. After each number appears the series of open doors; in the next column, the total number of doors open; and finally in the last column, the number of the right compartment in which the reward of food might be obtained.

PROBLEM 1. First door at the subject's right to be chosen			
Settings	Doors open	No. of doors open	No. of right door
1.....	7.8.9.....	3.....	9
2.....	2.3.4.....	3.....	4
3.....	3.4.5.6.7.....	5.....	7
4.....	1.2.....	2.....	2
5.....	2.3.4.5.6.....	5.....	6
6.....	6.7.8.....	3.....	8
7.....	3.4.5.....	3.....	5
8.....	2.3.4.5.6.....	5.....	6
9.....	1.2.3.....	3.....	3
10.....	7.8.9.....	3.....	9

In this series of ten settings, a total of thirty-five doors were open, of which number, ten were of course "right doors." Consequently, the chance of a selection of the right door, without previous experience or trial, is one to two and one-half.

In general, it was the purpose of the experimenter, as far as possible, to follow through this series of settings from 1 to 10, and then to return to the beginning and repeat the series. No matter how many trials in succession could be given, the exper-

iments were resumed at the point of interruption of the regular series of settings. Thus, if five trials were given, beginning with setting 1 and extending through setting 5, the next series would begin with setting 6 and continue through setting 10.

As a matter of convenience, it was also decided to have the two crows work on different settings. For example, while crow 3 was presented with the settings 1 to 5, crow 4 would be presented with settings 6 to 10. This enabled the experimenter to avoid the necessity of refilling the food containers after each trial, and it also prevented the crows from developing the tendency to follow one another by sensory cues.

After a very few days of experimentation, both birds reacted with remarkable alacrity and facility. They were, as a rule, prompt to enter the reaction area and almost as prompt to leave the exit area.

In the initial regular experiments, thirty seconds confinement in the wrong compartment was used as punishment for mistakes. But it shortly appeared that this was too long an interval, for the birds hesitated to enter any of the compartments after a half minute confinement in one of them. It was therefore decided to use the period of fifteen seconds as punishment for incorrect choices. Especially during the early experiments, the crows often exhibited considerable fear and excitement when shut in the small compartments. This diminished toward the end of our work, and it was then possible to confine them for a half minute or even a minute without causing disturbing excitement.

The experimenter kept, as a matter of routine, a record of the time from admission to the reaction chamber to entrance into the right compartment. There is no special reason to consider these records significant, and we shall omit them from this report.

Careful record was also kept of the chief features of the behavior of the bird during this interval. The simple system of symbols, which appears below, was adopted for this purpose.

- O, to center of the reaction area
- ⌞, to left hand far corner of the area
- ⌟, to right hand far corner of the area
- ⌠, to left hand near corner of the area
- ⌡, to right hand near corner of the area
- ⌢, to center of the near side of the area
- ⌣, to center of the left side of the area
- ⌤, to center of the right side of the area

If the crow merely looked into one of the compartments without entering, the number of the compartment was recorded.

If it, instead, entered a compartment, the number was underscored. In case the compartment entered happened to be a "wrong one", the time of entrance was placed in parenthesis immediately after the number of the compartment. When the time exceeded a minute, the number indicating the minutes was placed in a circle. For less frequent forms of behavior, other provisions were found convenient, and by the use of symbols and other abbreviations it was found easy to obtain a fairly complete description of the subject's behavior.

To illustrate the use of the above symbols, the following record of a trial (trial 32 of number 3 on July 23), setting 1.2, is presented.

4, 3, L, J, L, ①, L, O, L, 1, L, ②, (trying to get out of area),
L, J, ③, J, L, J, (pkd. at hole in floor), ④, 1, O, L, 9, ⑥, ⑥,
⑦, O, r, L, 2, ⑧, 1, 2, 8' 13".

In this trial, crow number 3 did not enter the wrong compartment at all. The time between the fifth and the seventh minutes was spent before compartment 9.

It was decided by the experimenters that when a crow had made ten correct choices in succession, its training should be considered complete, or in other words, it should be said to have solved the problem.

In the case of the problem in question, crow number 3 at the end of thirty-two trials had entered the right compartment twelve times in succession, but in several of these trials it had been aided by the experimenter, who moved the exit door slightly in order to attract the attention of the bird after it had for several minutes refused to enter any compartment.

In the accompanying table 1, a summary of the trials for each of the birds in problem 1 appears. At the head of the several columns are the settings numbered 1 to 10, with the right number in bold faced type. In the first column at the left, under each of the several settings, appears the number of the trial and the series of compartments entered. Thus, for example, referring to the results for crow number 3, in trial number 5, which was the first trial in the regular series, the bird entered compartment 8 and then compartment 9. In trial 6, immediately after compartment 4 the right one. The

letter a, following a number, indicates that the bird was aided in its choice by the experimenter.

It is possible by careful study of this and succeeding tables to discover the reactive tendencies of the organism, and to note both the appearance and the disappearance of the same.

Problem 1, the first door at the right, proved a very easy one for both crows. It was mastered by number 3 after fifty-five trials, and by number 4 after fifty-one trials.

Table 2 presents the results of the various series of trials, ranging in number from three to five for each subject. The number of successes and failures in each series and the ratio of successes to failures for each day appear. The letter R in this table indicates correct first choices, the letter W, incorrect first choices. The table has to do only with first choices.

In contrast with the above results in problem 1, the first door at the right, we present in table 3 a summary of the results for problem 1a, the first door at the left, the trials for which were given not immediately after those just described but at the end of the season, and after the crows had for several weeks worked on problem 2, the second door at the left. Naturally the influence of their training to go to the second door retarded the formation of the habit of choosing the first door at the left. For the satisfactory solution of the problem, one hundred trials were required by each bird. Doubtless a change of experimenters after trial 75 somewhat delayed progress. The results which appear in tables 3 and 4 demand no further comment.

Recurring now to problem 1, it is obvious that from the human point of view this is a very simple problem. The crows solved it readily, but in the course of their work they frequently experienced discouragement and were aided in a considerable number of their early trials by the experimenter. Doubtless our results would be more significant had this aid been withheld, but at the outset of our work we hesitated to run the risk of spoiling our subjects by over-discouraging them. In problem 1a, no aid was needed.

Varied reactive tendencies do not appear in connection with this problem. Very few wrong choices were made. Consequently, all that can be gleaned from the results is a general knowledge of the behavior of the crow in the face of a certain fairly simple experimental situation.

TABLE 1
RESULTS FOR CROW NUMBER 3 IN PROBLEM 1

	S. 1 7.8.9	T. 1.2.3	S. 2 2.3.4	T. 2.3.4	S. 3 3.4.5.6.7	T. 3.4.5.6.7	S. 4 1.2	T. 1.2	S. 5 2.3.4.5.6	T. 2.3.4.5.6	S. 6 6.7.8	T. 6.7.8	S. 7 3.4.5	T. 3.4.5	S. 8 2.3.4.5.6	T. 2.3.4.5.6	S. 9 1.2.3	T. 1.2.3	S. 10 7.8.9
5*	8.9	6	4		7	7a	8	2											
							9	2		4.6	8	11	5	12	6	13	3	15	8.9a
16	9a	17	4a		18	7a	19	2a	**		8a	21	5a	22	6	23			
															6	24	3	26	9
27	9	28	4																
29	9	30	4a		31	7	32	2		5.6	8	34	4.5	35	6	36	3	38	9
39	8.9	40	4		41	7													
42	8.9	43	4		44	6.7	45	1.2	6		8	47	5	48	6	49	3	51	9
52	9	53	4		54	7	55	2	6										

* Records for first four trials:

(1) 1.2.3 (2) 8.9 (3) 6.7.8 (4) 3.4.5.6.7
3 9 6.8a 3.7a

** Blanks indicate failures to choose.

RESULTS FOR CROW NUMBER 4 IN PROBLEM 1

T.	S. 1 7.8.9	T.	S. 2 2.3.4	T.	S. 3 3.4.5.6.7	T.	S. 4 1.2	T.	S. 5 2.3.4.5.6	T.	S. 6 6.7.8	T.	S. 7 3.4.5	T.	S. 8 2.3.4.5.6	T.	S. 9 1.2.3	T.	S. 10 7.8.9
*										5	6.7.8a	6	5	7	2.6a	8	—		
																9	3	10	9
11	8.9a	12	4a	13	7	14	2												
						15	2	16	6	17	8	18	5	19	4.2.6	20	3	21	9
22	8.9	23	4	24	4.7	25	2	26	6	27	7.8	28	5						
										29	6.8	30	5	31	6	32	3	33	9
34	9	35	3.4	36	7			37	6										
38	8.8.7.9											39	3.5	40	6	41	3		
										42	8	43	5	44	6	45	3	46	9
47	9	48	4	49	7	50	2	51	6										

* Records for first four trials:
(1) 1.2.3 3 (2) 8.9 9 (3) 3.4.5.6.7 (4) 2.3.4.5.6 6
5.5.7a

TABLE 2
DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

Crow Number 3							Crow Number 4						
PROBLEM 1													
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 30	4	2	2	2	2	1:1	June 30	4	3	1	3	1	1:33
July 1	3	2	1				July 1	3	1	2			
" "	1	1	0				" "	1	0	1			
" "	3	2	1				" "	3	2	1			
" "	3	3	0	8	2	1:25	" "	3	2	1	5	5	1:1
" 2	2	0	2				" 2	2	2	0			
" "	4	0	4	0	6	0:6	" "	4	3	1	5	1	1:20
" 3	3	1	2				" 3	3	2	1			
" "	5	5	0				" "	5	3	2			
" "	4	3	1				" "	4	3	1			
" "	4	2	2	11	5	1:45	" "	4	2	2	10	6	1:60
" 4	5	4	1				" 4	4	4	1			
" "	5	3	2				" "	5	5	0			
" "	5	5	0	12	3	1:25	" "	5	5	0	14	1	1:07
" 5	5	5	0	5	0	1:0							

TABLE 3
RESULTS FOR CROW NUMBER 3 IN PROBLEM 1A

T.	S. 1 1.2.3	T.	S. 2 7.8.9	T.	S. 3 3.4.5.6.7	T.	S. 4 8.9	T.	S. 5 2.3.4.5.6	T.	S. 6 6.7.8	T.	S. 7 5.6.7	T.	S. 8 4.5.6.7.8	T.	S. 9 7.8.9	T.	S. 10 1.2.3
1	3.2.3.1	2	8.9.8.7	3	3	4	9.8	5	3.2	6	6	7	5	8	5.4	9	8.8.8.8.7	10	{3.2.3.2 2.2.1 3.2.2.2.1 2.1 2.3.3.1
11	3.2.1	12	8.7	13	3	14	9.8	15	2	16	6	17	5	18	4	19	8.8.7	20	3.2.2.2.1
21	3.2.2.1	22	8.7	23	3	24	9.9.8	25	2	26	6	27	5	28	4	29	8.8.8.7	30	2.1
31	3.2.3.1	32	8.7	33	3	34	8	35	2	36	6	37	5	38	4	39	{8.8.8.8 8.8.7	40	2.3.3.1
41	2.2.3.1	42	7	43	3	44	8	45	2	46	6	47	5	48	4	49	8.8.7	50	2.3.1
51	2.1	52	8.8.7	53	3	54	8	55	2	56	6	57	5	58	4	59	8.7	60	2.1
61	2.3.2.2	62	7	63	3	64	8	65	2	66	6	67	5	68	4	69	7	70	2.1
71	2.1	72	7	73	3	74	8	75	2*										
	7.8.9		6.7.8.9		(2.3.4.5 16.7		4.5.6.7		{1.2.3.4 5.6		{5.6.7.8 9		{1.2.3.4 5.6.7.8.9		{3.4.5.6 7.8		7.8.9		2.3.4.5
76	8.7	77	8.6	78	2	79	7.4	80	1	81	5	82	2.1	83	3	84	8.7	85	3.2
86	7	89	6									90	1	87	3			88	2
91	1	92	7	93	3	94	8	95	2	96	6	97	5	98	4	99	7	100	1

RESULTS FOR CROW NUMBER 4 IN PROBLEM 1A

S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	T.	S. 10
1.2.3	7.8.9	3.4.5.6.7	8.9	2.3.4.5.6	6.7.8	5.6.7	4.5.6.7.8	7.8.9	T.	1.2.3
3.2.1	7	3	8	2	1	5	{8.7.6.6 7.5.6.7.5 7.6.8.4}	7	5	3.2.3.1
2.3.1	7	5.3	8	10	11	5	4	7	14	3.2.1
3.2.3.1	17	18	8	20	21	5	4	7	24	3.2.3.1
3.1	27	28	8	30	31	5	4	7	34	3.2.2.1
3.1	37	38	8	40	41	5	4	7	44	2.1
2.3.1	47	48	8	50	51	5	5.6.7.4	7	54	1
3.1	57	58	9.8	60	61	5	4	7	64	2.1
2.1	67	68	8	70	71	5	5.6.4	8.7	74	1.*
7.8.9	6.7.8.9	{2.3.4.5 6.7}	4.5.6.7	{1.2.3.4 5.6}	{5.6.7.8 9}	{1.2.3.4 5.6.7.8.9}	{3.4.5.6 7.8}	7.8.9		2.3.4.5
8.7	77	78	4	80	81	1	6.3	7	85	2
	90		4	87	88				89	2
1	92	93	8	95	96	5	4	7	100	1

* At this point, danger in experimenters occurred. The settings were also changed as is indicated. Beginning with trial 91 the original series of settings was used.

TABLE 4

DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

PROBLEM 1A											
Crow Number 3						Crow Number 4					
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	Ratio of R to W
Aug. 8	5	1	4				Aug. 8	5	2	3	
" "	5	2	3				" "	5	4	1	
" "	5	2	3	5	10	1:2	" "	5	4	1	10 5
" 9	5	3	2				" 9	5	1	4	
" "	5	2	3				" "	5	3	2	
" "	5	3	2				" "	5	3	2	
" "	5	3	2	11	9	1:81	" "	5	4	1	11 9
" 10	5	3	2				" 10	5	2	3	
" "	5	4	1				" "	5	4	1	
" "	5	3	2				" "	5	4	1	
" "	5	3	2	13	7	1:53	" "	5	4	1	14 6
" 11	5	4	1				" 11	5	1	4	
" "	5	4	1				" "	5	4	1	
" "	5	4	1				" "	5	4	1	
" "	5	4	1	16	4	1:25	" "	5	3	2	12 8
" 13	5	2	3				" 13	5	4	1	
" "	5	2	3				" "	5	3	2	
" "	5	5	0	9	6	1:67	" "	5	5	0	12 3
" 14	5	5	0				" 14	5	5	0	
" "	5	5	0	10	0	1:0	" "	5	5	0	10 0

RESULTS, PROBLEM 2

Our second problem, which we arranged as a more difficult one than the first, proved for the crows much more difficult than we had expected. It may be described as *the problem of the second door at the left*. The series of ten settings for this problem is as follows:

PROBLEM 2. Second door at the subject's left to be chosen			
Settings	Doors open	No. of doors open	No. of right door
1.....	7 8 9.....	3.....	8
2.....	6 7 8 9.....	4.....	7
3.....	2 3 4 5 6 7.....	6.....	3
4.....	4 5 6 7.....	4.....	5
5.....	1 2 3 4 5 6.....	6.....	2
6.....	5 6 7 8 9.....	5.....	6
7.....	1 2 3 4 5 6 7 8 9.....	9.....	2
8.....	3 4 5 6 7 8.....	6.....	4
9.....	7 8 9.....	3.....	8
10.....	2 3 4 5.....	4.....	3

For these ten settings the total number of doors open is fifty, of which ten are "right doors." The chance of a correct first choice, without previous experience or the prejudicial influence of training in problem 1, is one to four.

In this problem, the first fifty trials were given to the crows in groups of two and three each. The birds made so many mistakes at the beginning that they became discouraged, and after two or three trials, would refuse to work. Later, as they became accustomed to the situation and the experimenter increased the degree of hunger, they could be induced to react five times in succession. Consequently the number of trials per series was increased to five.

Now, as in the case of problem 1, the experimenter was forced, in order to avoid the possibility of utterly discouraging his subjects, to aid them after they had worked for several minutes without success in locating the right door. This was always done by slightly moving the exit door of the right compartment.

After sixty-one trials had been given, the period of punishment was increased from fifteen seconds to thirty seconds. The thirty-second interval was used up to the four hundred and sixtieth trial. It was then increased to sixty seconds, but as the crows refused to work, it was decreased after forty trials to fifteen seconds.

After the fiftieth trial, the series regularly consisted of five trials, and four series were, as a rule, given each day.

Table 5 presents for problem 2, as does table 1 for problem 1, a summary of the choices for each of five hundred trials given each crow. As in table 1, the settings are indicated at the top of the various columns, and under each setting appear the results of the various trials for that particular setting.

It appears from table 5 that number 3, in the case of setting No. 1, 7-9, failed with few exceptions in its first choices until the three hundred and forty-third trial, whereas thereafter it usually succeeded. On the contrary, in the case of setting No. 6, 5-9, we observe that the bird almost never succeeded in selecting the right compartment in the first trial. Moreover, there is absolutely no evidence of improvement.

TAB
RESULTS FOR CROW NU

T.	S. 1 7.8.9	T.	S. 2 6.7.8.9	T.	S. 3 2.3.4.5.6.7	T.	S. 4 4.5.6.7	T.	S. 5 1.2.3.4.5.6
1	9.8	2	9.7	3	{7.6.7.7 7.7.3a	4	6.7.5a	5	6.2a
11	8a	12	7a	13	3	14	5a	15	2
21	{9.9.9.7 9.8	22	9.7a	23	2.6.7.3a	24	{4.7.4.4 4.5a	25	1.2a
31	7.9.8	32	6.9.7	33	4.3a	34	4.4.7.5	35	4.4.3.2
41	8	42	6.6.7	43	3	44	4.4.5	45	3.2
51	7.9.8	52	{6.6.6.6 7	53	2.3	54	4.4.4.5	55	3.2
61	7.8	62	6.7	63	3	64	{4.4.4.4 4.7.5	65	2
71	{7.9.7.7 7.8	72	{6.8.6.6 6.7	73	{2.2.2.7 2.3	74	{4.4.6.7 7.7.5	75	2
81	7.8	82	8.9.7	83	{4.5.6.7 7.7.7.3	84	{4.4.7.7 7.4.4.5	85	1.1.2
91	7.8	92	6.7	93	{5.6.7.7.7.6 4.5.7.2.3	94	4.5	95	2
103	7.8	104	6.7	105	4.5.6.7.3	106	{6.7.7.7.6 4.7.4.5	107	{3.4.5.1.1.1 3.1.1.5.3.2
113	8	114	6.7	115	7.7.7.3	116	7.5	117	2
123	7.8	124	6.7	125	3	126	4.5	127	2
133	7.8	134	6.7	135	3	136	5	137	3.4.5.1.2
143	7.8	144	6.7	145	{4.5.6.7.5 6.2.2.3	146	4.5	147	1.2
153	7.8	154	6.7	155	2.3	156	4.5	157	2
163	7.8	164	6.7	165	2.3	166	4.5	167	1.2
173	7.8	174	6.7	175	2.3	176	4.5	177	1.2
183	7.8	184	6.7	185	2.3	186	4.5	187	1.2
193	7.8	194	6.7	195	2.3	196	6.7.4.5	197	2
		204	6.7	205	3	206	4.5	207	{3.4.5.6 1.2
213	7.8	214	6.6.7	215	2.3	216	4.5	217	2
223	8	224	6.7	225	4.5.2.3	226	4.5	227	3.4.5.1.2
233	7.8	234	6.7	235	3	236	4.5	237	2
243	8	244	7	245	2.3	246	7.5	247	1.2
253	{7.7.7.7.7 7.7.7.7.7	254	6.6.7	255	3	256	{4.4.4.4 4.7.5	257	3.4.2
263	8	264	8.7	265	3	266	4.6.7.5	267	2
273	7.7.8	274	9.6.8.7	275	{4.2.2.6 5.3	276	4.4.4.7.5	277	3.2
283	7.7.8	284	8.7	285	3	286	5	287	3.2

LE 5

MBER 3 IN PROBLEM 2

T.	S. 6 5.6.7.8.9	T.	S. 7 1.2.3.4.5 6.7.8.9	T.	S. 8 3.4.5.6.7.8	T.	S. 9 7.8.9	T.	S. 10 2.3.4.5
6	8.9.9.6a	7	2a	8	3.4a	9	9.8a	10	3a
16	6a	17	2a	18	4a	19	8	20	3a
26	6a	27	2a	28	4a	29	8	30	3
36	{5.8.7.5 5.5.6a 5.5.6a	37	3.2	38	{3.3.3.3 3.3.3.4	39	7.8a	40	3
46		47	2	48	4	49	{7.7.7.7.7 7.7.9.7.8a 7.7.7.9.7 7.9.7.8	50	4.2.3
56	5.6	57	2	58	3.3.3.3.4	59		60	2.3
66	8.9.7.6			68	{3.3.6.8 5.6.4	69	8	70	3
76	8.5.6	77	2	78	3.4	79	7.8	80	3
86	6	87	1.2	88	4	89	7.8	90	2.3
96	6	97	2						
98	5.6	99	2	100	3.3.6.4	101	7.8	102	3
108	{7.8.9.5 8.6	109	{3.4.5.1 2	110	3.3.3.3.4	111	7.8	112	2.2.3
118	5.5.6	119	3.4.2	120	3.4	121	7.8	122	2.3
128	7.8.9.6	129	3.4.5.2	130	3.4	131	7.8	132	2.4.5.2.3
138	{7.8.5.8 9.6	139	1.2	140	3.4	141	8	142	2.3
148	6	149	{3.4.5.6.7 8.9.5.5 6.7.8.2	150	3.4	151	7.7.7.8	152	2.3
158	8.9.5.5.6	159	2	160	3.4	161	7.8	162	2.3
168	5.6	169	2	170	3.4	171	7.8	172	3
178	5.6	179	1.2	180	3.4	181	7.8	182	2.3
188	5.6	189	2	190	4	191	7.8	192	3
198	5.6	199	2	200	4	201	7.8	202	{4.5.5.5 2.3
203	5.6								
208	5.6a	209	1.2	210	{8.8.3.3 3.4	211	8a	212	2.3
218	7.8.8.6a	219	2	220	4	221	8	222	4.5.4.2.3
228	8.9.6	229	2	230	4	231	7.8	232	4.5.2.3
238	8.9.7.6	239	{3.4.5.6 7.2	240	3.3.3.3.4	241	8	242	3
248	5.6	249	2	250	3.3.3.6.5.4	251	7.7.8a	252	3
258	5.6	259	2	260	{3.3.3.7.8 7.5.3.3.4	261	7.7.8	262	3
268	7.6	269	1.2	270	{7.3.8.6.8 6.3.3.4	271	7.8	272	3
278	8.7.6	279	3.2	280	8.7.6.4	281	7.8	282	3
288	9.8.7.6	289	3.1.2	290	4	291	7.7.8	292	4.3

TABLE 5—
RESULTS FOR CROW NUMBER

T.	S. 1 7.8.9	T.	S. 2 6.7.8.9	T.	S. 3 2.3.4.5.6.7	T.	S. 4 4.5.6.7	T.	S. 5 1.2.3.4.5.6
293	8	294	8.7	295	3	296	4.4.5	297	3.2
303	8	304	6.7	305	5.4.3	306	{4.4.7.6.4 6.4.6.7.5	307	3.1.2
313	7.7.8	314	7	315	3	316	4.4.4.6.5	317	3.2
323	8	324	8.7	325	3	326	4.5	327	4.3.1.2
333	7.8	334	8.7	335	4.3	336	6.7.4.5	337	1.2
343	8	344	6.7	345	2.2.3	346	7.5	347	2
353	8	354	8.7	355	3				
361	8	362	8.7	363	2.2.3	364	6.5	365	3.2
371	8	372	7	373	4.3	374	7.6.5	375	3.2
381	7.8	382	7	383	4.2.2.3	384	4.4.4.5	385	2
391	8	392	8.7	393	3	394	6.5	395	2
401	8	402	8.7	403	3	404	6.5	405	2
411	8	412	6.7	413	2.2.3	414	5	415	3.2
421	7.8	422	6.7	423	3	424	4.4.4.5	425	2
431	8	432	8.7	433	3	434	4.5	435	1.2
441	8	442	8.7	443	4.3	444	6.5	445	2
451	8	452	7	453	2.4.3	454	4.5	455	2
461	8	462	6.7	463	2.4.3	464	7.6.5	465	2
471	8	472	8.7	473	2.4.3	474	7.6.4.5	475	{5.4.5.6 5.5.4.2
481	7.8	482	8.7	483	3	484	7.4.5	485	2
491	8	492	9.8.7a	493	2.3	494	6.4.5	495	4.3.2

RESULTS FOR CROW NU

6	9.9.8a	7	9.7a	8	4.3a	9	5a	10	2a
16	8a	17	7a	18	3	19	5	20	{5.6.6.6 2a
26	7.8	27	6.7	28	4.6.7.3	29	{4.6.7.7 4.4.5a	30	4.5.3.2a
36	8	37	8.9.7	38	{4.6.2.7 7.3a	39	7.5a	40	2a
46	7.9.8	47	7	48	{4.6.5.7.2 7.7.2.2.3a	49	5	50	3.6.2
56	8	57	7	58	2.7.2.6.3a	59	4.4.4.7.5	60	2
66	9.8	67	7a	68	2.3	69	7.4.7.6.5a	70	3.2
76	7.7.8	77	{6.8.6.8 9.8.7a	78	{2.7.2.6 2.3	79	4.7.5a	80	2a
86	8	87	6.8.7	88	2.7.7.3	89	5	90	6.1.4.6.2

*Continued***3 IN PROBLEM 2—Continued**

T.	S. 6 5.6.7.8.9	T.	S. 7 1.2.3.4.5. 6.7.8.9	T.	S. 8 3.4.5.6.7.8.	T.	S. 9 7.8.9	T.	S. 10 2.3.4.5
298	8.7.6	299	2	300	4	301	7.7.8	302	4.3
308	5.6	309	8.7.5.3.2	310	7.6.4	311	8	312	5.3
318	7.6	319	2	320	8.7.6.5.4	321	7.8	322	4.3
328	7.6	329	4.3.1.2	330	3.4	331	8	332	5.5.3
338	8.6	339	1.2	340	3.3.3.4	341	7.7.7.8	342	4.3
348	7.6	349	{4.3.4.3 1.1.2	350	3.3.3.4	351	7.8	352	5.4.3
356	7.8.7.6	357	2	358	8.7.5.4	359	8	360	3
366	7.6	367	4.3.2	368	5.4	369	8	370	5.4.3
376	8.7.6	377	3.2	378	6.5.4	379	8	380	2.3
386	8.7.6	387	2	388	7.6.5.4	389	9.8	390	5.4.3
396	8.7.6	397	2	398	5.4	399	7.7.8	400	2.2.2.3
406	7.6	407	2	408	8.7.6.5.4	409	7.7.8	410	3
416	7.6	417	4.3.2	418	3.3.4	419	8	420	4.3
426	5.5.6	427	3.2	428	3.4	429	8	430	3
436	8.6	437	3.2	438	3.4	439	7.8	440	2.4.4.3
446	6	447	2	448	6.5.4	449	8	450	2.2.4.3
456	8.7.6	457	1.2	458	6.7.5.4	459	7.8	460	2.2.3
466	7.6	467	3.2	468	3.5.4	469	7.8	470	2.3
476	{8.7.9.8 7.8.8.7 8.8.6 8.7.8.5 7.7.9.8 8.6a	477	2	478	4	479	8	480	5.4.3
486	{8.6a 9.8.8.8 5.6	487	2	488	3.4a	489	7.8	490	2.2.3
496		497	2	498	3.3.4	499	9.9.8	500	3

NUMBER 4 IN PROBLEM 2

1	9.9.6a	2	2a	3	4a	4	7.8	5	{5.5.2.2 5.5.3a
11	6a	12	3.2a	13	4a	14	8a	15	4.3a
21	6	22	5.6.6.8.2	23	4	24	7.8	25	{4.5.5.2 2.3 5.5.2.5.2 5.5.2.3a
31	{5.9.5.9.8 5.9.5.6a	32	4.2	33	3.3.3.4a	34	8	35	4.3
41	9.9.6a	42	1.6.2a	43	{5.8.3.8 4a	44	7.9.7.8	45	4.5.3
51	5.9.8.6	52	3.2	53	4	54	8	55	2.5.3a
61	{9.5.7.9 7.9.8.7 9.5.8.6a	62	{3.9.1.8 4.2	63	3.8.4	64	8	65	2.5.2.4.3
71	9.8.6	72	{9.1.6.3 7.2	73	3.8.8.4a	74	7.9.8	75	2.2.3a
81	{8.9.8.7 6	82	8.8.2	83	3.4	84	8	85	3
91	8.8.6	92	{8.4.7.1 8.9.1.4 8.4.2a	93	4	94	7.7.8	95	

TABLE 5—
RESULTS FOR CROW NU

S. 1		S. 2		S. 3		S. 4		S. 5	
T.	7.8.9	T.	6.7.8.9	T.	2.3.4.5.6.7	T.	4.5.6.7	T.	1.2.3.4.5.6
96	9.8	97	{8.8.8.6 8.7a						
98	{7.9.7.7 7.7.7.8	99	8.9.9.7	100	{7.4.2.6 2.4.4.3a	101	{7.7.6.4 4.5	102	2
108	7.9.8	109	6.9.8.7	110	3	111	{4.7.4.4 6.4.6.5	112	2
118	8	119	6.8.7	120	{2.6.4.6 2.7.3	121	4.6.4.5a	122	1.1.4.2
128	7.8	129	7	130	3	131	{4.6.6.4 6.7.4.5	132	{1.3.3.1 5.2
138	7.8	139	7	140	2.4.2.3	141	4.6.5	142	1.3.2
148	7.8	149	{8.6.8.6 6.8.6.6.7	150	{5.2.7.2 5.2.4.2.3	151	4.5a	152	2
158	8	159	8.8.8.7	160	3	161	4.4.5	162	2
168	7.7.8	169	6.8.8.7	170	2.3	171	5	172	1.6.4.2
178	8	179	7	180	{5.2.2.6 4.3	181	{6.4.4.7 4.6.6.5	182	2
188	8	189	7	190	4.2.3	191	5	192	1.3.2
198	7.8	199	6.7	200	4.2.3	201	6.5	202	3.1.4.2
208	8	209	7	210	5.3	211	{4.4.7.6 4.5a	212	2
218	8	219	{8.6.6.9 7	220	4.3	221	7.5	222	2
228	7.8	229	6.7	230	2.4.3	231	{4.4.4.7.4 4.6.4.4.5	232	1.3.2
238	7.8	239	7	240	3	241	7.7.4.5	242	2
248	8	249	7	250	2.3	251	4.5	252	1.2
258	7.8	259	6.7	260	2.4.5.6.3	261	5	262	2
268	8	269	6.7	270	2.3	271	4.5	272	1.2
278	7.8	279	6.7	280	2.3	281	4.5	282	1.3.1.2
288	7.9.7.8	289	6.7	290	2.3	291	5	292	1.2
298	7.8	299	6.7	300	2.4.2.3	301	4.5	302	{1.3.5.3 4.1.2
308	7.8	309	7	310	2.3	311	6.7.5	312	1.2
318	8	319	8.8.7	320	{6.7.4.2 2.3	321	4.5	322	1.3.4.2
328	8	329	7	330	2.4.2.3	331	5	332	6.3.4.1.2
338	8	339	8.8.6.7	340	2.2.3	341	5	342	3.2
348	8	349	7	350	2.3	351	4.5	352	1.2
356	8	357	6.7	358	7.5.2.3	359	7.4.5	360	6.3.4.1.2
366	8	367	8.7	368	{2.4.6.7 4.5.3	369	5	370	3.4.5.2
376	7.8	377	5.7.8.6*	378	3	379	5	380	3.4.5.2
386	7.8	387	6.7	388	2.3	389	4.5	390	1.3.4.2

* Mistake, setting 5-9

Continued

MBER 4 IN PROBLEM 2

T.	S. 6 5.6.7.8.9	T.	S. 7 1.2.3.4.5 6.7.8.9	T.	S. 8 3.4.5.6.7.8	T.	S. 9 7.8.9	T.	S. 10 2.3.4.5
103	6	104	8.2	105	4	106	9.8	107	3
113	{9.7.8.7 9.8.6	114	{3.8.7.4 6.2	115	{3.6.3.3 3.4	116	8	117	2.3
123	5.9.7.6	124	2	125	3.6.8.6.4	126	{7.7.7.7.. 7.8	127	3
133	5.8.8.6	134	{1.3.8.7 8.6.7.2	135	{3.5.3.7 3.6.4	136	{7.9.7.7 7.8	137	2.2.3a
143	9.5.8.6	144	{1.8.5.7 1.4.5.3.2	145	3.4	146	8	147	2.3
153	{8.7.8.7 6	154	2	155	3.4	156	7.8	157	3
163	8.9.9.6	164	2	165	3.3.4	166	8	167	2.2.5.2.3
173	8.8.6	174	2	175	4	176	8	177	3
183	8.6	184	2	185	3.3.3.5.4	186	7.7.7.8	187	2.2.3
193	6	194	{3.6.7.8 5.4.6.3 4.3.2	195	4	196	7.9.7.8	197	4.3
203	{7.9.7.7 8.6	204	2	205	4	206	7.8	207	3
213	6	214	{3.4.9.5 3.2	215	3.3.4	216	7.8	217	2.3
223	{9.7.5.8 7.8.5.7.6	224	3.1.2	225	5.3.3.3.4	226	9.7.8	227	2.3
233	5.6	234	3.2	235	5.3.7.3.4	236	8	237	2.3
243	8.8.6	244	2	245	3.4a	246	8	247	2.3
253	6	254	3.4.3.2	255	4	256	8	257	3
263	{7.8.7.8.7 7.5.8.7.6	264	2	265	4	266	8	267	2.2.2.3
273	5.6	274	3.4.2	275	{8.7.7.3 5.7.4	276	7.8	277	2.3
283	5.6	284	3.4.2	285	5.6.4	286	7.8	287	4.2.3
293	5.6	294	2	295	3.4	296	7.8	297	2.4.2.3
303	8.6	304	1.2	305	5.7.6.4	306	7.8	307	3
313	5.7.8.6	314	{1.3.4.5 6.7.5	315	{7.6.7.6 7.4	316	9.9	317	2.3
323	8.7.5.6	324	5.6.7	325	5.7	326		327	2.3
333	6	334		335		336		337	2.3a
343	7.8.6	344	1.5	345		346		347	3
353	8.9.5	354	{1.5 6.2	355					
361	8.7.8.5.7	362		363		364	2.5	365	
374	{5.7.8.6 5.5.6	372		373		371		375	
381	5.6	382		383		384		385	
391	5.4	392		393		394		395	

TABLE 5—
RESULTS FOR CROW NUMBER

S. 1		S. 2		S. 3		S. 4		S. 5	
T.	7.8.9	T.	6.7.8.9	T.	2.3.4.5.6.7	T.	4.5.6.7	T.	1.2.3.4.5.6
396	8	397	6.7	398	4.4.5.6.3	399	4.5	400	1.3.4.5.2
406	7.8	407	8.6.7	408	2.3	409	4.5	410	2
416	7.8	417	7	418	{2.4.5.2.4 2.4.2.4.3}	419	5	420	1.2
426	7.9.7.8	427	6.7	428	2.3	429	4.6.4.5	430	2
436	8	437	6.7	438	3	439	5	440	6.2
446	8	447	{6.8.8.6 8.6.7}	448	{2.2.4.5 6.2.4.2.3}	449	4.5	450	2
456	8	457	6.7	458	2.4.2.3	459	4.5	460	3.2
466	8	467	{8.6.8.8 6.7}	468	3	469	4.5	470	3.4.3.2
476	8	477	7	478	2.4.5.2.3	479	4.5	480	1.2
486	8	487	8.7	488	2.4.2.3	489	7.6.5a	490	1.2
496	7.8	497	7	498	2.3	499	4.6.7.5	500	2

*Concluded*4 IN PROBLEM 2—*Concluded*

T.	S. 6	T.	S7	T.	S. 8	T.	S. 9	T.	S. 10
	5.6.7.8.9		1.2.3.4.5 6.7.8.9		3.4.5.6.7.8		7.8.9		2.3.4.5
401	6	402	2	403	4	404	8	405	3
411	5.7.8.5.6	412	3.4.2	413	3.4	414	7.9.7.8	415	2.3
421	5.7.5.7	422	2	423	8.3.4	424	7.8	425	2.3
	8.6								
431	6	432	3.4.2	433	3.4	434	8	435	4.3
441	6	442	2	443	6.6.7.3.4	444	7.8	445	2.3
451	5.8.8.5	452	2	453	3.4	454	8	455	2.3
	7.8.5.6a								
461	5.6	462	3.1.2	463	3.4	464	7.7.8	465	2.3
471	5.7.5.6	472	2	473	3.4	474	8	475	2.4.2.3
481	8.6	482	2	483	3.4	484	7.8	485	3
491	5.6	492	2	493	3.4	494	7.8	495	2.3

TABLE 6
DAILY SERIES AND AVERAGES, WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES
PROBLEM 2

Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July 6	3	0	3				July 6	3	0	3			
" "	3	0	3				" "	3	0	3			
" 7	2	0	2	0	8	0:8.	" "	2	0	2	0	8	0:8.
" "	2	0	2				" "	2	0	2			
" "	2	1	1				" "	2	0	2			
" 8	2	1	1	2	6	1:3.	" "	2	0	2	0	8	0:8.
" "	1	0	1				" 8	1	0	1			
" "	3	1	2	1	3	1:3.	" "	3	2	1	2	2	1:1.
" 10	3	0	3				" 10	3	2	1			
" "	2	0	2				" "	2	0	2			
" "	3	0	3				" "	3	0	3			
" "	2	1	1	1	9	1:9.	" "	2	0	2	2	8	1:4.
" 11	3	0	3				" 11	3	0	3			
" "	2	0	2				" "	2	1	1			
" "	3	0	3				" "	3	1	2			
" "	2	1	1	1	9	1:9.	" "	2	0	2	2	8	1:4.
" 12	2	1	1				" 12	2	0	2			
" "	3	1	2				" "	3	0	3			
" "	3	2	1				" "	3	1	2			
" "	0	2		4	6	1:1.50	" "	2	1	1	2	8	1:4.
" 13	5	0	5				" 13	5	2	3			
" "	1	1	4				" "	3	2				
" "	2	3	3	12		1:4.	" "	1	4	6	9		1:1.5
" 14	1	1	4				" 14	5	0	5			
" "	3	0	3				" "	3	0	3			
" "	1	1					" "	2	0	2			
" "	2	3	4	11		1:2.75	" "	5	0	5	0	15	0:15
" 15	5	0	5				" 15	5	1	4			
" "	2	3					" "	5	1	4			
" "	1	4					" "	2	3	4			
" "	2	0	5	12		1:2.40	" "	2	0	2	4	13	1:3.25
" 16	3	3					" 16	5	1	4			
" "	0	5					" "	3	2				
" "	0	5					" "	5	2	3			
" "	2	3	4	16		1:4.	" "	1	4	7	13		1:1.85
" 17	0	5					" 17	5	1	4			
" "	2	3					" "	2	3				
" "	0	5					" "	2	3				
" "	2	3	4	16		1:4.	" "	0	5	5	15		1:3.
" 18	1	1	4				" 18	5	1	4			
" "	5	0	5				" "	1	1	4			
" "	1	1					" "	5	1	4			
" "	1	4	3	17		1:5.66	" "	2	3	5	15		1:3.
" 19	1	1	4				" 19	3	2				
" "	5	0	5				" "	2	3				
" "	2	3					" "	1	4				
" "	0	5	3	17		1:5.66	" "	4	1	10	10		1:1.
" 20	5	0	5				" 20	5	3	2			

TABLE 6—*Continued*
DAILY SERIES AND AVERAGES, WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

PROBLEM 2—*Continued*

Crow Number 3

Crow Number 4

Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July 20	5	0	5				July 20	5	1	4			
" "	5	3	2				" "	5	3	2			
" "	5	1	4	4	16	1:4.	" "	5	2	3	9	11	1:1.22
" 21	5	2	3				" 21	5	0	5			
" "	5	1	4				" "	5	3	2			
" "	5	0	5				" "	5	3	2			
" "	5	1	4	4	16	1:4.	" "	5	1	4	7	13	1:1.85
" 22	5	3	2				" 22	5	2	3			
" "	5	1	4				" "	5	0	5			
" "	5	2	3	8	12	1:1.50	" "	5	1	4	3	17	1:5.66
" "	5	2	3	2	3	1:1.05	" "	5	3	2	3	2	1: .66
" 23	5	2	3				" 23	5	2	3			
" 24	5	2	3				" 24	5	2	3			
" "	5	1	4				" "	5	4	1			
" "	5	2	3	7	13	1:1.85	" "	5	2	3	10	10	1:1.
" 25	5	3	2				" 25	5	3	2			
" "	5	1	4				" "	5	1	4			
" "	5	0	5				" "	5	0	5			
" "	5	1	4	5	15	1:3.	" "	5	0	5	4	16	1:4.
" 26	5	2	3				" 26	5	0	5			
" "	5	1	4				" "	5	1	4			
" "	5	2	3				" "	5	1	4			
" "	5	2	3	7	13	1:1.85	" "	5	0	5	2	18	1:9.
" 27	5	1	4				" 27	5	1	4			
" "	5	1	4				" "	5	1	4			
" "	5	1	4				" "	5	0	5			
" "	5	1	4	4	16	1:4.	" "	5	1	4	3	17	1:5.66
" 28	5	2	3				" 28	5	1	4			
" "	5	1	4				" "	5	3	2			
" "	5	0	5	3	12	1:4.	" "	5	1	4	5	10	1:2.
" 30	5	0	5				" 30	5	1	4			
" "	5	2	3				" "	5	0	5			
" "	5	0	5				" "	5	2	3			
" "	3	1	2	3	15	1:5.	" "	3	1	2	4	16	1:4.
" 31	5	3	2				" 31	5	1	4			
" "	5	1	4				" "	5	2	3			
" "	5	1	4	5	10	1:2.	" "	5	2	3	5	10	1:2.
Aug. 1	5	2	3				Aug. 1	5	3	2			
" "	5	2	3				" "	5	2	3			
" "	5	1	4	6	14	1:2.33	" "	5	1	4			
" 2	5	3	2				" "	5	0	5	6	14	1:2.33
" "	5	1	4				" "	5	1	4			
" "	5	3	2	7	8	1:1.14	" "	5	5	0	6	9	1:1.5
" 3	5	2	3				" "	5	1	4			
" "	5	2	3				" "	5	0	5			
" "	5	1	4	5	10	1:2.	" "	5	2	3	3	12	1:4.
" 4	5	2	3				" "	5	1	4			

TABLE 6—*Continued*DAILY SERIES AND AVERAGES, WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

PROBLEM 2													
Crow Number 3							Crow Number 4						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug. 4	5	2	3				Aug. 4	5	1	4			
" "	5	2	3				" "	5	2	3			
" "	5	0	5	6	14	1:2.33	" "	5	3	2	7	13	1:1.85
" 5	5	2	3				" 5	5	2	3			
" "	5	3	2				" "	5	2	3			
" "	5	3	2	8	7	1: .87	" "	5	2	3	6	9	1:1.5
" 6	5	0	5				" 6	5	1	4			
" "	5	2	3				" "	5	0	5			
" "	5	0	5				" "	5	2	3			
" "	5	1	4	3	17	1:5.66	" "	5	1	4	4	16	1:4.
" 7	5	2	3				" 7	5	2	3			
" "	5	2	3				" "	5	2	3			
" "	5	1	4				" "	5	1	4			
" "	5	1	4	6	14	1:2.33	" "	5	1	4	6	14	1:2.33
" 8	5	2	3	2	3	1:1.5	" 8	5	2	3	2	3	1:1.5

The summary of choices given in table 5 is chiefly valuable as a means of detecting reactive tendencies. But it also indicates that neither crow succeeded in solving the problem. We had supposed, from our previous experience, that within two or three weeks the crows would be choosing the second compartment at the left with ease, but as a matter of fact, with the appearance and disappearance of the more or less unsatisfactory reactive tendencies apparent in table 5, they continued their work over a period of several weeks without mastering the situation. It seemed utterly useless to continue the experiment with this problem beyond the five hundredth trial. Had there been any consistent improvement, even although extremely slow, we should have felt justified in continuing the training.

The presentation of results in table 6 is of interest primarily because the reader can from it see the fluctuation in the measure of success during the long continued period of training. We have presented in this table for each bird the number of correct and incorrect first choices by series of trials under each date. Following the results appear the ratios of successes to failures for each day.

In table 7 the ratios of correct to incorrect first choices are given for the trials by groups of twenty-five, in order that the influence of "good" and "bad" days may be fairly distributed.

TABLE 7
RATIOS OF CORRECT TO INCORRECT FIRST CHOICES IN PROBLEM 2 BY
GROUPS OF TWENTY-FIVE

Trials	Crow No. 3	Crow No. 4
1- 25	1: 7.30	1: 5.25
26- 50	1: 3.16	1: 5.25
51- 75	1: 4.00	1: 3.16
76-102	1: 2.00	1: 4.40
103-127	1: 5.25	1: 1.77
128-152	1: 5.25	1: 1.40
153-177	1: 5.25	1: 1.08
178-202	1: 3.16	1: 1.77
203-227	1: 3.16	1: 1.77
228-252	1: 1.50	1: 2.12
253-277	1: 2.57	1: 1.50
278-302	1: 1.12	1:11.50
303-327	1: 3.16	1: 5.25
328-352	1: 7.33	1: 2.57
353-375	1: 1.87	1: 1.55
376-400	1: 2.12	1: 5.25
401-425	1: 1.15	1: 1.77
426-450	1: 1.77	1: 1.50
451-475	1: 3.16	1: 3.16
476-500	1: 2.12	1: 2.12

The probability of a correct choice in this experiment, supposing that chance alone is involved⁵, is one to four. At the beginning of the experiment, it is noted (table 7) that the ratio for number 3 was 1 to 7.30; that for number 4, 1 to 5.25. For neither bird does the ratio fall as low as 1 to 1 at any time during the training. The nearest approach to this measure of success was made by crow number 4 in the trials 153 to 177, for which the ratio was 1 to 1.08.

Further, it is to be noted that neither crow shows a steady increase in the number of correct choices. There is, instead, for each, an increase up to a certain point, then a sudden decrease, followed by a more or less rapid increase. Number 3 exhibits three well marked improvement waves. Beginning with the ratio 1 to 7.3, there is fairly constant improvement until the ratio stands 1 to 2. Then a backsliding occurs which, for the next twenty-five trials results in a ratio of 1 to 5.25. Slowly the bird improves again, achieving, after about three hundred trials, a

⁵ Of course the previous work on Problem 1 influenced the birds very markedly in their early trials.

ratio of 1 to 1.12. But this, after fifty additional trials, is replaced by a ratio of 1 to 7.33. Immediately thereafter, rapid improvement sets in, and shortly a ratio of 1 to 1.15 results.

The same in general holds for number 4. At the end of fifty trials, its ratio is 1 to 5.25. After one hundred and seventy-seven trials, it is 1 to 1.08. Then the number of correct first choices slowly decreases until finally, at about the three hundredth trial, the ratio is 1 to 11.50. There follows, during the next seventy-five trials, improvement which results in the ratio of 1 to 1.55, which, in turn, is immediately followed by the ratio of 1 to 5.25.

These fluctuations in the ratio of right to wrong first choices are indicative of the appearance, "trying out," and disappearance of more or less satisfactory reactive tendencies. The fact that neither bird achieved a ratio of 1 to 0 indicates that no reactive tendency appeared which was wholly satisfactory, or in other words, led to the complete solution of the problem. These various reactive tendencies we should describe, in the case of a human subject, as the "trying out" of ideas, but it is unnecessary for us to have recourse to this mode of psychological description in the case of the crows. They may or may not have had ideas corresponding to those which would have existed in the ordinary human subject. In any event, their behavior is strikingly similar to that of the human subject of a low level of intelligence.

ANALYSIS OF THE REACTIONS OF CROW NUMBER 3, ♂

An analysis of the data of table 5 renders these fluctuations of the ratio of correct to incorrect first choices at once intelligible and deeply significant. We shall attempt an analytical examination of the results for each subject in order to bring the several reactive types and tendencies into prominence.

For crow number 3 the first thirty or forty trials in problem 2 in large measure destroyed the subject's well formed habit of choosing first, as a result of previous training in problem 1, the first compartment at the right. The bird then began to choose very frequently the first compartment at the left and to distribute the remainder of its choices among the other compartments, until the right one happened to be chosen. From the beginning of work on this problem, the persistency of number 3,

as also of number 4, in reentering the same compartment was surprising. Examples of this are trials three, twenty-one, twenty-four, thirty-eight, forty-nine, fifty-nine, sixty-four, and so on, for number 3.

By the time number 3 had been given one hundred trials, a habit of always going to the first compartment at the left, and after receiving the punishment of confinement in the compartment, entering the one next in order, had become fairly well fixed. For some of the settings, this habit developed earlier than for others. For instance, in settings Nos. 1 and 9, 7-9, this particular reactive tendency appeared first in the thirty-ninth trial, again in the sixty-first, and in the seventy-ninth, when it seems to have been accepted as the most satisfactory method of reacting, and appears as a habit for almost two hundred trials.

With setting No. 2, 6-9, this same reactive tendency appears definitely at about the ninety-second trial; for setting No. 3, 2-7, at about the one hundred and fifty-fifth; for No. 4, 4-7, at the one hundred and twenty-sixth; for No. 5, 1-6, at the one hundred and twenty-sixth; for No. 6, 5-9, at the one hundred and sixty-eighth; for No. 8, 3-8, at the one hundred and twentieth; for No. 10, 2-5, at the one hundred and forty-second; but for No. 7, 1-9, neither this tendency nor any other became well established during the five hundred trials.

In the fifty trials, one hundred and fifty-one to two hundred inclusive, the habit of entering the first compartment at the left, and next the adjacent one, which of course was also the correct one, appeared thirty-eight times. In ten of these fifty trials, the right compartment was entered immediately, and in the remaining two trials, the compartment first entered was the second from the right instead of the second from the left end of the group.

From trials two hundred and forty to two hundred and fifty, a new reactive tendency began to appear. This shortly replaced the one just described. Previously, number 3, when it came out of the first compartment at the left, turned sharply to its left and entered the second compartment, the right one, but now instead of turning to its left, it began to turn to its right, with the result that it faced the compartment which it had just left. Formerly, it had always met with reward when, after

coming out of the first compartment and turning sharply it entered the one directly in front of it, but now it met, instead, with punishment. Nevertheless, it persisted in reentering the wrong compartment, and in trial two hundred and fifty-three it was punished thirteen times for entering compartment 7. It was then aided in finding the right compartment. In this instance, even after the door of the wrong compartment which had been so often reentered had been closed and the door of the right compartment left open beside it, the bird stood for some seconds before the wrong door, cawing and apparently eager to enter.

Naturally the tendency to turn to its right instead of to its left greatly diminished the number of correct first choices by number 3, and completely obliterated the old reactive tendency. Shortly, the number of reentrances diminished to two or three, and the bird began to enter the second compartment from the left, even although it were not facing it after it turned about. This peculiar behavior continued for only a short time, and was followed by a tendency to enter first a compartment near the right end of the series. On escaping from this, it would turn to its right and enter the compartment directly in front of it. Repetition of this performance of course soon brought the bird to the right compartment. In trial after trial, number 3 would enter the first compartment at the right and then work back, compartment by compartment, until it reached the second from the left. Examples of this behavior are trials two hundred and eighty, two hundred and eighty-eight, three hundred and nine, three hundred and twenty, and so on.

After the thorough testing of the reactive tendency just described, no additional habit became well established, but the crow shifted from one method to another. The one most often used was that of entering the third from the left, and on leaving this, turning to the right and entering the compartment before it, which was of course the right one. This method is exhibited for setting No. 2, 6-9, after the two hundred and eighty-fourth trial; for setting No. 5, 1-6, after trial two hundred and seventy-seven; for setting No. 6, 5-9, after trial three hundred and eighteen; and for setting No. 10, 2-5, after trial two hundred and ninety-two. For the other settings, it appeared less frequently.

To complete our comments on the behavior of number 3, we may say that beginning with trial four hundred and sixty-one, the period of punishment was increased from thirty seconds to sixty seconds, since it seemed possible that the crow might improve under this condition. But the punishment was over-severe, and after only a few trials, number 3, as also number 4, began to work very badly indeed. It would move about constantly and excitedly while confined in a compartment, and when the door was opened would rush out and immediately enter another compartment without pause. This random and excited choosing naturally yielded few successes, and by the time forty trials had been given, number 3 was very hesitant about entering any of the compartments and had returned to an earlier habit of wandering about the reaction chamber. When the time of punishment was reduced to fifteen seconds, he very quickly resumed his former method of reacting, and worked quite as assiduously as ever, and with as small a measure of success.

ANALYSIS OF THE REACTIONS OF CROW NUMBER 4, ♀

The behavior of number 4 in problem 2 differs in some respects from that of number 3 and is worthy of brief description. The first fifty trials served to break up the habit of choosing the first compartment at the right. Thereupon, her attention shifted to the opposite end of the series. But this was not so definite as in the case of number 3. Number 4 often went to the first compartment at the left and then to the one next to it, thus requiring but two choices in order to get the right compartment. This tendency became fixed only after two hundred and fifty trials, and even then it was not so definite as for number 3.

It is indicative of the temperamental differences in the two subjects that number 4 should have required assistance in almost twice as many of the first two hundred trials as did number 3. Significant also is the fact that until very late in her training she was not nearly as systematic in her choices as was he. She tended rather more frequently to the compartments near the middle instead of those at the ends, and chose in no definite or predictable way. This naturally resulted in a much larger number of choices before the right compartment was reached than in the case of number 3. Discouragement was pronounced

to the number of mistakes. But at the same time, number 4, just because of the lack of a definite inadequate reactive tendency, happened upon the right compartment more frequently than did number 3. For her, therefore, the ratio of correct to incorrect choices is more favorable than for him. This is true up to about the three hundredth trial, when it appears that number 4 for some reason became more systematic in her work, going most frequently to the first compartment at the left and then to the second. This habit, which had appeared also in the behavior of number 3, had by this time been replaced, and as a result he was more often choosing correctly than she. Number 4 continued to exhibit this reactive tendency rather insistently throughout the remaining trials.

We must conclude from this analysis of the data of table 5 that both crows, in the five hundred trials with problem 2 which were given them, tried and found inadequate all of the reactive tendencies which were immediately available. Toward the end of the experiment, it was evident, especially in the case of number 3, that the bird's only resource was to return to some one of the methods previously employed. Strange as it may seem to the human subject, and especially to those human beings who have a high estimate of the intelligence and originality of the crow, these individuals proved entirely incapable of learning to enter directly the second compartment from the left in a series of compartments.

Doubtless, many readers will object that longer training would almost certainly have enabled our subjects to solve this problem. We cannot deny this possibility, but we must insist that all of the indications of our results are against it, for ordinarily the adequate solution of such a problem as this by an animal of intelligence far lower than the human is achieved by slow improvement. We are of the opinion that the crow is incapable of perceiving and properly reacting to the relation of second from the left, and we do not hesitate to admit that we were very much surprised by this outcome of our experiments, as we had fully expected our subjects, and especially crow number 3, to deal successfully with much more difficult problems than this one.

This opinion rests not solely upon the fact that no steady and consistent improvement occurred as the result of five hundred

trials distributed over a period of thirty-two days, but also upon the observation that in the case of the setting 7-9, which appeared twice in the series, as Nos. 1 and 9, and was therefore presented to each crow one hundred times instead of fifty times, the successes were surprisingly few. In the first presentation of this setting, number 1 in the series, for both of the crows there is marked increase in the number of correct first choices between the beginning and the end of the training. But for the second presentation, number 9 in the series, this is not the case. Crow number 3, in the first ten presentations of this setting, as number 9 in the series, chose correctly only twice, and in the last ten, only four times, while crow number 4 chose correctly in the first ten, four times, and in the last ten, four times. Even without experience they should have chosen correctly twice in ten trials.

This is an easy setting and it is surprising indeed that the crows should not have succeeded in reacting correctly in the latter part of the training. Doubtless their failure is due to the confusing effect of the diverse settings.

We feel that our analysis and discussion of results is inadequate, but the report is already overlong because of the necessarily lengthy description of apparatus and experimental procedure, and we may add only a brief summary.

SUMMARY

1. Two crows, No. 3, a male, and No. 4, a female, about three months old, were presented with two of the simplest types of standard problem in the Yerkes multiple choice apparatus. These problems were: (1) selection of the compartment first at the right in a series; (2) selection of the compartment second at the left; and (3) the other form of problem 1, the selection, namely, of the compartment first at the left.

2. Of these three problems both birds succeeded in solving perfectly, in from fifty to one hundred trials, the first and the last. The second problem was failed to solve in five hundred trials.

3. Various types of errors and selective tendencies appeared during the work on the problems. Examples of these are: (1) to go to the first compartment first at the right because of training in problem 1, the selection of the compartment first at the left; (2) to go to the

next in order ;(3) to reenter the compartment first chosen and then to choose the second from the left of the series; (4) to enter a compartment at or near the right end of the series and on emerging to turn to the right and enter the one directly in front, and so on until the right compartment is entered.

4. Since no one of these types of reaction is satisfactory, the birds shifted from one to another, trying them for varying periods.

5. The male was more tame, bold, and aggressive than the female. Consequently, he made the better showing in the experiments.

The multiple choice method, with four standardized problems, has been employed with pigs, rats, and ring-doves, as well as with crows; and, among human subjects, with normal and defective children and adults and with dementia praecox patients. The results will appear in a series of papers of which this is the first.

A STUDY OF THE BEHAVIOR OF THE PIG *SUS SCROFA* BY THE MULTIPLE CHOICE METHOD

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INTRODUCTION

The multiple choice method of studying ideational and allied forms of behavior was first briefly described in a lecture on the study of human behavior delivered at Cold Spring Harbor in 1913.¹ It has recently been more fully described in a paper which presents the results of its application in the study of the crow.² We shall, in the present report, assume knowledge of the previous descriptions and state only the essential features of the method and its adaptation to the organism observed.

It was devised in the Psychopathic Hospital, Boston, as a means of obtaining comparable records of the ideational behavior of mentally deficient and deranged individuals. But it was also hoped that it might prove widely serviceable as a comparative method for the study of various types of organism.

In many of its essential features, the Yerkes multiple choice method is similar to the Hamilton quadruple choice method,³ but whereas in the latter four reaction-mechanisms are employed and only problems which, strictly speaking, are insoluble are presented to the subject, the present method involves the use of a variable number of reaction-mechanisms and the presentation of soluble problems of a wide range of difficulty.

The experimenter seeks, in using the multiple choice method, to present to his subject, no matter what its type, age, or condition, a problem which may be solved by the perception of a

¹Yerkes, Robert M. The study of human behavior. *Science*, 1914, **39**, pp. 625-633.

²Coburn, Charles A. and Yerkes, Robert M. A study of the behavior of the crow *Corvus Americanus* Aud. by the multiple choice method. *Journal of Animal Behavior*, 1915, **5**, pp. 75-114.

³Hamilton, G. V. A study of trial and error reactions in mammals. *Journal of Animal Behavior*, 1911, **1**, pp. 33-66.

certain constant relation or group of relations within the reaction-mechanisms. For example, the mechanism to be operated may, in the case of one problem, be the middle one of the group, and the total number of mechanisms presented may vary from three to nine. Only by perceiving and appropriately responding to the relation which the experimenter designates as middleness, can the subject solve the problem.

It is necessary only, in the presentation of a varied series of multiple choice problems to a given subject, for the experimenter to devise a type of reaction-mechanism which is appropriate to the action-system of the organism to be observed. We have thus far made use of a simple keyboard for human subjects, while for crows, ring-doves, and rats, we have employed a series of similar boxes, each with entrance and exit doors which can be operated at a distance by the experimenter. The form of device which has proved suitable for the study of pigs will be described in this report.

It has proved very easy to develop suitable mechanisms and we have every reason to suppose that this new method has great advantages over most others for the comparative study of behavior in that essentially the same problems may be presented to extremely different types of subject.

The method has been employed in experiments with normal and defective children, normal and insane adults, crows, and ring-doves.⁴ To all of these subjects, five problems have been presented. They may be described briefly as follows: problem 1, the correct reaction-mechanism, as Problem 1, the correct mechanism at the subject's right; problem 2, the second mechanism at the subject's left (that is, from the end nearest the subject's left); problem 3, alternately the first mechanism at the subject's right and the first at its left; problem 4, the middle mechanism of the series.

It has become increasingly clear, as our investigation has progressed, that the perfect solution of a problem by a given subject is of much less importance as a matter of record than the information concerning the types of reaction and the appearance and disappearance of reactive tendencies during the experimentation. For the solution of a problem is

⁴ The results of our experiments, except in the case of the crow, have been published.

the termination of a series of observations. It is essential, therefore, that the experimenter fix his attention rather on the immediate response of his subject than on the attainment of the solution of problems. We especially call attention to this matter because many experimenters seem to feel dissatisfied with other than speedy and completely positive results. It seems fair to insist that by the multiple choice method positive results are obtained even if a subject cannot solve any of the problems which are presented to it.

Since it is our intention to more fully discuss the essential features and the technique of the multiple choice method elsewhere, we shall here content ourselves with these brief introductory statements and references. It should perhaps be added that only by reading the earlier article on the behavior of the crow can the reader hope to fully understand the present report.

SUBJECTS

The subjects of the experiments which constitute the observational basis for this paper were two Chester white pigs. They were born April 1st, 1914, and they were therefore two months old when, on June 2nd, they were taken to the Field Station from an adjoining farm and placed in the experimental situation. We shall refer to these individuals as the male and the female, since both sexes were represented. The male, however, had been castrated before we obtained the animals.

From the first, individual differences were conspicuous. The male was considerably smaller and less active and energetic than the female; he ate less and showed less initiative. Throughout the period of observation, both animals were in perfect health and at no time was there reason to suppose that either environmental or physiological conditions were unfavorable to our experiments.

From birth these pigs lived practically out of doors, having a yard to run in and a rather open shelter from storm.

Although the experimenters had expected much of the pigs because of the indications from casual observation of their behavior, it may be said at once that they proved far more satisfactory subjects than we had dared to hope. Indeed, they worked so steadily and uniformly through the investigation that there was practically no loss of time due to theft, because of

this unexpectedly favorable relation of subject to method that we were enabled to obtain, during the summer of 1914, the numerous results reported below.

APPARATUS

Fortunately, it was possible at the Franklin Field-Station to locate our apparatus in an orchard convenient to the buildings. A rough shelter was built for the pigs under a large apple tree, and convenient yards were arranged by the appropriate use of wire fencing.

The accompanying figures give a fairly good idea of the experimental situation. In figure 1 A, the multiple choice apparatus appears in the foreground, behind a fence which completely surrounds the enclosure. Immediately in front of the apparatus is a bench for the observer. Systems of weighted cords, conspicuous in 1 A, enable the experimenter to operate the slide doors of the multiple choice boxes.

The arrangement of the yards is made clear by figure 1B and figure 2. It was necessary to be able to isolate the pigs for observation as well as to have the apparatus so arranged that an individual could readily be admitted for a trial and on the completion of its reaction, be returned to its appropriate yard.

The multiple choice apparatus proper consists of nine similar boxes, shown in ground plan in figure 2. They were built of rough boards and numbered conspicuously 1 to 9. Each box is sixty inches long, by twenty inches wide, by forty-eight inches deep, with a slide door at each end. The distance between these doors on the inside of the box is forty-eight inches.

From each of the entrance and exit doors a woven window-weight cord extends upward, through a pulley, then horizontally forward through another pulley, and downward, ending in a weight nearly over the observer's bench. To all of the cords from the entrance doors, white weights were attached; to all from exit doors, black weights. Each weight was sufficient to hold its door in position after the latter had been raised. It was found that this required about ten pounds, and iron window weights served our purpose.

In front of the exit door of each box is a v-shaped food trough which is divided into nine like parts by the partitions between

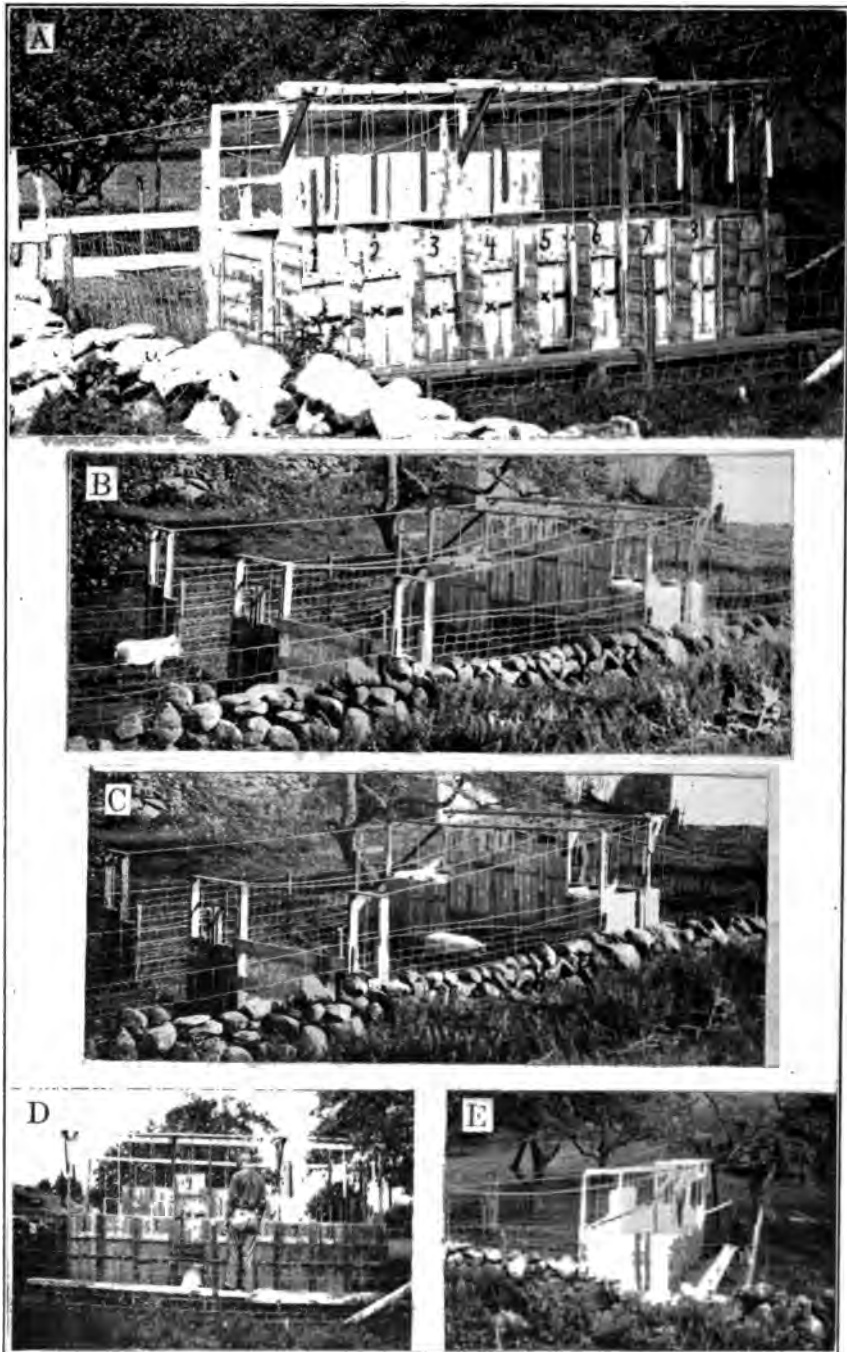


FIGURE 1. Multiple Choice Apparatus for Use with Pigs

A. The reaction-mechanisms from the experimenter's position, showing weighted cords for operating doors. Entrance doors 2 to 6 are raised.

B. The same from the pig's point of view, showing one pig waiting in yard for trial. Entrance doors 2 to 6 raised as in figure A.

C. The same view as that of figure B except that the pig has been admitted to the reaction-space and is about to enter the middle box (no. 4) of those whose doors are open.

D. Here the pig is shown, after appropriate reaction, feeding in the trough of box no. 4. The experimenter appears in the position necessary for manipulation of cords and observation of response.

E. The reaction-mechanisms seen from one end.

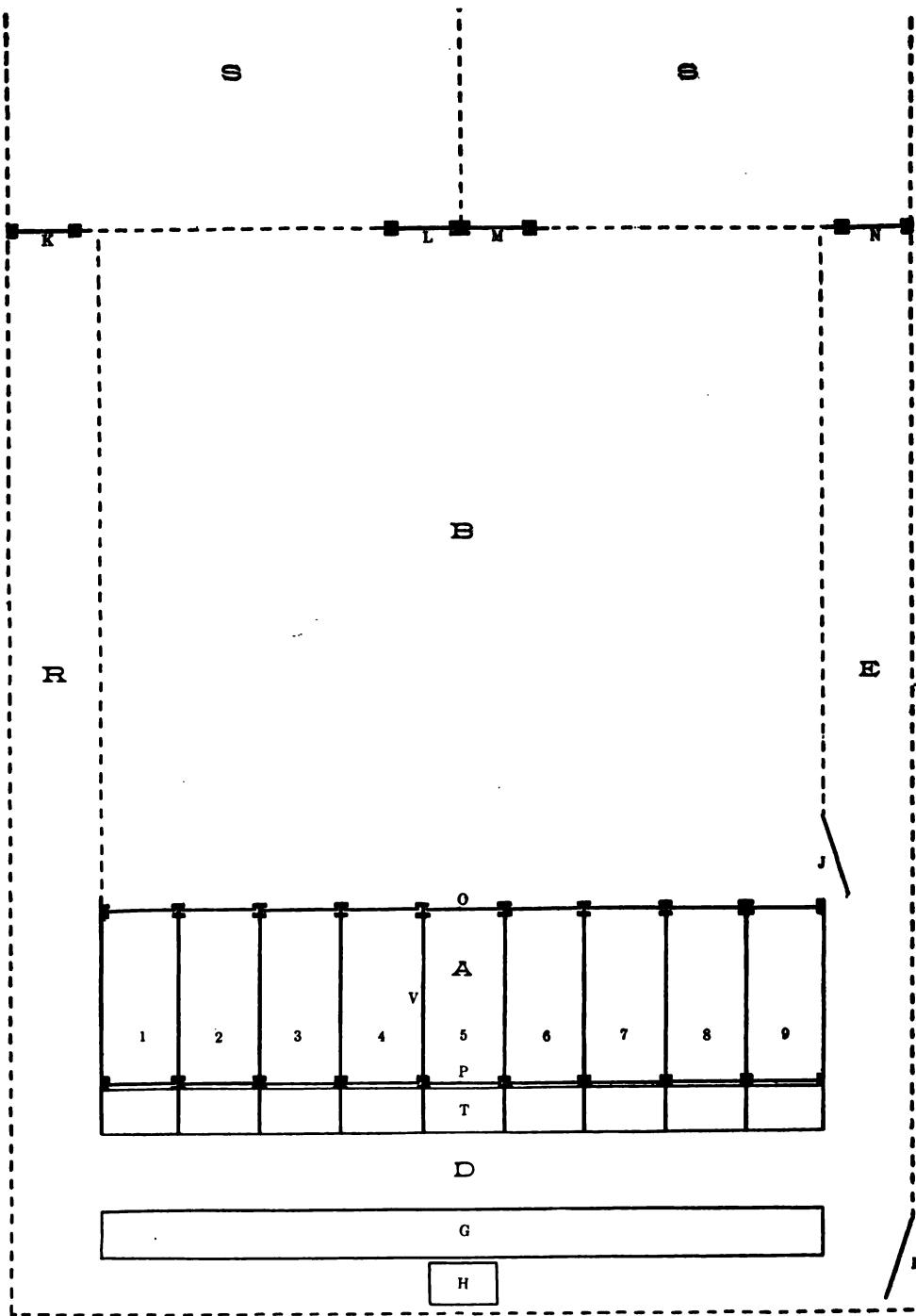


FIGURE 2. Ground Plan of Multiple Choice Apparatus Used for Figs. Scale $\frac{1}{4}$

A, reaction mechanisms, nine similar boxes or stalls; V, stall number 4; O, entrance door of box; P, exit door of box; T, food trough of box; G, observer's stand and H, writing table; D, runway between trough, T, and stand; G, S, yards; B, reaction space; R, E, alleys or runways connecting D with S; I, observer's entrance door to apparatus; J, observer's entrance door to reaction space B; L, M, slide doors between yards and reaction space; K, N, slide doors between yards and alleys.

The weighted cord systems for operating the entrance and exit doors (twenty in all) are not shown in this figure. They may be seen in figure 1, A, B, and C.

boxes. When the exit doors are down, the various parts of the food trough are covered by a horizontally placed sheet of metal which fits closely over them and thus prevents the subjects from obtaining food from the outside of the apparatus.

The large enclosure is divided into four principal parts: (1) the part which contains the reaction-mechanisms with space for the observer's bench, G, and writing table, H, and a passageway for the subject from the exit doors of the apparatus to the yard, S; (2) second, the reaction space which is labelled B in figure 2, in which the subject responded to the multiple choice situation; (3) and finally, the two yards, S, S, from which the subjects started in the case of each trial and to which they returned on the completion of their reaction. K, L, M, and N, designate slide doors between the several portions of the large enclosure, while J and I represent doors which were used by the experimenter.

The entire apparatus was constructed in sections, so that at the end of the season it might readily be taken down and stored.

This brief and very incomplete description will be supplemented somewhat in the section on experimental procedure.

PROBLEMS AND GENERAL METHOD

The four problems enumerated on page 186 were presented to each subject in the order named. For each of these problems, a series of ten settings of the doors was determined upon. These settings differ somewhat from those employed in our study of the crow. It is our intention, so far as possible, to use them with all types of subjects until our observations indicate desirable changes.

We present below for each of the four problems (1) the numbers of the settings, (2) the numbers of the doors open, (3) the total number of doors open in each setting and for the series of ten settings, and (4) the number of the right door.

It was our plan to give each subject an opportunity to respond to each of the ten settings for a given problem in order and to return then to setting 1 and repeat the series. It was found impossible, however, to give ten trials in succession in our early experiments, and in the case of both problems 1 and 2, as a rule a subject was given five trials in succession. For problems 3 and 4 it was found possible to give ten trials in succession.

PROBLEM 1. First Mechanism at the Subject's Right

Settings	Doors open	No. of doors open	No. of right door
1.....	1.2.3.....	3.....	1
2.....	8.9.....	2.....	8
3.....	3.4.5.6.7.....	5.....	3
4.....	7.8.9.....	3.....	7
5.....	2.3.4.5.6.....	5.....	2
6.....	6.7.8.....	3.....	6
7.....	5.6.7.....	3.....	5
8.....	4.5.6.7.8.....	5.....	4
9.....	7.8.9.....	3.....	7
10.....	1.2.3.....	3.....	1
		Total	35

PROBLEM 2. Second Mechanism at the Subject's Left

Settings	Doors open	No. of doors open	No. of right door
1.....	7.8.9.....	3.....	8
2.....	1.2.3.4.....	4.....	3
3.....	2.3.4.5.6.7*	5.....	6
4.....	1.2.3.4.5.6.....	6.....	5
5.....	4.5.6.7.8.....	5.....	7
6.....	1.2.3.....	3.....	2
7.....	2.3.4.5.....	4.....	4
8.....	1.2.3.4.5.6.7.8.9.....	9.....	8
9.....	1.2.3.4.....	4.....	3
10.....	3.4.5.6.7.8.....	6.....	7
		Total	50

* Changed from 3.4.5.6.7 to 2.3.4.5.6.7 after about one hundred trials.

PROBLEM 3. Alternately the First Mechanism at Subject's Right and the First at Its Left

Setting	Doors open	No. of doors open	No. of right door
1.....	5.6.7.....	3.....	5
2.....	5.6.7.....	3.....	7
3.....	1.2.3.4.5.6.....	6.....	1
4.....	1.2.3.4.5.6.....	6.....	6
5.....	4.5.6.7.8.....	5.....	4
6.....	4.5.6.7.8.....	5.....	8
7.....	2.3.4.5.....	4.....	2
8.....	2.3.4.5.....	4.....	5
9.....	3.4.5.6.7.8.9.....	7.....	3
10.....	3.4.5.6.7.8.9.....	7.....	9
		Total	50

PROBLEM 4. Middle Mechanism of the Series

Setting	Doors open	No. of doors open	No. of right door
1.....	2.3.4.....	3.....	3.....
2.....	5.6.7.8.9.....	5.....	7.....
3.....	1.2.3.4.5.6.7.....	7.....	4.....
4.....	7.8.9.....	3.....	8.....
5.....	4.5.6.7.8.....	5.....	6.....
6.....	1.2.3.4.5.6.7.8.9.....	9.....	5.....
7.....	1.2.3.....	3.....	2.....
8.....	2.3.4.5.6.....	5.....	4.....
9.....	3.4.5.6.7.8.9.....	7.....	6.....
10.....	6.7.8.....	3.....	7.....
		Total	50

Both punishment and reward were used in these experiments. The punishment consisted of confinement for a definite interval, usually one minute, in each wrong box entered, while the reward consisted of food which could be obtained in the trough of the right box.

EXPERIMENTAL PROCEDURE

We shall now briefly enumerate, in order to supplement the descriptions of apparatus and methods which have been given, the steps in a regular series of observations.

The experimenter having entered the enclosure with a supply of food, record-book, stop-watch, etc., first raises each of the nine exit doors and places in each section of the trough a quantity of food (sour milk, shelled corn, vegetables). He then lowers the exit doors, thus covering the food, and takes his position on the observation bench. In case both pigs are in the shelter yard, it is next necessary for him to drive one of them into the other yard. This having been done, he may proceed to set the entrance doors for the first trial. Let us suppose that the problem to be presented is problem 1 and that setting 1 is first to be used. In this case the experimenter raises entrance doors 1, 2, and 3. He is now ready to admit one of the pigs to the reaction space B of figure 2. This he does by raising momentarily the appropriate slide door between B and S.

The instant the pig enters the reaction space, the experimenter starts his stop-watch and begins to record the important features of the behavior of the animal, noting especially its approach to the several doors, its tendency to enter boxes and the actual entrance and time of entrance into any one of the three acces-

sible boxes. Let us suppose that the animal enters directly box 3. Immediately the experimenter lowers the entrance door and thus confines the animal in the small compartment as punishment for an incorrect choice. At the expiration of one minute, the entrance door is raised and the pig is allowed to retreat from the box and make another choice. We may now suppose that the animal, after passing in front of boxes 2 and 1, returns to 1 and enters it. The experimenter immediately stops his stop-watch, lowers the entrance door, and, since this box is by definition the right one, he immediately raises the exit door and rewards the animal for correct choice by allowing it to eat for a few seconds. He then, either by speaking to the pig or by touching it with a whip, induces it to pass from the box by way of the passage, D, and the alley, R or E, back to the appropriate yard, S.

Having reset the apparatus, the experimenter now gives the other pig a trial with the same problem and either with the same or with a different setting of the doors.

As a rule, the animals were fed only in the trough of the apparatus. They were almost always hungry, and although sufficiently well fed to keep them growing and in excellent health, they usually seemed fairly hungry at the end of a day's work. In no case was it necessary, in order to induce them to work steadily, to have them extremely hungry.

The influence of visual and olfactory factors was to be expected, and at various points in the investigation, precautions had to be taken against following.

PRELIMINARY TRAINING

On June 2nd the pigs were brought to the Field Station and placed in the shelter yard, and in the afternoon of the same day, they were fed in the trough of the apparatus, all of the doors of the boxes and the yards being raised.

During the next six days they became thoroughly accustomed to the apparatus and learned both to feed in the trough and to make the trip readily from the yards, through the apparatus, and back to the starting point. They very quickly and satisfactorily adapted themselves to the situation, while at the same time becoming thoroughly tame and indifferent to the presence of the experimenter.

On June 9th it seemed fitting to attempt a series of preliminary trials. Each animal was given, in turn, opportunity to secure food in each of the nine boxes. When the subject entered the reaction space, B, the entrance door of a certain box stood open, and as soon as the animal had entered that box, the experimenter closed the door behind it and opened the exit door in front of it, thus enabling it to obtain food. During these preliminary trials, the pigs were in separate yards and were given their trials alternately.

We shall now report the results of our regular experiments.

RESULTS OF EXPERIMENTS

As it is essential to present the data for each trial in the series of experiments, tables 1, 3, 4, 6, 7, 9, and 10 have been constructed after the following manner. At the head of each table stand the several settings, the letter S serving as an abbreviation for setting and the number following it designating the place of the setting in the series. Immediately under the number of the setting appear the numbers of the doors open with the one to be chosen (correct one) printed in bold face type. Below this preliminary information concerning the particular problem in question, appear the results for each of the trials of each subject. The column headed T gives the number of a trial in the total series of trials for a given subject, in a given problem. Following the number of the trial are the numbers of the boxes entered, in the order of entrance. Referring to table 1, we discover that the female in her first trial under problem 1 selected, of the three boxes whose doors were open, first, number 3. She was, of course, punished by being confined in this box for one minute, and on release entered box 1, which was the correct box, and received the reward of food. Or again, in table 3 it may be noted that in trial 146, under problem 2, the female entered, in order, boxes 7, 9, 7, and 8, the group of open doors including 7, 8, and 9, and the box to be entered being number 8.

These tables will enable the reader to obtain quickly definite information concerning the forms of response and the changes therein during the course of experimentation. We shall present the several tables under the problem numbers and reserve further comment for the section on the discussion of results.

DISCUSSION OF RESULTS

The results will now be discussed under the headings of the four problems, and in connection with each a condensed tabular summary of the experiments will be offered, together with such comments as are necessary on the experimental procedure, the behavior of the subjects, and the significance of the various forms of response.

PROBLEM 1

This problem, for which the definition of the correct mechanism is the first at the subject's right, proved extremely easy for the pigs. Incorrect choices were surprisingly few, and the number of trials necessary for the perfect solution of the problem was also surprisingly few for both subjects, the female having chosen correctly throughout a series of ten settings at the end of forty trials and the male having similarly succeeded at the end of forty-five trials.

As is indicated by tables 1 and 2, which contain all of the data for this problem, the experiments were not discontinued at this point, but each individual was given additional opportunity to work out the problem. In the light of our later experience, this was a mistake, but at the time we were unconvinced that the animals were depending upon the relation of the correct mechanism to the other members of the group, and we proceeded further with our observations in order to settle certain points which were in doubt.

From the first it was evident in connection with this problem that the female was more intelligent than the male, and that he tended to be markedly influenced by her. After observations were discontinued with her on June 14th, he reacted very poorly for a number of series, and then again improved and reacted perfectly in the last three series given on June 15th.

In this problem the total number of doors open in the ten settings was 30, and by reference to the data presented on page 10, thirty per cent of these, ten were of course correct. Hence the probability of a correct first choice apart from experience would be 1 to 9. In table 2, it appears from the data of the last column that for the individual that the ratio of correct to incorrect first choices was one the first day of training 1 to 1 for the female and 1 to 2 for the male. It should here be stated that the data for the male are based on the first three

TABLE 1
RESULTS FOR FEMALE IN PROBLEM 1

S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	S. 10
T. 1.2.3	T. 8.9	T. 3.4.5.6.7	T. 7.8.9	T. 2.3.4.5.6	T. 6.7.8	T. 5.6.7	T. 4.5.6.7.8	T. 7.8.9	T. 1.2.3
1 3.1	2 8	3 6.3	4 7	5 2	6 7.8.6	7 7.5	8 4	9 7	10 3.1
11 1	12 8	13 7.4.3	14 7	15 2	16 6	17 5	18 4	19 7	20 1
21 2.1	22 8	23 3	24 7	25 3.2	26 6	27 5	28 4	29 7	30 1
26 1	27 8	28 5.3	29 5.7	30 2	31 6	32 5	33 4	34 7	35 1
36 1	37 8	38 3	39 7	40 2	41 6	42 6.5	43 4	44 7	45 1
46 1	47 8	48 3	49 7	50 2	51 6	52 5	53 4	54 7	55 1
56 1	57 8	58 3	59 7	60 3.2	61 6	62 5	63 5.4	64 7	65 1
1.2.3.4.5	5.6.7.8.9	3.4.5.6	7.8.9	2.3.4.5	6.7.8.9				
1 1	2 5	3 3	4 7	5 2					

RESULTS FOR MALE IN PROBLEM 1

S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	S. 10
T. 1.2.3.1	T. 8	T. 7.6.3	T. 4 9.7	T. 5 2	T. 6 8.7.6	T. 7 6.5	T. 8 4	T. 9 7	T. 10 2.3.1
11 3.1	12 8	13 7.5.3	14 8.9.7	15 2	16 7.6	17 5	18 6.5.4	19 9.7	20 3.2.1
21 2.1	22 8	23 6.3	24 7	25 6.4.2	26 3	27 5	28 4	29 7	30 1
26 1	27 8	28 3	29 8.7	30 2	31 7.6	32 6.5	33 4	34 7	35 1
36 1	37 8	38 3	39 7	40 2	41 6	42 5	43 4	44 7	45 1
46 1	47 8	48 3	49 7	50 2	51 8.6	52 7.7.5	53 8.4	54 8.7	55 1
56 1	57 8	58 3	59 7	60 5.2	61 6	62 5	63 4	64 7	65 3.1
1.2.3.4.5	5.6.7.8.9	3.4.5.6	7.8.9	2.3.4.5	6.7.8.9				
1 3.1	2 5	3 3	4 9.7	5 4.2					
6 3.1	7 5	8 3	9 9.7	10 5.2					
66 3.1	67 8	68 3	69 8.7	70 2	71 6	72 6.5	73 4	74 7	75 1
76 1	77 8	78 3	79 7	80 2	81 6	82 5	83 4	84 7	85 1
1.2.3.4.5	5.6.7.8.9	3.4.5.6	7.8.9	2.3.4.5	6.7.8.9				
11 1	12 5	13 3	14 7	15 2					

TABLE 2
DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES
PROBLEM 1

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 10	1- 5	3	2				June 10	1- 5	2	3			
"	6-10	2	3	5	5	1:1	"	6-10	1	4	3	7	1:2.33
11	11-15	4	1				11	11-15	2	3			
"	16-20	5	0				"	16-20	2	3			
"	21-25	4	1	13	2	1: .15	"	21-25	2	3	6	9	1:1.50
12	26-30	4	1				12	26-30	4	1			
"	31-35	5	0				"	31-35	3	2			
"	36-40	5	0				"	36-40	5	0			
"	41-45	4	1	18	2	1: .11	"	41-45	5	0	17	3	1: .18
13	46-50	5	0				13	46-50	4	1			
"	51-55	5	0				"	51-55	1	4			
"	56-60	4	1				"	56-60	4	1			
"	61-65	4	1	18	2	1: .11	"	61-65	4	1	13	7	1: .54
14	1- 5	5	0	5	0	1:0	14	1- 5	2	3			
							"	6-10	2	3	4	6	1:1.50
							15	66-70	3	2			
							"	71-75	4	1			
							"	76-80	5	0			
							"	81-85	5	0	17	3	1: .18
							"	11-15	5	0	5	0	1:0

problems, the data refer only to first choices in each trial, the column headed R containing the number of correct first choices and that headed W the number of incorrect first choices for each series of trials or for the day. It further appears from this table that five trials constituted the regular series in problem 1, and it should here be stated that the experimenter always resumed experimentation at the point in the regular series of settings at which work had been interrupted. He therefore proceeded in regular order from setting 1 to setting 10 and then returned to setting 1 and repeated the trials.

Further comment on the behavior of the animals in problem 1 is needless, for the task is but slightly more difficult than the acquisition of a simple position habit, and it has already been satisfactorily demonstrated that many of the vertebrates acquire such habits.

PROBLEM 2

For this problem, which is definable as the second mechanism from the subject's left, all of the data for discussion will be found in tables 3, 4, and 5. Again, as in the case of problem 1, the regular series consisted, throughout the training, of five trials, but as many as six such series were given on a single day. Bracketed series appearing, for example, in table 5, under the dates June 23, 24, 25, and 28 and July 1, 2, 3 and 4, were continuous, that is, ten trials were given in succession instead of only five.

For the ten settings of problem 2, the total number of open doors is fifty, and the expectation therefore is that prior to experience an animal will choose correctly once in five times, thus giving a ratio of right to wrong choices of 1 to 4. That this expected ratio does not appear on the first day of experimentation is due to the effect of the previous training in problem 1. The tendency to enter the first box at the left was persistent in both subjects and often that box was re-entered a number of times in spite of punishment. In tables 3 and 4 the data for these statements are presented, and in table 5 it may be noted that on the first day of work on problem 2 neither subject made a single correct first choice.

The ratio of correct to incorrect first choices for the female rapidly, although somewhat irregularly, decreased with experience until on July 4th it stood 1 to .19. On this date she succeeded in choosing correctly in ten successive trials, and was therefore considered to have solved the problem perfectly.

Similarly, the ratio for the male changed fairly rapidly until on July 11th it stood 1 to .11. At this time, although he had not succeeded in choosing correctly in each of the ten settings consecutively, his training was discontinued, for he had already delayed experimentation with the female for a week, and it was perfectly clear that although he made an occasional error, he was capable of perfectly solving the problem.

Whereas the female finished this problem as a result of 390 trials, the male had made only nine out of ten correct choices at the end of 520 trials, when his training was discontinued. We are inclined to think that this is a reliable indication of the difference in docility between these two individuals.

We shall now turn to tables 3 and 4 for a further brief analysis of the reactions.

For about 50 trials in problem 2, both pigs showed the effect of their experience in problem 1. Then the number of correct first choices rapidly increased for each of the ten settings. There were in the case of setting 1 few mistakes on the part of the female after 150 trials, whereas on the part of the male there were more than twice as many incorrect first choices. The same holds in general of each of the other settings, she proving herself much more steady and predictable in response than he. This was doubtless due in a measure to hunger, for it was much more difficult to keep him in the proper condition of eagerness for food than her.

The data of these tables indicate no definite and persistent reactive tendencies during the course of experimentation other than the original acquired tendency to enter the first box at the right in the group and the subsequently acquired tendency to select the second box from the left in the group. Certain of the settings proved very much more difficult than others. Contrary to expectation, difficultness is not directly variable with the number of doors open. Setting 1, for example, as contrasted with setting 6, is much the easier, yet three doors are open in each case. In general, however, it is evidently true that the larger the group of open doors the more difficult it is for the animal to choose correctly and the larger the number of mistakes in a given trial, if the first choice is not correct.

From the behavior of the two pigs in this problem, as contrasted with the first, it is safe to conclude that they are perfectly capable of selecting the proper reaction, mechanism by its relation in a group of similar mechanisms when the number in the group is as large as one and six when the constant relation of the correct mechanism is second from one end. It is further clear that this problem is a much more difficult one for the pigs than problem 1. It is also certain that the difference in difficultness is not entirely due to the difference in the number of experiences. The early behavior of the pig in problem 1, since the early days of the experiment, served merely to overcome the tendency toward the wrong reaction with problem 2. It seems probable that the pig in problem 2 learned from the total number of trials in problem 1 that the correct reaction was the second from the left.

TABLE 3
RESULTS FOR FEMALE IN PROBLEM 2

S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	S. 10
T. 7.8.9	T. 1.2.3.4	T. 3.4.5.6.7	T. 1.2.3 4.5.6	T. 4.5.6.7.8	T. 1.2.3	T. 2.3.4.5	T. 1.2.3.4.5 6.7.8.9	T. 1.2.3.4	T. 3.4.5 6.7.8
1 7.8	1.1.1 2	3 3.7.3.6	4 {6.3.6.2 1.6.5a	5 8.4.5.7a	6 {3.1.3.3 3.1.2a	7 5.2.5.4a	8 6.4.8	9 4.3a	10 {4.8.8 5.7
11 8	7.4.3a† 3	13 5.7.3.5.6	14 6.1.5	15 6.8.4.7	16 3.1.2	17 2.4	18 2.7.4.8	19 4.1.4.4.3	20 8.7
21 7.8	1.4.4 4.3	23 3.6	24 {6.3.6 1.4.5	25 5.8.7	26 2	27 {2.5.2.5 5.3.4	28 7.1.4.8	29 4.3	30 7
31 7.8	4.2.3	33 7.6	34 5	35 {6.8.5.8 6.4.8.7	36 1.2	37 5.4	38 6.8	39 {4.2.4 1.4.3	40 {8.6.3 5.8.6.4
41 7.9.8	4.2.4 1.3	43 7.6	44 5	45 {8.6.5.4 8.5.7	46 3.3.2	47 5.4	48 {7.9.6.7 5.8	49 3	50 8.7
51 8	5.2 3	53 6	54 6.2.5	55 7	56 3.3.2	57 5.2.4	58 8	59 4.3	60 7
61 8	6.2 2.4.1.3	63 7.6	64 6.5	65 8.4.5.7	71 2	72 4	73 {7.9.3 6.2.8	74 4.3	75 5.7
66 7.8	6.7 3	68 {7.5.7 5.6	69 {6.5 6.5	70 8.7	81 1.3.2	82 4	83 7.8	84 2.4.3	85 7
76 7.9.7.8	7.7 4.3	78 7.6	79 6.5	80 {8.5.8.4.7 8.7	81 1.3.2	82 4	83 7.8	84 2.4.3	85 7
86 7.8	8.7 2.4.3	88 7.6	89 6.5	90 8.7	91 3.2	92 2.5.4	93 7.8	94 4.2.3	95 8.7
96 8	9.7 3	98 7.6	99 6.5	100 8.5.7	101 3.2	102 5.4	103 8	104 2.3	105 8.6.7
106 7.8	10.7 4.3	108 7.6*	109 3.6.6.4.5	110 8.5.7	111 1.2	112 5.5.4	113 8	114 3	115 8.6.7
116 7.7.8	11.7 2.4.3	118 5.6	119 3.5	120 8.8.7	121 3.2	122 2.5.4	123 7.8	124 3	125 8.7
126 7.8	12.7 3	128 7.6	129 3.5	130 5.7	131 3.2	132 2.4	133 7.7.4.8	134 3	135 3.8.6.7
136 7.8	13.7 3	138 6	139 2.3.4.5	140 5.7	141 2	142 2.4	143 {3.7.6 3.5.8	144 4.3	145 5.8.7
146 7.9.7.8	14.7 4.3	148 3.6	149 5	150 7	151 3.2	152 5.4	153 8	154 4.2.3	155 8.5.7
156 7.8	15.7 3	158 7.6	159 6.5	160 5.8.7	161 3.2	162 4	163 8	164 3	165 8.7
166 7.8	16.7 3	168 5.6	169 5	170 7	171 2	172 4	173 8	174 3	175 8.7
176 8	17.7 3	178 4.6	179 3.5	180 8.7	181 2	182 5.3.3.4	183 7.8	184 3	185 7
186 8	18.7 3	188 7.6	189 3.5	190 7	191 3.2	192 5.4	193 8	194 3	195 7
196 8	19.7 2.3	198 7.6	199 3.5	200 8.7	201 3.2	202 5.5.4	203 8	204 3	205 7

206	8	207	3	208	7.6	209	2.5	210	8.7	211	1.3,2	212	4	213	8	214	3	215	8.7
216	8	217	3	218	7.6	219	3.5	220	8.7	221	2	222	5.3,4	223	7.7,7.8	224	3	225	8.6,7
226	8	227	4.3	228	6	229	6.5	230	8.7	231	3.2	232	4	233	8	234	3	235	3.6,7
236	8	237	3	238	7.6	239	6.5	240	8.7	241	2	242	4	243	8	244	3	245	7
246	8	247	3	248	6	249	4.3,5	250	7	251	2	252	4	253	8	254	3	255	7
256	8	257	3	258	7.6	259	3.5	260	8.7	261	3.2	262	5.3,2.5,4	263	8	264	3	265	7
266	8	267	3	268	6	269	5	270	8.6,7	271	2	272	3.5,4	273	8	274	4.3	275	7
276	8	277	4.3	278	6	279	6.5	280	8.7	281	3.2	282	4	283	8	284	3	285	7
286	8	287	4.2,3	288	6	289	5	290	7	291	3.2	292	4	293	7.8	294	4.3	295	7
296	8	297	3	298	6	299	5	300	7	301	3.2	302	5.4	303	8	304	3	305	8.7
306	8	307	4.3	308	7.6	309	5	310	7	311	3.2	312	4	313	8	314	3	315	7
316	8	317	3	318	6	319	5	320	7	321	3.2	322	5.4	323	7.8	324	3	325	7
326	8	327	3	328	6	329	5	330	7	331	2	332	4	333	7.6,8	334	3	335	7
336	8	337	4.3	338	6	339	5	340	7	341	3.2	342	4	343	8	344	3	345	7
346	8	347	3	348	6	349	5	350	7	351	2	352	3.2,2.5,4	353	7.6,8	354	4.3	355	7
356	8	357	3	358	6	359	6.5	360	7	361	3.2	362	5.4	363	8	364	3	365	7
366	8	367	3	368	6	369	4.5	370	7	371	3.2	372	4	373	7.8	374	3	375	7
376	7.8	377	3	378	6	379	5	380	7	381	2	382	4	383	8	384	3	385	7
386	8	387	3	388	6	389	5	390	7	396	2	397	4	398	8	399	3	400	7
391	8	392	3	393	6	394	5	395	6.7										

* Setting 3 was changed in this series and the succeeding series from 3.4.5.6.7 to 2.3.4.5.6.7 in order to make the ratio of right doors to wrong doors in the series of ten settings 10:40.

† The letter "a" indicates that the pig was aided in choosing the right door.

TABLE 4
RESULTS FOR MALE IN PROBLEM 2

T.	S. 1 7.8.9	S. 2 1.2.3.4	S. 3 3.4.5.6.7	S. 4 1.2.3 4.5.6	S. 5 4.5.6.7.8	S. 6 1.2.3	S. 7 2.3.4.5	S. 8 1.2.3.4.5 6.7.8.9	S. 9 1.2.3.4	S. 10 3.4.5 6.7.8
1	7.7.7 9.7.8	2	3.3.6	4	5	6	7	8	9	10
11	8	12	7.6	14	15	16	17	18	19	20
21	8	22	7.6	24	25	26	27	28	29	30
31	8	32	7.3.4.3.6	34	35	36	37	38	39	40
41	8	42	7.6	44	45	46	47	48	49	50
51	8	52	7.6	54	55	56	57	58	59	60
61	8	62	7.6	64	65	66	67	68	69	70
76	9.7.8	77	3.3.7.6	79	80	81	82	83	84	85
86	8	87	7.6	89	90	91	92	93	94	95
96	7.7.9.8	97	5.3.7.6	99	100	101	102	103	104	105
106	9.8	107	7.6*	109	110	111	112	113	114	115
116	8	117	6	119	120	121	122	123	124	125
126	8	127	7.6	129	130	131	132	133	134	135
136	8	137	6	139	140	141	142	143	144	145
146	8	147	6	149	150	151	152	153	154	155
156	8	157	6	159	160	161	162	163	164	165
166	8	167	6	169	170	171	172	173	174	175
176	8	177	5.4.3.7.6	179	180	181	182	183	184	185
186	7.8	187	7.3.6	189	190	191	192	193	194	195
196	8	197	7.6	199	200	201	202	203	204	205
206	8	207	7.6	209	210	211	212	213	214	215
216	7.8	217	5.4.7.6	219	220	221	222	223	224	225

226	8	227	4.3	228	7.6	229	5	230	8.7	231	3.2	232	5.4	233	8	234	3	235	7
236	8	237	3	238	6	239	5	240	8.7	241	3.2	242	5.4	243	7.6.4.3	244	3	245	7
246	8	247	3	248	6	249	5	250	8.7	251	3.2	252	5.4	253	8	254	3	255	5.4.3.7
256	8	257	3	258	5.4.3.2.6	259	5	260	8.6.5.4.7	261	2	262	4	263	8	264	3	265	6.5.4.3.7
266	8	267	2.3	268	6	269	5	270	7	271	3.2	272	5.4	273	8	274	4.2.3	275	7
276	7.8	277	3	278	3.2.5	279	6.5	280	7	281	2	282	3.4	283	8	284	3	285	7
286	7.8	287	3	288	4.2.6	289	3.4.6.5	290	7.5.7	291	2	292	2.3.5.4	293	7.6.8	294	3	295	5.6.5.8.7
296	8	297	3	298	5.6	299	5	300	7	301	2	302	5.4	303	8	304	2.3	305	7
306	8	307	3	308	5.4.3.7.6	309	5	310	5.4.8.7	311	3.2	312	5.4	313	8	314	3	315	7
316	8	317	3	318	7.6	319	4.6.2.5	320	7	321	2	322	4	323	7.6.5.4	324	3	325	4.8.7
326	8	327	3	328	5.6	329	4.3.2.1.5	330	7	331	2	332	5.4	333	8	334	3	335	6.5.7
336	8	337	3	338	6	339	5	340	4.6.5.7	341	3.2	342	5.4	343	8	344	4.3	345	7
346	7.9.8	347	3	348	5.4.5.6	349	5	350	5.5.7	351	2	352	3.4	353	8	354	3	355	5.6.8.7
356	8	357	4.3	358	7.6	359	6.5	360	7	361	3.2	362	5.4	363	8	364	3	365	7
366	8	367	4.3	368	6	369	5	370	7	371	3.2	372	3.5.4	373	8	374	3	375	7
376	8	377	4.3	378	6	379	6.5	380	7	381	3.2	382	2.4	383	8	384	3	385	7
386	8	387	4.3	388	7.6	389	5	390	7	396	3.2	397	5.4	398	8	399	2.3	400	7
391	8	392	3	393	7.6	394	5	395	7	406	3.2	407	4	408	7.6.5.8	409	2.4.3	410	8.4.3.6
401	8	402	4.3	403	7.6	404	5	405	7	416	3.2	417	5.4	418	7.6.8	419	2.3	420	5.8.6.7
411	7.8	412	3	413	6	414	4.5	415	7	431	3.2	432	5.4	433	8	434	4.3	435	8.7
421	8	422	3	423	7.5.3	424	5	425	7	441	3.2	442	5.4	443	7.6.8	444	3	445	7
426	8	427	4.3	428	2.6	429	5	430	8.7	451	3.2	452	5.3.4	453	8	454	3	455	7
436	8	437	3	438	7.6	439	3.2.1.5	440	7	461	3.2	462	5.4	463	7.6.8	464	3	465	7
446	8	447	3	448	5.7.6	449	5	450	8.7	476	3.2	477	5.4	478	7.6.8	479	3	480	6.5.7
456	8	457	3	458	6	459	5	460	7	486	3.2	487	5.4	488	7.5.8	489	3	490	5.7
466	8	467	3	468	5.4.7.6	469	5	470	7	496	2	497	3.4	498	7.8	499	3	500	7
471	8	472	3	473	6	474	5	475	7	506	3.2	507	5.4	508	7.8	509	3	510	7
481	7.8	482	3	483	6	484	4.3.2.5	485	6.5.7	516	2	517	4	518	7.8	519	3	520	7
491	8	492	3	493	6	494	3.2.5	495	6.4.7					518	7.8				
501	8	502	3	503	6	504	5	505	6.7										
511	8	512	3	513	6	514	5	515	7										

* Setting 3 was changed in this series and the succeeding ones from 3.4.5.6.7 to 2.3.4.5.6.7 in order to make the ratio of right doors to wrong doors in the series of ten settings 10:50.

TABLE 4
RESULTS FOR MALE IN PROBLEM 2

T.	S. 1 7.8.9	S. 2 1.2.3.4	S. 3 3.4.5.6.7	S. 4 1.2.3 4.5.6	S. 5 4.5.6.7.8	S. 6 1.2.3	S. 7 2.3.4.5	S. 8 1.2.3.4.5 6.7.8.9	S. 9 1.2.3.4	T.	S. 10 3.4.5 6.7.8
1	7.7.7 9.7.8	2	3	4	5	6	7	8	9	10	8.7
11	8	12	13	14	15	16	17	18	19	20	8.7
21	8	22	23	24	25	26	27	28	29	30	8.7
31	8	32	33	34	35	36	37	38	39	40	8.7
41	8	42	43	44	45	46	47	48	49	50	8.7
51	8	52	53	54	55	56	57	58	59	60	8.7
61	8	62	63	64	65	66	67	68	69	70	8.7
76	9.7.8	77	78	79	80	81	82	83	84	85	7
86	8	87	88	89	90	91	92	93	94	95	6.5.4 3.8.7
96	7.7.9.8	97	98	99	100	101	102	103	104	105	8.7
106	9.8	107	108	109	110	111	112	113	114	115	8.7
116	8	117	118	119	120	121	122	123	124	125	8.7
126	8	127	128	129	130	131	132	133	134	135	8.7
136	8	137	138	139	140	141	142	143	144	145	3.7
146	8	147	148	149	150	151	152	153	154	155	8.7
156	8	157	158	159	160	161	162	163	164	165	7
166	8	167	168	169	170	171	172	173	174	175	8.7
176	8	177	178	179	180	181	182	183	184	185	5.7
186	8	187	188	189	190	191	192	193	194	195	8.7
196	8	197	198	199	200	201	202	203	204	205	7
206	8	207	208	209	210	211	212	213	214	215	8.7
216	8	217	218	219	220	221	222	223	224	225	8.7

TABLE 5
DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

Female						Male							
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 16	1- 5	0	5				June 16	1- 5	0	5			
"	6-10	0	5	0	10	0:1	"	6-10	0	5	0	10	0:1
17	11-15	2	3				17	11-15	2	3			
"	16-20	0	5				"	16-20	1	4			
"	21-25	0	5	2	13	1:6.50	"	21-25	1	4	4	11	1:2.75
18	26-30	2	3				18	26-30	1	4			
"	31-35	1	4				"	31-35	2	3			
"	36-40	0	5				"	36-40	1	4			
"	41-45	1	4	4	16	1:4.00	"	41-45	1	4	5	15	1:3.00
19	46-50	1	4				19	46-50	1	4			
"	51-55	4	1				"	51-55	2	3			
"	56-60	2	3				"	56-60	1	4			
"	61-65	1	4	8	12	1:1.50	"	61-65	3	2	6	9	1:1.50
20	66-70	1	4				20	66-70	2	3			
"	71-75	2	3				"	71-75	2	3			
"	76-80	0	5				"	76-80	0	5			
"	81-85	2	3	5	15	1:3.00	"	81-85	2	3	6	14	1:2.33
21	86-90	0	5				21	86-90	3	2			
"	91-95	0	5				"	91-95	3	2			
"	96-100	2	3				"	96-100	1	4			
"	101-	1	4	3	17	1:5.67	"	101-	2	3	9	11	1:1.22
22	106-	0	5				22	106-	0	5			
"	111-	2	3				"	111-	2	3			
"	116-	0	5				"	116-	4	1			
"	121-	1	4	3	17	1:5.67	"	121-	1	4	7	13	1:1.86
23	126-	1	4				23	126-	2	3			
"	131-	1	4				"	131-	3	2			
"	136-	2	3				"	136-	2	3			
"	141-	1	4				"	141-	3	2			
"	146-	2	3	7	18	1:2.57	"	146-	5	0	15	10	1: .67
24	151-	1	4				24	151-	2	3			
"	156-	1	4				"	156-	4	1			
"	161-	3	2				"	161-	2	3			
"	166-	3	2				"	166-	5	0			
"	171-	4	1	12	13	1:1.08	"	171-	2	3	15	10	1: .67
25	176-	2	3				25	176-	2	3			
"	181-	3	2				"	181-	4	1			
"	186-	3	2				"	186-	1	4			
"	191-	3	2	11	9	1: .82	"	191-	3	2	10	10	1:1
26	196-	1	4				26	196-	1	4			
"	201-	3	2				"	201-	3	2			
"	206-	2	3	6	9	1:1.50	"	206-	3	2	7	8	1:1.14
27	211-	3	2				27	211-	2	3			
"	216-	2	3				"	216-	2	3			
"	221-	2	3	7	8	1:1.14	"	221-	3	2	7	8	1:1.14
28	226-	2	3				28	226-	2	3			
"	231-	3	2				"	231-	3	2			
"	236-	2	3				"	236-	4	1			
"	241-	5	0				"	241-	2	3			
"	246-	4	1				"	246-	4	1			
"	251-	5	0	21	9	1: .43	"	251-	2	3	17	13	1: .76
29	256-	2	3				29	256-	3	2			

TABLE 5—*Continued*
 DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
 FIRST CHOICES
 PROBLEM 2

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 29	261-	3	2				June 29	261-	4	1			
"	266-	4	1				"	266-	4	1			
"	271-	3	2				"	271-	2	3			
"	276-	2	3				"	276-	2	3			
"	281-	4	1	18	12	1: .67	"	281-	4	1	19	11	1: .58
30	286-	4	1				30	286-	1	4			
"	291-	2	3				"	291-	2	3			
"	296-	5	0				"	296-	4	1			
"	301-	2	3	13	7	1: .54	"	301-	3	2	10	10	1:1
July 1	306-	3	2				July 1	306-	3	2			
"	311-	4	1				"	311-	3	2			
"	316-	5	0				"	316-	3	2			
"	321-	2	3	14	6	1: .43	"	321-	2	3	11	9	1: .82
2	326-	5	0				2	326-	4	1			
"	331-	4	1				"	331-	3	2			
"	336-	4	1				"	336-	4	1			
"	341-	4	1	17	3	1: .18	"	341-	2	2	13	7	1: .54
3	346-	5	0				3	346-	2	3			
"	351-	2	3				"	351-	3	2			
"	356-	4	1				"	356-	2	3			
"	361-	3	2	14	6	1: .43	"	361-	3	2	10	10	1:1
4	366-	4	1				4	366-	4	1			
"	371-	3	2				"	371-	3	2			
"	376-	4	1				"	376-	3	2			
"	381-	5	0				"	381-	3	2			
"	386-	5	0	21	4	1: .19	"	386-	3	2	16	9	1: .56
11	391	4	1				5	391-	5	0			
"	396	5	0	9	1	1: .11	"	396-	2	3			
							"	401-	3	2			
							"	406-	1	4			
							"	411-	3	2	14	11	1: .79
							6	416-	0	5			
							"	421-	4	1	4	6	1:1.50
							7	426-	2	3			
							"	431-	1	4			
							"	436-	3	2			
							"	441-	2	3	8	12	1:1.50
							8	446-	4	1			
							"	451-	3	2			
							"	456-	5	0			
							"	461-	2	3			
							"	466-	4	1	18	7	1: .39
							9	471-	5	0			
							"	476-	1	4			
							"	481-	2	3			
							"	486-	1	4	9	11	1:1.22
							10	491-	3	2			
							"	496-	3	2			
							"	501-	4	1			
							"	506-	2	3	12	8	1: .67
							11	511-	5	0			
							"	516-	4	1	9	1	1: .11

TABLE 5
DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 16	1- 5	0	5				June 16	1- 5	0	5			
"	6-10	0	5	0	10	0:1	"	6-10	0	5	0	10	0:1
17	11-15	2	3				17	11-15	2	3			
"	16-20	0	5				"	16-20	1	4			
"	21-25	0	5	2	13	1:6.50	"	21-25	1	4	4	11	1:2.75
18	26-30	2	3				18	26-30	1	4			
"	31-35	1	4				"	31-35	2	3			
"	36-40	0	5				"	36-40	1	4			
"	41-45	1	4	4	16	1:4.00	"	41-45	1	4	5	15	1:3.00
19	46-50	1	4				19	46-50	1	4			
"	51-55	4	1				"	51-55	2	3			
"	56-60	2	3				"	56-60	1	4			
"	61-65	1	4	8	12	1:1.50	"	61-65	3	2	6	9	1:1.50
20	66-70	1	4				20	66-70	2	3			
"	71-75	2	3				"	71-75	2	3			
"	76-80	0	5				"	76-80	0	5			
"	81-85	2	3	5	15	1:3.00	"	81-85	2	3	6	14	1:2.33
21	86-90	0	5				21	86-90	3	2			
"	91-95	0	5				"	91-95	3	2			
"	96-100	2	3				"	96-100	1	4			
"	101-	1	4	3	17	1:5.67	"	101-	2	3	9	11	1:1.22
22	106-	0	5				22	106-	0	5			
"	111-	2	3				"	111-	2	3			
"	116-	0	5				"	116-	4	1			
"	121-	1	4	3	17	1:5.67	"	121-	1	4	7	13	1:1.86
23	126-	1	4				23	126-	2	3			
"	131-	1	4				"	131-	3	2			
"	136-	2	3				"	136-	2	3			
"	141-	1	4				"	141-	3	2			
"	146-	2	3	7	18	1:2.57	"	146-	5	0	15	10	1: .67
24	151-	1	4				24	151-	2	3			
"	156-	1	4				"	156-	4	1			
"	161-	3	2				"	161-	2	3			
"	166-	3	2				"	166-	5	0			
"	171-	4	1	12	13	1:1.08	"	171-	2	3	15	10	1: .67
25	176-	2	3				25	176-	2	3			
"	181-	3	2				"	181-	4	1			
"	186-	3	2				"	186-	1	4			
"	191-	3	2	11	9	1: .82	"	191-	3	2	10	10	1:1
26	196-	1	4				26	196-	1	4			
"	201-	3	2				"	201-	3	2			
"	206-	2	3	6	9	1:1.50	"	206-	3	2	7	8	1:1.14
27	211-	3	2				27	211-	2	3			
"	216-	2	3				"	216-	2	3			
"	221-	2	3	7	8	1:1.14	"	221-	3	2	7	8	1:1.14
28	226-	2	3				28	226-	2	3			
"	231-	3	2				"	231-	3	2			
"	236-	2	3				"	236-	4	1			
"	241-	5	0				"	241-	2	3			
"	246-	4	1				"	246-	4	1			
"	251-	5	0	21	9	1: .43	"	251-	2	3	17	13	1: .76
29	256-	2	3				29	256-	3	2			

TABLE 5—*Continued*
 DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
 FIRST CHOICES
 PROBLEM 2

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
June 29	261-	3	2				June 29	261-	4	1			
"	266-	4	1				"	266-	4	1			
"	271-	3	2				"	271-	2	3			
"	276-	2	3				"	276-	2	3			
"	281-	4	1	18	12	1: .67	"	281-	4	1	19	11	1: .58
30	286-	4	1				30	286-	1	4			
"	291-	2	3				"	291-	2	3			
"	296-	5	0				"	296-	4	1			
"	301-	2	3	13	7	1: .54	"	301-	3	2	10	10	1:1
July 1	306-	3	2				July 1	306-	3	2			
"	311-	4	1				"	311-	3	2			
"	316-	5	0				"	316-	3	2			
"	321-	2	3	14	6	1: .43	"	321-	2	3	11	9	1: .82
2	326-	5	0				2	326-	4	1			
"	331-	4	1				"	331-	3	2			
"	336-	4	1				"	336-	4	1			
"	341-	4	1	17	3	1: .18	"	341-	2	2	13	7	1: .54
3	346-	5	0				3	346-	2	3			
"	351-	2	3				"	351-	3	2			
"	356-	4	1				"	356-	2	3			
"	361-	3	2	14	6	1: .43	"	361-	3	2	10	10	1:1
4	366-	4	1				4	366-	4	1			
"	371-	3	2				"	371-	3	2			
"	376-	4	1				"	376-	3	2			
"	381-	5	0				"	381-	3	2			
"	386-	5	0	21	4	1: .19	"	386-	3	2	16	9	1: .56
11	391	4	1				5	391-	5	0			
"	396	5	0	9	1	1: .11	"	396-	2	3			
							"	401-	3	2			
							"	406-	1	4			
							"	411-	3	2	14	11	1: .79
							6	416-	0	5			
							"	421-	4	1	4	6	1:1.50
							7	426-	2	3			
							"	431-	1	4			
							"	436-	3	2			
							"	441-	2	3	8	12	1:1.50
							8	446-	4	1			
							"	451-	3	2			
							"	456-	5	0			
							"	461-	2	3			
							"	466-	4	1	18	7	1: .39
							9	471-	5	0			
							"	476-	1	4			
							"	481-	2	3			
							"	486-	1	4	9	11	1:1.22
							10	491-	3	2			
							"	496-	3	2			
							"	501-	4	1			
							"	506-	2	3	12	8	1: .67
							11	511-	5	0			
							"	516-	4	1	9	1	1: .11

with problem 1. It would then appear to be from four to eight times as difficult as the latter.

One important aspect of the experiment should be here considered. According to our procedure, one of the pigs led and the other followed in a series of trials. It was therefore possible that the follower might be aided in its choice either by watching its companion or by the odor of the box in which the animal fed. There can be no doubt of the tendency of the pigs both to watch one another and to be influenced by the odor of the boxes, but that the solution of the problems did not depend upon either of these factors, although the number of trials necessary to solution may have been modified thereby, is proved by the fact that both subjects made ninety per cent of correct choices when leading.

PROBLEM 3

All of the data in connection with this problem are to be found in tables 6, 7, and 8. The problem is definable as alternately the first mechanism at the right and the first at the left. At the beginning of work on this problem, the animals were given their trials alternately as in the preceding problems, but a strong tendency to follow manifested itself, and on the second day the trials were given by pairs. That is, each individual was allowed to choose in succession the first door at its right and the first door at its left, and was then required to wait while its companion responded to the same pair of settings. Thus, following was rendered impossible.

The tendency to choose the second door from the left naturally manifested itself in the early work on this problem, but it was soon destroyed by training, and the course of experimentation proceeded smoothly to the perfect solution of the problem.

It is to be noted that from the first, ten trials constituted a series. Because of the familiarity with the general experimental situation which the animals had acquired and the experience of the experimenters in the control of hunger and punishment, it was easier to obtain reactions to ten successive trials at this time in the investigation than to five early in the work, with problems 1 or 2.

The female succeeded in solving problem 3 as the result of 420 trials; the male, as the result of 470.

For this problem as for problem 2, the expectation prior to experience is one correct first choice to four incorrect first choices. The male in his first series exhibited exactly this ratio, whereas the female gave a ratio of 1 to 1. Her success, however, was undoubtedly due to following, for in immediately subsequent trials when following was rendered impossible by the giving of the trials by pairs, she did very poorly. The daily ratios for each individual, as presented in table 8, are of interest, but they are by no means as important as are the detailed data of tables 6 and 7.

As might have been expected, after the previously acquired tendency to select the first mechanism at the left had been overcome, the pigs shortly exhibited the tendency to select the end boxes, and they then had to overcome the difficulty of selecting the right end. It is quite possible that this task was rendered easier by the rhythm which resulted from the giving of trials by pairs, but it was perfectly evident from control experiments that the animals could choose correctly even if given their trials in rapid succession, without the irregularity due to alternate experimenting with the two individuals.

Since it seemed possible that the animals might have learned the proper settings and be responding to definite situations rather than to the relation of the right box to the other members of the group, a control experiment was made by the presentation of a new series of settings. At the bottoms of tables 6 and 7 appear the results of these control observations.

The female had solved problem 3 on the completion of trial 420 (see tables 6 and 8), and the male on the completion of trial 470 (see tables 7 and 8). The next series of ten trials for each was preliminary to the control experiments and served also as a demonstration series to certain other observers. Following this demonstration in which both pigs reacted fairly well, the series of settings indicated in tables 6 and 7 was presented. Both individuals were somewhat disturbed by the change, her record being seven correct choices out of ten, and his nine out of ten. Later in the day another series of ten trials, according to the original settings, was given with the result that the female made three incorrect first choices in ten and the male two. Still later, the control settings were again presented. This time she chose correctly eight times in ten and he only five times.

TABLE 6
RESULTS FOR FEMALE IN PROBLEM 3

S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	S. 10
5.6.7	5.6.7	1.2.3 4.5.6	1.2.3 4.5.6	4.5.6.7.8	4.5.6.7.8	2.3.4.5	2.3.4.5	3.4.5.6 7.8.9	3.4.5.6 7.8.9
T.	T.	T.	T.	T.	T.	T.	T.	T.	T.
2	6.5.7	3	4	5	6	7	8	9	10
12	6.7	13	14	15	16	17	18	19	20
22	5.7	23	24	25	26	27	28	29	30
32	5.7	33	34	35	36	37	38	39	40
42	5.6.7	43	44	45	46	47	48	49	50
52	5.7	53	54	55	56	57	58	59	60
62	7	63	64	65	66	67	68	69	70
72	7	73	74	75	76	77	78	79	80
82	5.7	83	84	85	86	87	88	89	90
92	6.7	93	94	95	96	97	98	99	100
102	7	103	104	105	106	107	108	109	110
112	6.7	113	114	115	116	117	118	119	120
122	7	123	124	125	126	127	128	129	130
132	7	133	134	135	136	137	138	139	140
142	7	143	144	145	146	147	148	149	150
152	7	153	154	155	156	157	158	159	160
162	6.7	163	164	165	166	167	168	169	170
172	7	173	174	175	176	177	178	179	180
182	7	183	184	185	186	187	188	189	190
192	7	193	194	195	196	197	198	199	200
202	7	203	204	205	206	207	208	209	210

211	6.7.5	212	7	213	6.1	214	6	215	7.8.5 7.8.4	216	8	217	3.5.2	218	5	219	9.3	220	9
221	5	222	7	223	2.6.1	224	6	225	4	226	8	227	2	228	5	229	9.3	230	9
231	5	232	7	233	2.1	234	6	235	4	236	8	237	2	238	3.5	239	3	240	9
241	5	242	5.7	243	1	244	6	245	4	246	8	247	2	248	5	249	3	250	9
251	5	252	7	253	1	254	6	255	4	256	8	257	2	258	3.5	259	3	260	9
261	5	262	7	263	1	264	6	265	4	266	8	267	2	268	5	269	9.3	270	9
271	5	272	7	273	1	274	3.6	275	4	276	8	277	2	278	4.5	279	3	280	9
281	5	282	5.7	283	1	284	3.6	285	8.4	286	8	287	2	288	5	289	9.3	290	9
291	5	292	7	293	1	294	3.6	295	8.4	296	8	297	2	298	5	299	9.3	300	9
301	5	302	7	303	1	304	6	305	8.4	306	7.8	307	2	308	3.5	309	3	310	9
311	5	312	7	313	1	314	6	315	8.4	316	8	317	2	318	4.5	319	3	320	9
321	7.5	322	7	323	1	324	5.6	325	4	326	8	327	2	328	4.5	329	9.3	330	9
331	5	332	7	333	1	334	6	335	4	336	8	337	2	338	3.5	339	3	340	9
341	6.5	342	7	343	2.1	344	6	345	4	346	8	347	2	348	5	349	9.3	350	8.9
351	5	352	7	353	1	354	3.6	355	4	356	8	357	2	358	5	359	3	360	9
361	5	362	7	363	1	364	6	365	4	366	8	367	2	368	5	369	9.3	370	9
371	6.5	372	7	373	1	374	6	375	4	376	8	377	2	378	5	379	3	380	9
381	5	382	7	383	1	384	4.6	385	4	386	8	387	2	388	4.5	389	9.3	390	9
391	5	392	7	393	1	394	6	395	8.4	396	8	397	2	398	5	399	3	400	9
401	6.5	402	7	403	2.1	404	6	405	4	406	8	407	2	408	5	409	3	410	9
411	5	412	7	413	1	414	6	415	4	416	8	417	2	418	5	419	3	420	9
421	5	422	6.7	423	1	424	6	425	5.4	426	7.8	427	2	428	5	429	3	430	9
					4.5.6 7.8.9		4.5.6 7.8.9		1.2.3.4.5		1.2.3.4.5		2.3.4 5.6.7		2.3.4 5.6.7		3.4.5 6.7.8		3.4.5 6.7.8
1	4.6.3	2	6	3	4	4	9	5	2.1	6	5	7	7.2	8	7	9	3	10	8
431	5	432	7	433	1	434	6	435	6.8.4	436	7.8	437	2	438	5	439	9.3	440	9
					4.5.6 7.8.9		4.5.6 7.8.9		1.2.3.4.5		1.2.3.4.5		2.3.4 5.6.7		2.3.4 5.6.7		3.4.5 6.7.8		3.4.5 6.7.8
11	3	12	6	13	5.9.4	14	9	15	1	16	5	17	2	18	7	19	5.3	20	8

TABLE 7
RESULTS FOR MALE IN PROBLEM 3

T.	S. 1 5.6.7	T.	S. 2 5.6.7	T.	S. 3 1.2.3 4.5.6	T.	S. 4 1.2.3 4.5.6	T.	S. 5 4.5.6.7.8	T.	S. 6 4.5.6.7.8	T.	S. 7 2.3.4.5	T.	S. 8 2.3.4.5	T.	S. 9 3.4.5.6 7.8.9	T.	S. 10 3.4.5.6 7.8.9
1	6.5	2	7	3	5.4.3.2 6.5.3 4.2.5 3.5.1	4	1.3.5.5.6	5	7.6.4	6	7.6.8	7	5.4.3.2	8	5	9	8.7.6.8 7.7.3	10	5.7.8.9
11	6.5	12	6.7	13	2.1 5.4.3.2	14	5.4.3.6	15	7.8.5.4	16	8	17	2	18	5	19	3	20	8.7.9
21	6.5	22	7	23	5.4.3.2	24	3.2.6	25	8.7.4	26	6.8	27	3.5.2	28	3.5	29	8.7.9 8.6.3	30	8.7.5 4.8.9
31	7.6.5	32	7	33	6.5.1 5.4.3.2.1	34	5.4.3.5.6	35	8.6.8.7.4	36	8	37	3.2	38	2.5	39	7.6.9 8.7.3	40	8.9
41	7.6.5	42	7	43	5.3.2.4.1	44	5.2.1.4.6	45	7.8.6.7.4	46	8	47	5.2	48	2.4.3.2.5	49	9.7.6.8 9.8.5.4 8.7.4.6	50	9
51	6.7.5	52	5.7	53	3.1	54	2.5.6	55	8.6.4	56	8	57	3.5.2	58	4.5	59	8.3	60	6.3.8.7.9
61	7.5	62	6.5.7	63	3.1	64	2.6	65	7.8.7.4	66	8	67	4.2	68	3.5	69	9.3	70	7.9
71	7.5	72	6.7	73	5.1	74	2.1.4.2.6	75	4	76	6.8	77	5.2	78	3.5	79	9.3	80	9
81	7.5	82	6.7	83	1	84	3.4.6	85	8.7.8.4	86	7.8	87	2	88	3.5	89	9.3	90	9
91	6.5	92	7	93	3.1	94	6	95	8.4	96	8	97	3.5.2	98	5	99	9.4.8.9 4.8.9.3	100	6.9
101	5	102	7	103	4.1	104	6	105	8.5.8 6.8.4	106	5.8	107	3.2	108	5	109	9.3	110	8.9
111	7.5	112	6.7	113	3.1	114	4.6	115	8.4	116	8	117	3.5.2	118	5	119	9.3	120	9
121	6.7.5	122	6.7	123	3.1	124	5.1.6	125	6.4	126	8	127	3.2	128	5	129	9.3	130	9
131	7.5	132	7	133	3.1	134	6	135	8.4	136	7.8	137	2	138	5	139	8.9.3	140	9
141	6.5	142	7	143	1	144	3.4.3.2.6	145	4	146	8	147	2	148	3.5	149	4.9.7.4 9.5.8.3	150	9
151	7.5	152	7	153	2.6.4.2.1 2.6.4.6	154	2.5.3.6	155	8.4	156	8	157	3.2	158	3.5	159	9.3	160	9
161	6.5	162	7	163	2.6.1	164	3.2.6	165	8.5.4	166	8	167	3.5.2	168	5	169	9.3	170	9
171	7.5	172	7	173	1	174	3.5.3.2.6	175	4	176	7.8	177	2	178	3.5	179	3	180	9
181	5	182	7	183	2.6.1	184	3.6	185	8.4	186	8	187	2	188	3.5	189	3	190	9
191	5	192	7	193	1	194	2.6	195	7.5.4	196	8	197	2	198	3.5	199	9.3	200	9
201	7.5	202	7	203	3.1	204	6	205	4	206	8	207	2	208	2.5	209	9.3	210	9

211	7.5	212	7	213	2.6.1	214	3.6	215	8.6.4	216	8	217	2	218	5	219	9.3	220	9
221	5	222	7	223	2.6.1	224	2.1.6	225	4	226	8	227	3.2	228	2.5	229	9.3	230	9
231	6.5	232	7	233	1	234	2.2.6	235	7.4	236	8	237	2	238	3.5	239	9.3	240	9
241	7.5	242	7	243	2.6.4.1	244	5.2.6	245	4	246	8	247	3.5.2	248	3.5	249	3	250	9
251	6.5	252	7	253	1	254	3.6	255	8.4	256	8	257	2	258	3.5	259	3	260	9
261	7.5	262	7	263	3.5.1	264	6	265	4	266	8	267	2	268	4.5	269	9.3	270	9
271	5	272	7	273	1	274	6	275	4	276	8	277	2	278	3.5	279	3	280	9
281	5	282	7	283	1	284	3.6	285	4	286	8	287	2	288	5	289	3	290	9
291	7.5	292	7	293	2.3.1	294	6	295	4	296	8	297	2	298	4.5	299	3	300	9
301	7.5	302	7	303	1	304	6	305	4	306	8	307	2	308	5	309	9.3	310	9
311	6.5	312	7	313	2.5.6.1	314	6	315	4	316	8	317	2	318	5	319	9.3	320	9
321	5	322	7	323	1	324	6	325	8.4	326	8	327	2	328	5	329	9.3	330	9
331	5	332	7	333	1	334	5.4.6	335	4	336	8	337	2	338	5	339	3	340	9
341	7.5	342	7	343	2.6.1	344	6	345	4	346	8	347	2	348	5	349	3	350	9
351	7.5	352	7	353	1	354	4.6	355	4	356	8	357	3.5.2	358	5	359	3	360	9
361	6.5	362	5.7	363	1	364	2.6	365	4	366	8	367	2	368	3.5	369	3	370	9
371	5	372	7	373	1	374	6	375	4	376	8	377	2	378	5	379	9.3	380	9
381	7.5	382	7	383	2.1	384	6	385	4	386	8	387	2	388	5	389	3	389	9
391	5	392	7	393	2.4.6.1	394	6	395	4	396	8	397	2	398	5	399	3	400	9
401	5	402	7	403	1	404	4.3.6	405	4	406	8	407	2	408	3.5	409	3	410	9
411	5	412	7	413	1	414	4.2.6	415	4	416	8	417	2	418	5	419	3	420	8.3.9
421	5	422	7	423	2.6.1	424	6	425	8.4	426	8	427	2	428	5	429	3	430	9
431	5	432	7	433	6.1	434	6	435	4	436	8	437	2	438	4.5	439	3	440	9
441	5	442	6.7	443	1	444	6	445	5.8.4	446	8	447	2	448	5	449	3	450	9
451	5	452	7	453	1	454	6	455	7.4	456	8	457	3.5.2	458	4.5	459	9.3	460	9
461	5	462	7	463	1	464	6	465	4	466	8	467	2	468	5	469	3	470	9
471	7.5	472	7	473	1	474	6	475	8.4	476	8	477	2	478	5	479	3	480	9
	3.4.5.6		3.4.5.6		4.5.6 7.8.9		4.5.6 7.8.9		1.2.3.4.5		1.2.3.4.5		2.3.4 5.6.7		2.3.4 5.6.7		3.4.5 6.7.8		3.4.5 6.7.8
1	3	2	6	3	4	4	9	5	1	6	5	7	2	8	7	9	4.3	10	8
481	6.5	482	7	483	2.6.1	484	6	485	4	486	8	487	2	488	5	489	3	490	9
	3.4.5.6		3.4.5.6		4.5.6 7.8.9		4.5.6 7.8.9		1.2.3.4.5		1.2.3.4.5		2.3.4 5.6.7		2.3.4 5.6.7		3.4.5 6.7.8		3.4.5 6.7.8
11	5.3	12	6	13	5.4	14	9	15	2.5.1	16	5	17	5.2	18	7	19	7.3	20	8

TABLE 8
DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES
PROBLEM 3

Female							Male						
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
July 11	1-	5	5	5	5	1:1	July 11	1-	2	8	2	8	1:4.00
12	11-	0	10				12	11-	4	6			
"	21-	3	7	3	17	1:5.66	"	21-	1	9	5	15	1:3.00
13	31-	1	9				13	31-	2	8			
"	41-	3	7	4	16	1:4.00	"	41-	3	7	5	15	1:3.00
14	51-	4	6				14	51-	1	9			
"	61-	3	7	7	13	1:1.86	"	61-	1	9	2	18	1:9.00
15	71-	4	6				15	71-	2	8			
"	81-	2	8	6	14	1:2.33	"	81-	3	7	5	15	1:3.00
16	91-	2	8				16	91-	4	6			
"	101-	4	6	6	14	1:2.33	"	101-	4	6	8	12	1:1.50
17	111-	3	7				17	111-	3	7			
"	121-	3	7	6	14	1:2.33	"	121-	3	7	6	14	1:2.33
18	131-	5	5				18	131-	5	5			
"	141-	6	4				"	141-	6	4			
"	151-	5	5	16	14	1: .88	"	151-	3	7	14	16	1:1.14
19	161-	4	6				19	161-	4	6			
"	171-	6	4	10	10	1:1	"	171-	6	4	10	10	1:1
20	181-	4	6				20	181-	6	4			
"	191-	3	7	7	13	1:1.86	"	191-	6	4	12		1:
21	201-	6	4				21	201-	6	4			
"	211-	5	5	11	9	1: .82	"	211-	5	5	11		
22	221-	8	2				22	221-	5	5			
"	231-	7	3	15	5	1: .33	"	231-	5	5	10	10	1
23	241-	9	1				23	241-	5	5			
"	251-	8	2	17	3	1: .18	"	251-	6	4	11	9	
24	261-	9	1				24	261-	6	4			
"	271-	8	2				"	271-	9	1			
"	281-	7	3	24	6	1: .25	"	281-	9	1	24	6	
25	291-	7	3				25	291-	7	3			
"	301-	7	3				"	301-	8	2			
"	311-	9	1	23	7	1: .30	"	311-	7	3	22	8	
26	321-	6	4				26	321-	8	2			
"	331-	9	1	15	5	1: .33	"	331-	9	1	17	3	1
27	341-	6	4				27	341-	8	2			
"	351-	9	1	15	5	1: .33	"	351-	7	3	15	5	1
28	361-	9	1				28	361-	6	4			
"	371-	9	1	18	2	1: .11	"	371-	9	1	15	5	1:
"	381-	7	3				29	381-	8	2			
"	391-	9	1	16	4	1: .25	"	391-	9	1	17	3	1:
"	401-	8	2				30	401-	8	2			
"	411-	10	0	18	2	1: .11	"	411-	8	2	16	4	1:
							31	421-	8	2			
							Aug. 1	431-	8	2			
							"	441-	8	2			
							"	451-	6	4			
							"	461-	10	0	24	6	1:
421		7	3	7	3	1: .43	2	471	8	2	8	2	1:
1-10		7	3	7	3	1: .43	3	1-10	9	1	9	1	1:
431		7	3	7	3	1: .43	3	481	8	2	8	2	1:
				8	2	1: .25	3	11-20	5	5	5	5	1:

Although these figures are far from conclusive, we are convinced from the behavior of the animals that neither was choosing by familiarity with the particular settings. She, as has been pointed out, did as well with the control series as with the regular series, and he did even better in the first control series than in the regular series, while showing extreme confusion in the second control series. This was doubtless due to insufficient hunger and the distracting influence of a mistake in the first trial of the series. His carelessness throughout the last control series was conspicuous.

Comparison of the results for problems 2 and 3 indicate that for the female problem 3 was somewhat the more difficult, whereas for the male, problem 2 required a larger number of trials. We are by no means convinced by this comparison that the problems have not been used in the order of increasing difficulty, for we consider the female subject a much more reliable individual than the male, and we suspect that his greater facility in the solution of the third problem was due in part, at least, to the experience of the experimenters in dealing with his temperamental and other peculiarities.

PROBLEM 4

The data to be considered in this connection appear in tables 9, 10 and 11. The correct mechanism is definable simply as the middle one, and the expectation prior to experience is one correct to four incorrect first choices, since the total number of doors open in the series of ten settings is fifty. As is shown in table 11, precisely this ratio resulted from the first day's experimentation in the case of each individual.

Ten trials per series were given regularly throughout the work on this problem.

Unlike the preceding problems, this one proved insoluble. Consequently, the detailed results as they appear in tables 9 and 10 are especially important, since from them may be read the reactive tendencies and their relations to one another. It is, of course, easy to understand why the ratio of correct to incorrect first choices should change steadily in the direction of the solution of the problem, for each subject gradually learned to react appropriately to certain of the settings while failing to acquire the ability to react to the relatively middle ones.

TABLE 9
RESULTS FOR FEMALE IN PROBLEM 4

S. 1	T.	S. 2	T.	S. 3	T.	S. 4	T.	S. 5	T.	S. 6	T.	S. 7	T.	S. 8	T.	S. 9	T.	S. 10
2.3.4	2	5.6.7.8.9	3	1.7.4	4	7.9.7.8	5	4.8.4.8.6	6	1.9.5	7	1.3.2	8	2.5.6 2.6.3 6.5.4	9	3.9.5 3.4.5.6 7.8.9	10	6.8.7
2.3	12	5.8.9 6.8.7	13	1.7.3.6 1.5.3.7 2.1.6.5 2.7.5.6	14	9.8	15	6	16	5	17	2	18	6.3.5.6 2.3.6.5 3.6.2.4	19	3.8.9.6	20	7
2.4.4.3	22	5.8.7	23	1.6.4	24	7.8	25	6	26	1.8.5	27	1.2	28	5.4	29	5.8.3 9.7.6 6	30	7
2.4.3	32	7	33	3.4	34	9.7.8	35	5.6	36	5	37	3.2	38	3.6.4	39	6	40	8.7
2.3	42	5.7	43	6.3.4	44	8	45	6	46	2.7.9.1 4.2.9.5	47	3.2	48	5.4	49	6	50	6.8.7
2.4.3	52	7	53	5.1.6.7 5.6.2.4 5.3.6.1	54	7.8	55	6	56	1.9.7.3.5	57	2	58	6.3.5 7.2.4	59	3.6	60	7
2.4.3	62	8.6.5.7	63	2.7.5.2 6.1.4	64	8	65	5.6	66	6.7.9 3.2.5	67	2	68	6.3.4	69	3.6	70	6.8.6.8.7
4.2.3	72	8.5.9.6.7	73	5.6.2.5 1.7.4	74	9.8	75	5.6	76	5	77	2	78	4	79	7.8.3.7 8.3.5.6	80	7
3	82	7	83	2.6.4	84	8	85	5.4.6	86	2.6.4.5	87	2	88	6.3.4	89	7.8.5.6 7.6	90	6.7
4.2.3	92	6.8.6.7	93	2.6.5.4	94	8	95	5.7.5.6	96	6.7.2.9 2.4.6.5	97	2	98	6.4	99	7.6	100	8.6.8.7
4.2.3	102	5.6.7	103	5.4	104	7.8	105	6	106	2.4.6.7.5	107	2	108	6.5.4	109	8.7.6	110	8.7
2.3	112	8.6.7	113	2.5.4	114	8	115	5.6	116	2.4.5	117	2	118	6.6.5.4	119	6	120	6.7
3	122	8.6.7	123	2.5.6.4	124	7.8	125	5.6	126	1.4.3.5	127	2	128	4	129	3.6	130	7
3	132	7	133	1.3.4	134	8	135	6	136	5	137	2	138	6.5.6.4	139	6	140	8.7
4.3	142	7	143	2.5.6.5 6.1.4	144	8	145	7.6	146	6.5	147	2	148	6.4	149	8.9.6	150	8.7
3	152	7	153	2.5.6.7 5.2.4	154	8	155	7.6	156	7.8.4 8.8.5	157	2	158	5.3.6.4	159	8.7.3.7 8.4.6	160	6.8.7
2.4.3	162	7	163	4	164	8	165	6	166	2.5	167	2	168	3.4	169	8.7.4.6	170	7
3	172	5.7	173	2.4	174	8	175	5.6	176	2.3.5	177	2	178	3.4	179	6	180	7
181	182	8.7	183	2.4	184	7.8	185	5.7.6	186	2.4.7.8.5	187	2	188	4	189	8.7.6	190	8.7
191	192	6.7	193	2.4	194	7.8	195	5.6	196	2.4.8.4.5	197	2	198	6.3.4	199	6	200	7
201	202	7	203	5.4	204	7.8	205	5.6	206	2.4.5	207	2	208	4	209	6	210	6.8.7
211	212	6.8.5.7	213	5.4	214	7.8	215	6	216	4.6.2.5	217	2	218	6.4	219	8.7.6	220	6.7
221	222	6.8.5.7	223	5.6.4	224	8	225	5.6	226	5	227	2	228	6.4	229	8.7.5.6	230	7

231	3	232	7	233	4	234	8	235	6	236	2.1.5	237	2	238	6.5.3.4	239	6	240	7
241	3	242	6.8.7	243	4	244	8	245	5.6	246	2.4.6	247	3.2	248	6.5.4	249	8.9.5.7.6	250	8.7
251	4.3	252	7	253	4	254	8	255	5.6	256	8.9.5	257	3.2	258	3.4	259	4.5.6	260	7
261	3	262	7	263	4	264	8	265	7.6	266	2.4.5	267	2	268	5.4	269	6	270	7
271	4.3	272	8.7	273	6.5.7.4	274	8	275	6	276	4.6.2.6	277	3.2	278	5.4	279	6	280	7
281	4.3	282	7	283	6.4	284	8	285	6	286	4.3.2.4	287	2	288	5.4	289	6	290	7
291	4.3	292	8.6.7	293	5.6.4	294	8	295	6	296	6.7.5	297	3.2	298	6.5.4	299	7.6	300	7
301	3	302	6.7	303	4	304	7.8	305	6	306	4.6.5	307	3.2	308	6.5.4	309	7.8.7.6	310	8.7
311	3	312	6.5.7	313	5.6.4	314	8	315	5.6	316	4.7.6.2	317	3.2	318	4	319	4.7.6	320	8.6.7
321	4.3	322	6.8.7	323	7.6.2.4	324	8	325	7.5.6	326	4.3.5	327	2	328	6.4	329	8.6	330	7
331	3	332	6.7	333	4	334	8	335	4.5.6	336	2.4.3.5	337	2	338	6.4	339	6	340	7
341	4.3	342	7	343	2.4	344	8	345	4.6	346	2.4.5	347	2	348	6.4	349	8.4.6	350	7
351	3	352	6.6.8.7	353	5.6.4	354	8	355	5.6	356	7.3.5	357	2	358	4	359	8.7.6	360	8.7
361	4.3	362	6.8.5.7	363	4	364	7.8	365	5.6	366	5	367	2	368	6.5.4	369	8.7.5.6	370	7
371	4.3	372	7	373	5.6.4	374	8	375	7.6	376	7.8.6.4.5	377	2	378	6.4	379	7.6	380	7
381	4.3	382	8.7	383	6.5.4	384	8	385	6	386	5	387	2	388	6.5.4	389	7.6	390	7
391	3	392	8.7	393	5.6.4	394	8	395	5.6	396	6.5	397	2	398	5.4	399	6	400	7
401	3	402	7	403	5.6.4	404	8	405	4.5.6	406	2.4.5	407	2	408	4	409	6	410	7
411	3	412	7	413	2.4	414	8	415	5.6	416	2.4.5	417	2	418	3.4	419	6	420	7
421	3	422	6.7	423	4	424	8	425	6	426	2.5	427	2	428	2.4	429	6	430	7
431	3	432	6.7	433	2.6.5.4	434	8	435	4.5.6	436	2.4.5	437	2	438	3.4	439	6	440	8.7
441	3	442	8.7	443	2.4	444	8	445	6	446	6.6.2	447	2	448	6.4	449	6	450	7
451	4.3	452	8.7	453	2.4	454	8	455	7.6	456	4.6.5	457	3.2	458	6.4	459	8.7.6	460	7
461	3	462	7	463	2.4	464	8	465	6	466	2.5	467	2	468	6.4	469	6	470	7
471	3	472	7	473	3.4	474	8	475	7.6	476	2.4.5	477	2	478	4	479	4.6	480	7
481	3	482	7	483	3.5.4	484	8	485	6	486	4.5	487	2	488	4	489	4.5.4.5.6	490	8.7
491	3	492	7	493	5.4	494	8	495	6	496	2.5	497	2	498	4	499	6	500	8.7
501	3	502	8.7	503	6.5.4	504	8	505	7.5.6	506	2.5	507	2	508	6.4	509	7.6	510	7
511	3	512	8.7	513	2.5.6.4	514	8	515	7.6	516	2.5	517	2	518	5.6.4	519	8.7.6	520	7
521	3	522	6.7	523	4	524	8	525	7.6	526	2.4.5	527	2	528	6.5.4	529	6	530	8.7
531	3	532	7	533	4	534	8	535	7.6	536	5	537	2	538	6.5.4	539	7.6	540	7
541	4.3	542	8.7	543	4	544	8	545	7.6	546	2.5	547	2	548	6.5.4	549	8.6	550	7
551	3	552	8.7	553	6.4	554	8	555	7.6	556	7.4.5	557	2	558	5.4	559	6	560	7
561	4.3	562	8.7	563	4	564	8	565	6	566	5	567	2	568	6.5.4	569	7.6	570	7
571	3	572	8.7	573	4	574	8	575	7.6	576	4.5	577	2	578	4	579	7.6	580	7
581	3	582	7	583	6.5.4	584	8	585	5.6	586	6.5	587	2	588	6.5.4	589	7.6	590	7
591	3	592	6.7	593	4	594	7.7.8	595	5.6	596	2.3.4.5	597	2	598	3.5.4	599	5.6	600	7

TABLE 9—Continued
RESULTS FOR FEMALE IN PROBLEM 4

S.1 2,3,4	T. 2	S.2 5,6,7,8,9	T. 3	S.3 3,4,5,6,7	T. 4	S.4 7,8,9	T. 5	S.5 4,5,6,7,8	T. 6	S.6 1,2,3,4,5	T. 7	S.7 1,2,3	T. 8	S.8 2,3,4,5,6	T. 9	S.9 4,5,6	T. 10	S.10 6,7,8	T. 11
	12	7	13	4.5	14	7.8	15	6	16	2,4,3 (5,4,2 5,4,3	17	2	18	4	19	5	20	7	
	22	8,7	23	4.5	24	8	25	6	26	2,4,5,2 (4,5,4,3 2,4,5,4	27	2	28	4	29	5	30	7	
	32	7	33	7,6,5	34	8	35	7,6	36	4,2,4,3 (2,4,3 4,5,4,2,3	37	2	38	5,4	39	5	40	7	
	42	7	43	6,5	44	7,8	45	7,6	46	2,4,3	47	2	48	4	49	5	50	7	
	52	6,7	53	4,5	54	8	55	6	56	4,5,4,2,3	57	2	58	5,6,4	59	6,5	60	7	
	62	7	63	4,5	64	8	65	6	66	2,4,3	67	2	68	5,4	69	6,5	70	8,7	
	72	7	73	4,5	74	7,8	75	6	76	4,2,3	77	2	78	4	79	6,5	80	8,6,7	
	82	7	83	4,6,5	84	7,8	85	5,4,6	86	2,4,3	87	2	88	4	89	6,5	90	7	
	92	7	93	6,5	94	8	95	4,6	96	2,3	97	2	98	4	99	5	100	7	
	102	7	103	6,5	104	8	105	6	106	2,4,5,2,3	107	3,2	108	4	109	6,5	110	8,7	
	112	7	113	6,5	114	8	115	6	116	2,4,3	117	2	118	5,4	119	5	120	8,7	
	122	8,7	123	5	124	8	125	6	126	4,3	127	2	128	4	129	6,5	130	7	
	132	7	133	6,5	134	7,8	135	6	136	4,3	137	2	138	6,5,4	139	6,5	140	7	
	142	7	143	6,5	144	8	145	6	146	2,4,5,3	147	2	148	5,4	149	6,5	150	7	
	152	7	153	5	154	8	155	7,6	156	4,3	157	2	158	5,6,4	159	6,5	160	8,7	
	162	7	163	6,5	164	8	165	6	166	4,3	167	3,2	168	5,4	169	5	170	8,7	
	172	7	173	5	174	8	175	6	176	2,3	177	2	178	4	179	5	180	7	
	182	7	183	6,5	184	8	185	6	186	2,4,3	187	2	188	4	189	4,6,5	190	8,7	
	192	6,5,8,7	193	4,3,5	194	8	195	6	196	3	197	2	198	6,3,4	199	5	200	7	
	202	8,7	203	6,4,5	204	8	205	6	206	4,3	207	2	208	5,4	209	6,5	210	7	

TABLE 10
RESULTS FOR MALE IN PROBLEM 4

S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	S. 10
T. 2.3.4	T. 5.6.7.8.9	T. 1.2.3.4 5.6.7	T. 7.8.9	T. 4.5.6.7.8	T. 1.2.3.4.5 6.7.8.9	T. 1.2.3	T. 2.3.4.5.6	T. 3.4.5.6 7.8.9	T. 6.7.8
1 2.4.2.4.3	2 5.9.5.9.7	3 1.7.1.7.4	4 {7.9.7 9.7.8	5 {4.8.7.8 4.8.4.7 4.8.5.7 4.8.5.8 6	6 {1.9.2 8.6.5	7 1.3.2	8 2.6.5.4	9 {3.9.5.8 3.9.6	10 7
2.4.3	12 5.8.6.9.7	{1.6.5.7 2.3.5 1.7.4	14 8	15 5.7.4.6	16 {2.6.3.9 1.4.5	17 2	18 3.2.5.6.4	19 8.3.5.9.6	20 7
2.3	22 6.5.8.9.7	24 8	25 8	25 4.5.7.4.6	26 {1.3.2.4 1.9.7.5	27 2	28 6.5.4	29 5.3.8.6	30 6.8.6.7
2.4.3	32 5.8.9.6.7	34 8	35 8	35 5.7.6	36 {2.8.6 4.2.5	37 3.1.2	38 3.5.6.5.4	39 3.6	40 7
2.3	42 6.5.8.9.7	44 8	45 8	45 6	46 2.5	47 1.2	{3.2.6.5 3.6.5 6.2.4	49 8.3.8.7.6	50 7
2.3	52 7	54 8	55 8	55 5.6	{2.6.1.4 3.2.5	57 2	58 2.5.4	59 3.6	60 7
3	62 8.6.5.7	64 8	65 8	65 7.4.5.6	66 3.9.5	67 2	68 3.4	{7.9.8 5.9.8 7.3.6	70 8.7
3	72 8.6.5.5.7	74 8	75 8	75 5.4.5.6	{2.3.7.7 8.2.3.8 4.6.7.2	77 3.2	78 6.4	79 8.7.9.6	80 7
4.3	82 9.7	84 8	85 8	85 6	86 5	87 2	88 6.5.3.4	89 7.3.6	90 8.7
1 2.4.3	92 6.8.6.5.7	94 8	95 8	95 7.5.4.6	96 5	97 2	98 5.6.4	99 6	100 7
1 2.4.3	102 7	104 7.8	105 7.8	105 5.6	106 7.3.5	107 2	108 4	109 5.8.7.6	110 7
1 3	112 8.6.5.7	114 8	115 8	115 6	116 7.5	117 2	118 5.6.4	119 8.6	120 8.7
1 3	122 6.5.8.6.7	124 8	125 8	125 5.7.4.7.6	126 2.3.6.1.5	127 2	128 3.4	129 8.6	130 8.7
31 3	132 5.7	134 8	135 8	135 6	136 2.6.7.5	137 2	138 6.5.4	139 8.7.6	140 8.7
41 4.2.3	142 7	144 8	145 8	145 6	146 6.5	147 2	148 6.4	149 8.7.6	150 8.7
51 2.4.3	152 6.7	154 8	155 8	155 5.6	156 7.2.5	157 2	158 6.2.6.4	159 6	160 8.7
61 2.4.3	162 8.6.7	164 8	165 8	165 6	166 2.4.2.6.5	167 2	168 6.5.4	169 7.6	170 8.7
71 3	172 8.7	174 8	175 8	175 5.7.5.6	176 5	177 2	178 5.6.5.4	179 6	180 8.7
81 3	182 8.6.5.7	184 8	185 8	185 6	186 2.5	187 2	188 2.4	189 3.7.5.6	190 8.7
91 3	192 7	194 8	195 8	195 5.6	196 6.2.4.5	197 2	198 5.4	199 6	200 7
10 3	202 7	204 8	205 8	205 5.6	206 2.5	207 2	208 4	209 8.7.6	210 8.6.7
11 2.4.3	312 6.7	214 8	215 8	215 6	216 2.6.4.3.5	217 2	218 6.4	219 8.7.6	220 8.7
221 3	222 6.5.6.8.7	224 8	225 8	225 6	226 2.3.4.5	227 2	228 3.4	229 7.6	230 8.7

TABLE 10

RESULTS FOR MALE IN PROBLEM 4

T.	S. 1		S. 2		S. 3		S. 4		S. 5		S. 6		S. 7		S. 8		S. 9		S. 10	
	T.	2.3.4	T.	5.6.7.8.9	T.	1.2.3.4	T.	7.8.9	T.	4.5.6.7.8	T.	1.2.3.4.5	T.	1.2.3	T.	2.3.4.5.6	T.	3.4.5.6	T.	6.7.8
231	3	232	8.7	233	235	7.6	236	2.5	237	2	238	6.5.6.4	239	6	240	7	241	3	242	8.7
241	3	242	8.7	243	245	5.6	246	2.4.5	247	2	248	3.5.6.4	249	6	250	7	251	3	252	8.7
251	3	252	8.6.5.6.7	253	255	6	256	2.5	257	2	258	3.4	259	3.5.6	260	7	261	3	262	8.7
261	3	262	7	263	265	5.6	266	2.5	267	2	268	5.6.4	269	6	270	7	271	3	272	8.7
271	2.4.3	272	6.6.7	273	275	4.6	276	6.7.3.4.5	277	3.2	278	4	279	6	280	8.7	281	3	282	8.7
281	3	282	6.5.7	283	285	5.4.6	286	2.3.5	287	2	288	4	289	5.7.6	290	8.7	291	3	292	8.7
291	3	292	8.7	293	295	6	296	7.7.2.6.5	297	2	298	5.6.6.4	299	7.6	300	7	301	3	302	8.6.7
301	3	302	8.6.7	303	305	6	306	2.6.5	307	2	308	5.6.2.6.4	309	6	310	8.6.7	311	3	312	7
311	3	312	7	313	315	5.7.6	316	8.5	317	2	318	6.4	319	8.7.6	320	8.7	321	3	322	6.8.7
321	3	322	6.8.7	323	325	7.5.4.6	326	2.5	327	2	328	6.5.6.4	329	8.6	330	8.7	331	3	332	6.5.8.7
331	3	332	6.5.8.7	333	335	6	336	2.5	337	2	338	5.4	339	6	340	7	341	3	342	8.7
341	2.4.3	342	8.7	343	345	5.6	346	2.5	347	2	348	5.6.4	349	7.6	350	8.7	351	3	352	8.7
351	3	352	8.7	353	355	5.6	356	5	357	2	358	5.6.5.4	359	8.7.6	360	8.7	361	3	362	8.6.7
361	3	362	8.6.7	363	365	6	366	2.4.6.5	367	3.2	368	6.4	369	6	370	7	371	3	372	7
371	3	372	7	373	375	5.6	376	2.5	377	2	378	4	379	6	380	7	381	3	382	6.5.7
381	3	382	6.5.7	383	385	6	386	2.7.6	387	2	388	6.4	389	6	390	7	391	2.4.3	392	6.5.7
391	2.4.3	392	6.5.7	393	395	5.6	396	7.6.5	397	2	398	6.4	399	6	400	7	401	2.3	402	7
401	2.3	402	7	403	405	5.4.6	406	2.4.7.5	407	2	408	5.4	409	7.6	410	8.7	411	3	412	7
411	3	412	7	413	415	5.6	416	5	417	3.2	418	3.4	419	5.6	420	7	421	3	422	7
421	3	422	7	423	425	5.6	426	2.4.5	427	2	428	4	429	7.3.5.6	430	7	431	3	432	7
431	3	432	7	433	435	6	436	2.3.4	437	2	438	3.4	439	8.7.6	440	7	441	2.4.3	442	6.5.7
441	2.4.3	442	6.5.7	443	445	6	446	6.4.5	447	2	448	2.6.4	449	3.4.6	450	7	451	2.4.3	452	7
451	2.4.3	452	7	453	455	6	456	2.6.4.5	457	2	458	6.4	459	7.6	460	7	461	3	462	7
461	3	462	7	463	465	6	466	5	467	2	468	6.4	469	7.6	470	7	471	3	472	8.7
471	3	472	8.7	473	475	6	476	1.7.5	477	2	478	6.4	479	7.6	480	7	481	3	482	8.7
481	3	482	8.7	483	485	6	486	7.4.5	487	2	488	4	489	6	490	7	491	3	492	6.5.7
491	3	492	6.5.7	493	495	7.5.6	496	7.5	497	2	498	6.6.6	499	6	500	7	501	3	502	8.6.6.5.7
501	3	502	8.6.6.5.7	503	505	7.6	506	6.5	507	2	508	5.6.4	509	7.6	510	7	511	3	512	7
511	3	512	7	513	515	6	516	7.6.5	517	2	518	2.4	519	5.7.8.6	520	7	521	3	522	7
521	3	522	7	523	525	7.6	526	5	527	2	528	5.6.4	529	6	530	8.7	531	3	532	7
531	3	532	7	533	535	6	536	5	537	2	538	6.4	539	7.6	540	8.7	541	3	542	8.6.7
541	3	542	8.6.7	543	545	6	546	5	547	2	548	6.4	549	6	550	7	551	3	552	7
551	3	552	7	553	555	6	556	4.5	557	3.2	558	4	559	7.6	560	8.7	561	3	562	7
561	3	562	7	563	565	6	566	2.5	567	2	568	4	569	5.4.6	570	7	571	3	572	8.6.5.7
571	3	572	8.6.5.7	573	575	6	576	5	577	2	578	6.4	579	6	580	7	581	3	582	7
581	3	582	7	583	585	5.6	586	4.5	587	2	588	4	589	5.4.6	590	7	591	3	592	7
591	3	592	7	593	595	5.6	596	5	597	2	598	4	599	8.6	600	7				

TABLE 10—Continued
RESULTS FOR MALE IN PROBLEM 4

T.	S.1 2.3.4	T.	S.2 5.6.7.8.9	T.	S.3 3.4.5.6.7	T.	S.4 7.8.9	T.	S.5 4.5.6.7.8	T.	S.6 1.2.3.4.5	T.	S.7 1.2.3	T.	S.8 2.3.4.5.6	T.	S.9 4.5.6	T.	S.10 6.7.8
1	3	2	8.7	3	6.4.5	4	7.8	5	6	6	2.4.2.5.3	7	2	8	5.4	9	6.5	10	7
11	3	12	7	13	6.5	14	8	15	6	16	2.4.5	17	2	18	4	19	6.5.5	20	8.7
21	3	22	8.7	23	6.5	24	8	25	6	26	2.4.5.3	27	2	28	4	29	6.5	30	7
31	3	32	7	33	6.4.5	34	7.8	35	6	36	2.4.5.2.3	37	2	38	6.4	39	6.5	40	7
41	3	42	8.7	43	6.5	44	8	45	6	46	4.4.5	47	2	48	4	49	4.5	50	7
51	3	52	6.7	53	6.4.5	54	8	55	6	56	2.4.3	57	2	58	4	59	5	60	6.7
61	3	62	7	63	4.5	64	7.8	65	6	66	4.5.3	67	2	68	4	69	5	70	7
71	3	72	7	73	4.5	74	8	75	4.5.6	76	4.3	77	2	78	4	79	4.5	80	6.7
81	2.4.3	82	6.5.6.7	83	4.5	84	7.8	85	6	86	2.4.3	87	2	88	4	89	6.5	90	7
91	2.4.3	92	7	93	4.5	94	7.8	95	4.5.6	96	4.3	97	2	98	4	99	4.5	100	7
101	2.4.3	102	6.5.7	103	5	104	8	105	7.6	106	4.5.3	107	2	108	4	109	5	110	8.7
111	3	112	7	113	5	114	8	115	6	116	2.3	117	2	118	6.4	119	5	120	8.7
121	3	122	8.7	123	6.4.5	124	8	125	6	126	4.2.3	127	2	128	5.6.4	129	6.5	130	7
131	3	132	7	133	5	134	8	135	6	136	4.2.3	137	2	138	3.4	139	5	140	7
141	3	142	7	143	5	144	8	145	6	146	4.2.3	147	2	148	4	149	5	150	7
151	3	152	8.7	153	4.5	154	8	155	4.5.6	156	4.5.3	157	2	158	4	159	5	160	7
161	3	162	7	163	5	164	8	165	6	166	2.4.3	167	2	168	4	169	5	170	7
171	3	172	7	173	5	174	8	175	6	176	2.4.3	177	2	178	4	179	5	180	7
181	3	182	7	183	6.4.5	184	8	185	6	186	2.3	187	2	188	4	189	6.5	190	7
191	3	192	7	193	6.5	194	7.8	195	5.6	196	3	197	2	198	6.5.6.4.3	199	5	200	7
201	3	202	7	203	5	204	8	205	6	206	2.3	207	2	208	4	209	6.5	210	7

TABLE 11
DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

Female							Male						
PROBLEM 4													
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug. 4	1-6	0	5				Aug. 4	1-6	0	5			
"	11-11	4	6	4	16	1:4.00	"	11-11	3	7	4	16	1:4.00
5	21-31	2	8	5	15	1:3.00	5	21-31	2	8	4	16	1:4.00
6	41-51	3	7	7	13	1:1.86	6	41-51	4	6	8	12	1:1.50
"	61-71	4	6	6	14	1:2.33	"	61-71	3	7	5	15	1:3.00
7	81-91	4	6				7	81-91	4	6			
"	101-111	2	8	8	22	1:2.75	"	101-111	5	5	14	16	1:1.14
9	121-131	3	7				9	121-131	3	7			
"	141-151	4	6	15	15	1:1	"	141-151	4	6	10	20	1:2.00
10	161-171	6	4	13	17	1:1.31	10	161-171	3	7	10	20	1:2.00
"	181-191	5	5				"	181-191	6	4	15	15	1:1
11	201-211	5	5	12	18	1:1.50	11	201-211	5	5			
"	221-231	3	7	13	17	1:1.31	"	221-231	4	6	12	18	1:1.50
12	241-251	8	2				12	241-251	5	5			
"	261-271	3	7	15	15	1:1	"	261-271	5	5	14	16	1:1.14
13	281-291	4	6	18	12	1: .67	13	281-291	4	6	12	18	1:1.50
"	301-311	6	4				"	301-311	5	5			
14	321-331	3	7	9	21	1:2.33	14	321-331	5	5	13	17	1:1.31
"	341-351	3	7				"	341-351	3	7			
15	361-371	4	6	13	17	1:1.31	15	361-371	6	4	11	19	1:1.73
"	381-391	4	6				"	381-391	2	8			
16	401-411	4	6	12	18	1:1.50	16	401-411	3	7	15	15	1:1
17		4	6				17		5	5			
8		5	5				18		7	3			
"		5	5				"		3	7			
"		7	3	17	13	1: .76	"		4	6	12	18	1:1.50
"		3	7				19		6	4			

TABLE 11—*Continued*
DAILY SERIES AND AVERAGES WITH RATIOS OF CORRECT TO INCORRECT
FIRST CHOICES

Female							Male						
PROBLEM 4													
Date	No. of trials	R	W	R	W	Ratio of R to W	Date	No. of trials	R	W	R	W	Ratio of R to W
Aug.	421-	7	3				Aug.	421-	6	4			
"	431-	4	6	17	13	1: .76	"	431-	5	5	17	13	1: .76
20	441-	6	4				20	441-	4	6			
"	451-	2	8				"	451-	4	6			
"	461-	7	3	15	15	1:1	"	461-	7	3	15	15	1:1
21	471-	6	4				21	471-	5	5			
"	481-	6	4	12	8	1: .67	"	481-	7	3	12	8	1: .67
22	491-	7	3				22	491-	5	5			
"	501-	4	6				"	501-	4	6			
"	511-	4	6	15	15	1:1	"	511-	7	3	16	14	1: .88
23	521-	6	4				23	521-	6	4			
"	531-	7	3				"	531-	6	4			
"	541-	5	5	18	12	1: .67	"	541-	7	3	19	11	1: .58
24	551-	4	6				24	551-	5	5			
"	561-	6	4				"	561-	7	3			
"	571-	7	3	17	13	1: .76	"	571-	8	2	20	10	1: .50
25	581-	5	5				25	581-	4	6			
"	591-	4	6	9	11	1:1.22	"	591-	7	3	11	9	1: .82
25	1-	7	3	7	3	1: .43	25	1-	4	6	4	6	1:1.50
26	11-	7	3				26	11-	6	4			
"	21-	7	3				"	21-	6	4			
"	31-	6	4	20	10	1: .50	"	31-	5	5	17	13	1: .76
27	41-	6	4				27	41-	6	4			
"	51-	5	5				"	51-	6	4			
"	61-	5	5	16	14	1: .88	"	61-	7	3	19	11	1: .58
28	71-	5	5	5	5	1:1	28	71-	5	5	5	5	1:1
30	81-	5	5				30	81-	4	6			
"	91-	7	3				"	91-	4	6			
"	101-	5	5	17	13	1: .76	"	101-	5	5	13	17	1:1.31
31	111-	6	4				31	111-	7	3			
"	121-	7	3				"	121-	5	5			
"	131-	5	5	18	12	1: .67	"	131-	8	2	20	10	1: .50
Sept.							Sept.						
1	141	5-	5				1	141-	9	1			
"	151	5-	5				"	151-	6	4			
"	161	5-	5	15	15	1:1	"	161-	9	1	24	6	1: .25
2	171	9-	1				2	171-	9	1			
"	181	6-	4				"	181-	7	3			
"	191	7-	3	22	8	1: .36	"	191-	6	4	22	8	1: .36
3	201	5-	5	5	5	1:1	3	201-	8	2	8	2	1: .25

After six hundred trials had been given to each individual by use of the series of settings presented on page 192, under problem 4, it was apparent that the animals could succeed in solving the problem only by acquiring a definite habit for each particular setting, and it was further evident that the settings including seven and nine open doors were extremely difficult for the animals. For these reasons it was decided to present a modified series of settings in which the groups should consist of either three or five open doors. Two hundred trials were given with the new series of settings, and the settings themselves, as well as the results obtained, appear at the bottom of tables 9 and 10.

Two important conclusions are justified by these results. First, that the pigs, in so far as they had succeeded in responding correctly to the middle door, had reacted to particular settings. And second, that with sufficiently prolonged training they could perfectly solve the problem of the middle member of a series, if the total number in a group of open doors did not exceed five. As a matter of fact, no series of ten correct choices was obtained with either individual because of the surprisingly strong and persistent influence of the original settings.

Let us consider, for example, setting 3. This originally consisted of the group 1.2.3.4.5.6.7, in which no. 4 was the box to be entered. In the modified settings, this group was changed to 3.4.5.6.7, consequently, the box to be entered was 5 instead of 4. Now, whereas in the case of setting 1 which remained unchanged, the female made only one mistake in twenty-one trials subsequent to the modification of the settings, in the case of setting 3 she chose wrongly in all except three of the twenty-one trials, and this in spite of the fact that in the case of settings 2 and 5, both of which involved five open doors, she chose correctly sixteen times out of twenty-one. Similarly in the case of setting 6, in which originally all nine of the doors were open, whereas in the modification only doors 1, 2, 3, 4 and 5 were used, both the female and the male chose correctly only once in twenty-one trials.

Although the above conclusions are of primary importance, further examination of the data of tables 9 and 10 should throw additional light on the reactive capacity of our subjects.

We shall consider the materials according to the number of

mechanisms used in the settings. Settings 1, 4, 7 and 10 involve three members, setting 2, 5 and 8, five members; settings 3 and 9, seven members; and setting 6, nine members. Below are presented the number of correct first choices made by each individual in connection with each setting, the total number of choices being sixty.

Correct First Choices *in Sixty* for Each Setting in Problem 4

	S.1	S.2	S.3	S.4	S.5	S.6	S.7	S.8	S.9	S.10
Female.....	35	22	16	45	22	10	49	11	22	38
Male.....	41	21	4	48	27	12	52	11	19	34

These figures prove that to select the middle member of a group of three is fairly easy for the pig. This, to be sure, might be gathered from the fact that the animal can solve the problem of the second from the left. It further appears that attempts to locate the proper box when it was the middle of a series of five resulted in a gradual reduction in the number of incorrect choices, but never yielded success. The selection of the middle member of a group of seven or of nine is clearly still more difficult, and there is no reason to suppose that with less than thousands of trials the subjects in question would have learned to enter it directly.

It is practically certain that the series of settings rather than the number of members in a group is responsible for the animal's confusion. Doubtless by training a pig to react correctly to each setting and by then presenting the several settings in a certain definite order, a habit could be built up which would apparently yield a perfect solution of problem 4. It is, however, needless to point out that this would not be the kind of solution that has been obtained for problems 1, 2 and 3, or in other words, would not be dependent upon response to the general relation middleness.

Analysis of the records for the sixty trials under setting 6 are of special interest, since this setting proved the most baffling of all to the subjects.

To begin with, they naturally tried the end members of the series. This proving unsatisfactory, they next tended to choose rather at random, and then there gradually appeared a tendency to enter first box 2 and to proceed thence either directly

by way of 3, 4 and sometimes also 6, to the middle box, number 5. This tendency to select, when in doubt, a box second from the right end of the series may possibly be due in part to the fact that the box to be chosen in setting 7 was number 2. At any rate, the frequency with which the female throughout her training chose box 2 first of all under setting 6 is surprisingly high, whereas for the male, this frequency while rather high early in the course of the training, tended to diminish and to give place to the decidedly profitable tendency to choose a box near the middle of the series, 6, 7 and 5 frequently being entered.

Similarly, we might, if space permitted, analyse in detail the results for the other settings. We have chosen to use our space in this report for the presentation of data in tabular form rather than for their description, because we are convinced that the facts are more important than early attempts at interpretation.

SUMMARY

1. The pig has proved itself an ideal subject for studies in adaptive behavior.

2. The new multiple choice method, by means of which standardized problems ranging in difficultness from the very easy to the very difficult may be presented to widely differing types of organism, has in our opinion fully justified our expectations, for it has proved admirably suited to the discovery and analysis of increasingly complex types of behavior.

3. For the purpose of discovering the extent to which ideational and closely allied types of behavior exist in the pig, four problems were presented. They may be defined simply in terms of the constant relation of the right mechanism, as (1) the first at the right end of the series; (2) the second from the left end of the series; (3) alternately, the first at the left and the first at the right; (4) the middle member of the series.

The purpose of the experiments was to discover the pig's reactive tendencies and especially its degree of ability to dissociate the essential and constant relation of the right mechanism from its accidental and variable accompaniments.

4. The two subjects solved perfectly the first problem with less than fifty experiences. The indications are that visual and kinaesthetic guidance sufficed.

The second problem was solved more slowly, partly because the influence of the earlier training had to be overcome, but

also because this is a much more difficult problem than the first one. In this also, visual and kinaesthetic guidance seems to account for success, but the extent to which the animals learned to respond to the relation of secondness from the left, no matter what the other relations of the mechanism, was a surprise to the experimenters and is important in connection with the problem of ideation in animals.

The third problem also was solved with reasonable ease, and the animals demonstrated their ability to acquire the habit of alternation without respect to particular groups of reaction-mechanisms.

Problem 4 proved too difficult for the pigs. They learned to select the middle mechanism of the series when the groups were small, but when seven or nine mechanisms were in use, they were confused. The indications are that with long training they would learn to react to the particular settings correctly, although incapable of reacting to the constant relation of middleliness.

5. Our results indicate for the pig an approach to free ideas which we had not anticipated. There seems no reason to doubt that visual and kinaesthetic factors in the main determine their responses, but it is evident that they are not so dependent upon the particular situation as are many other mammals. While hesitating to claim that we have demonstrated the presence of ideas, we are convinced that the pig closely approaches, if he does not actually attain, to simple ideational behavior.

6. The multiple choice method has revealed a number of interesting reactive tendencies, their relations to one another, and the varied ways in which they are manifested in connection with situations which are rather difficult to meet.

7. Finally, we would again call attention to the fact that this method of studying behavior should enable us, when it has been reasonably perfected and its problems standardized, to determine the level of mental development in different individuals, species, stages of growth, and conditions of normality, and to compare the reactive tendencies, whether or not ideational, of other organisms with those of the human subject. Our results thus far fully convince us that the method may be made to yield most valuable psychological and behavioristic information. The method is applicable to the study of ideational behavior in many other animals.

A GRAPHIC METHOD OF RECORDING MAZE-REACTIONS

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From the Harvard Psychological Laboratory

One figure

For nearly twenty years the maze or labyrinth has been employed by students of animal behavior. Both apparatus and procedure have been improved steadily during the last decade, but even to-day we lack an intelligently standardized form of maze and a reliable method of recording the several important aspects of the subject's reaction.

We propose, in this paper, to describe a method of recording maze-reactions which should greatly increase the value of the results obtained in experiments with the maze. We shall not attempt to describe a type of maze which promises to be worthy of standardization, but instead shall limit ourselves to a brief discussion of methods of observation.

Experiments with the maze offer opportunities for the measuring of several aspects of reaction. Especially important among the data obtainable are (1) time of reaction; (2) distance; (3) number of errors; and (4) nature and distribution of errors. Prior to the devising of the method herein described, it has been practically impossible for even the highly practiced observer to obtain accurate measurements of all of these features of reaction. Indeed, with a rapidly moving subject like a rat or a mouse, it has been impossible, during the first few trials, to obtain with accuracy any other measurement than that of time. This is obviously quite as unnecessary as it is unfortunate, for we have good reason to believe that distance and error data are in many experiments more important than time data. Because of our conviction that a variety of data should be obtained in every maze experiment and that all measurements should be made with a reasonable degree of facility and accuracy, we have made it our business to attempt to devise a method which shall enable an experimenter to record the various aspects of reaction directly and graphically.

When the idea of using a graphic method of recording maze-reactions occurred to us, it was immediately suggested, through correspondence, to Professors J. B. Watson and Madison Bentley. The former, feeling the immediate need of such an improvement in the technique of maze experiments, promptly devised and con-

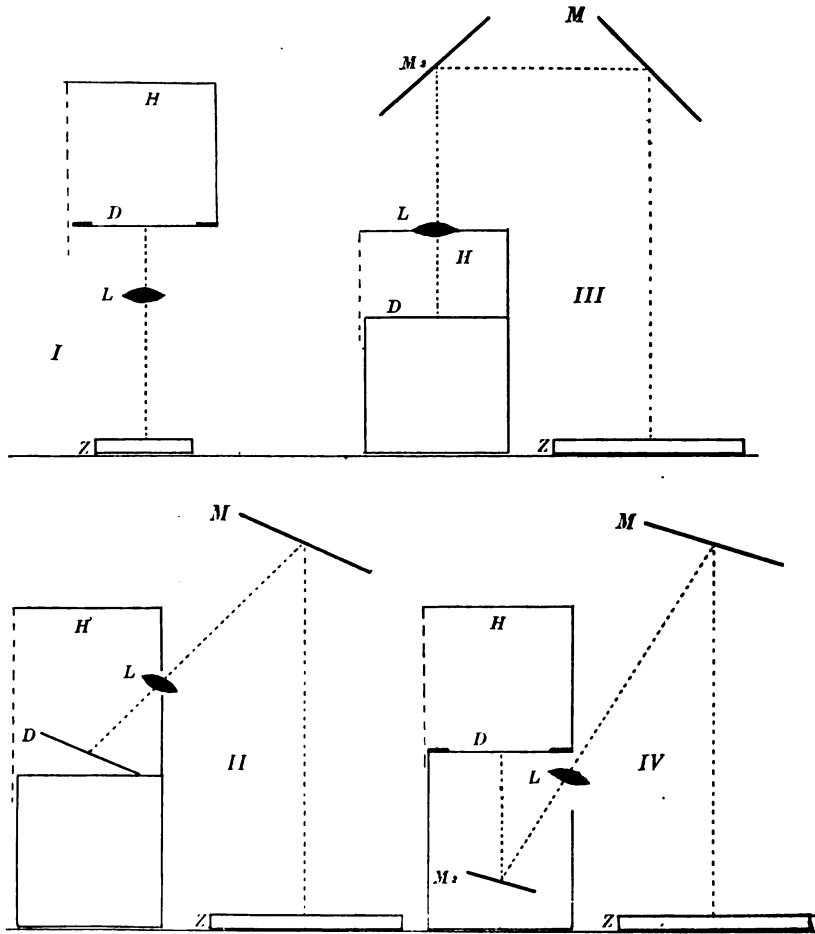


FIGURE 1

Diagrams of four types of apparatus for obtaining graphic records of maze-reactions. I. Apparatus for the direct method: Z, maze; L, lens; D, drawing surface; H, hood. II. Apparatus for simple reflection method: M, mirror. III. Apparatus for double reflection method of Watson: M and M₁, mirrors. IV. Apparatus for double reflection method of Kellogg.

structed what may be termed the camera lucida apparatus. This has been in use for several months in the Psychological laboratory of the Johns Hopkins University and is reported by Professor Watson to work satisfactorily.

In our search for simple, inexpensive, reasonably convenient and adaptable means of obtaining the desired data of reaction, we have happened upon the four methods or devices which are now to be described. Figure 1 is a diagrammatic representation of these several devices. Since they are not of precisely the same value, we shall point out the chief merits of each in describing them.

It was our aim to project, in some convenient manner, an image of the maze and of the reacting subject upon a record sheet which should bear a diagram of the maze. Upon this record sheet the experimenter may accurately trace the path of the animal, while, at the same time, keeping a record of the time of reaction. From the graphic record of the route taken by the animal, the distance and error data may be read.

We shall designate the four methods as the direct method (Fig. 1, I); the simple reflection method (Fig. 1, II); the double reflection method of Watson (Fig. 1, III); and the double reflection method of Kellogg (Fig. 1, IV).

I. *The direct method.* This is the simplest device which we have been able to imagine. Above the maze, Z, of Fig. 1, I, is placed either a simple or a compound lens, L, and at the proper distance above it, a plate of glass, D, conveniently framed in a drawing table, and enclosed by a hood, H. Upon this plate of glass, a thin sheet of paper bearing a plan of the maze is placed. As the observer looks down upon the record sheet, he sees an image of the maze and of the reacting subject, and at will he may trace with pen or pencil upon the record sheet the path followed by the subject.

This method has the important advantages of being extremely simple, inexpensive, and adaptable. It gives a reversed image of the maze, but this is no considerable disadvantage. The chief disadvantages of the method are its inconvenience in connection with large mazes because of the great distance necessary between the maze and drawing board. But even with very large mazes, this method may be used to advantage if a vertical distance of at least thirty feet is available. This arrangement

is likely to prove most practicable where two rooms, the one above the other, are available for maze experiments. The writers have tested the method only with very small mazes.

II. *The single reflection method.* The device for this method, as shown in Figure 1. II, consists of a mirror, M, which is placed above the maze, Z, and which causes an image of the maze to fall upon the lens, L. This image is focused upon a record sheet at D. As in the case of the direct method, the drawing board is hooded in order that the experimenter may work in dim light and thus be able to see, clearly, both the alleys of the maze and the moving animal. In comparison with the former method, this is somewhat more expensive. It yields a completely reversed image and it may be used for even very large mazes. Its chief defects are the inconvenient inclination of the drawing surface, at one end of which the observer must work. In this laboratory we have thoroughly tested the method and find it to work satisfactorily. A little practice enables the observer to follow a rapidly moving rat or mouse and to obtain records which yield accurate measurements of distance, time and errors, even in the early experiments with a given subject.

III. *The double reflection method of Watson 'camera lucida method'.* Two mirrors are used in this apparatus together with a lens and drawing board. The arrangement of these parts is shown in Fig. 1. III. This apparatus has the disadvantage of being more expensive by reason of an additional mirror than the preceding method, and it is also placed at a slight disadvantage because it supplies an image of the maze which is reversed from right to left. To counterbalance these disadvantages, we may mention the following obvious advantages: (1) the more convenient position of the drawing surface; (2) the removal of the experimenter to a considerable distance from the maze; and (3) the adaptability of the apparatus to spatial demands in room or laboratory.

A more detailed account of this method is given by Professor Watson on p. 58.

IV. *The triple reflection method of Kellogg.* This differs from method III in that M₁ is placed below the lens, and the image falls up on the record sheet from below, as in method I. Disturbing shadows cast by the hand of the experimenter are thus avoided. The image obtained by this method is completely

reversed and the apparatus, as a whole, is quite as adaptable as is Professor Watson's.

General discussion of methods. The above devices for obtaining graphic records of maze-reactions yield less satisfactory results than would a good photographic device, and we recommend them simply because they are less expensive in construction and operation. All are so simple that detailed description is needless. We shall, however, in concluding this article, call attention to certain important points which experience in the use of the graphic method has brought to our attention.

In the first place, although it is perfectly possible to get along with a simple lens, especially if one is working with small mazes, a much more satisfactory image may be obtained by the use of a compound lens. Second-hand portrait lenses are available and wholly suitable, but even such a lens is likely to be much more expensive than a simple lens.

Each of the four devices which we have described has its obvious advantages and disadvantages, and it is clear that choice of a method should depend upon the relative importance of these in each particular case. On the whole, it is likely to be more convenient for most experimenters to have their drawing board slightly inclined toward them. This is possible in methods III and IV. Method II necessitates the use of an inclined drawing board, but unfortunately the observer must sit at one end of this board and work in a somewhat awkward position. So far as the position of the drawing surface is concerned, methods III and IV would seem slightly more satisfactory than methods I and II.

In those devices in which the light falls upon the record sheet from above, the shadows cast by the experimenter's hand and pencil are disturbing, sometimes rendering it difficult to follow accurately a swiftly moving animal. Other things being equal, it is therefore preferable to have the light come from beneath the drawing surface, as in methods I and IV.

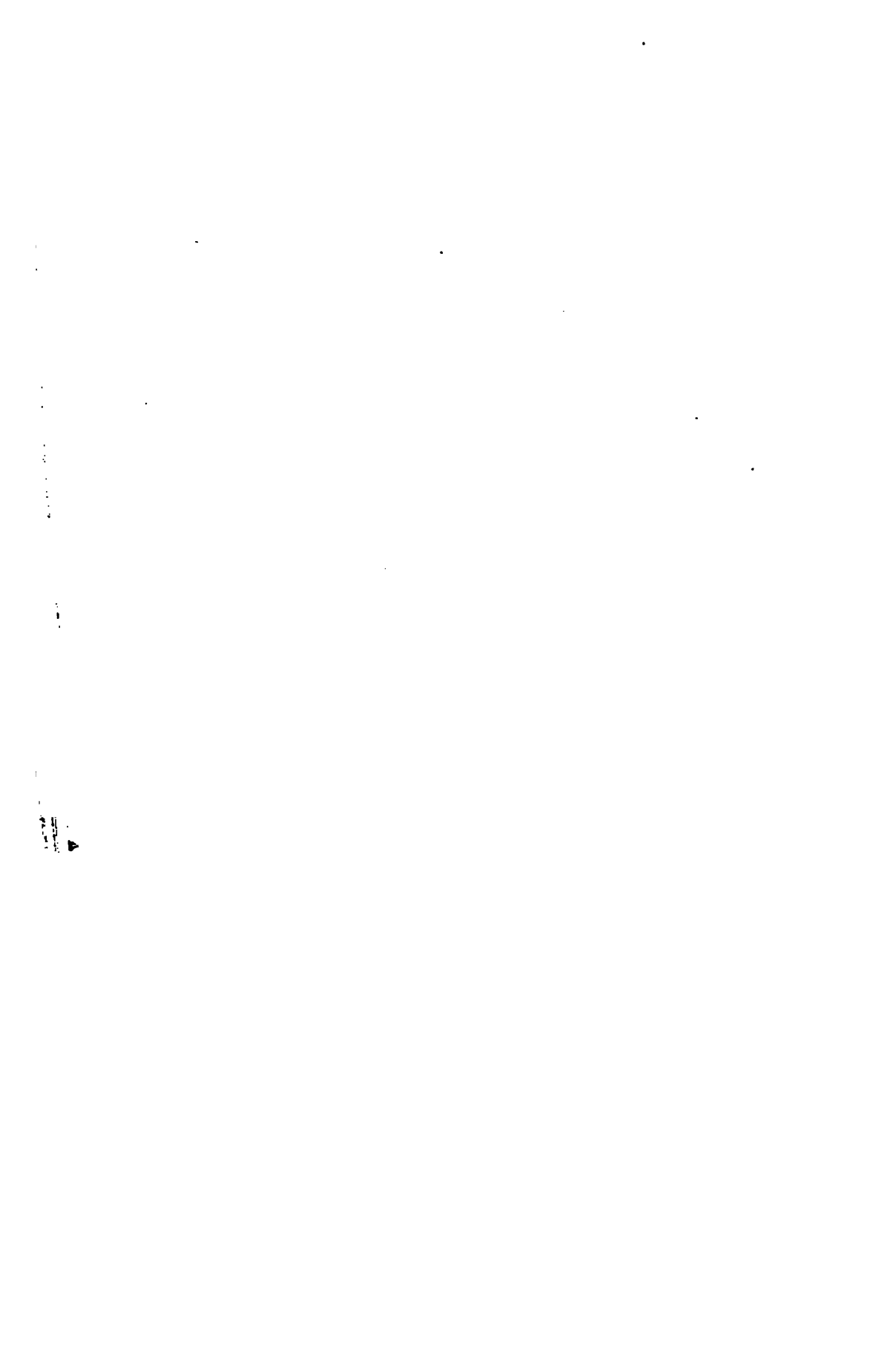
In the first few trials with a given animal, it is extremely important for the experimenter to be able to change record sheets quickly, since the animal is likely to traverse the alleys of the maze rapidly and repeatedly. If the image comes from above the record sheet, it is possible to have the sheets made up in the form of a tablet or block with two edges glued. The

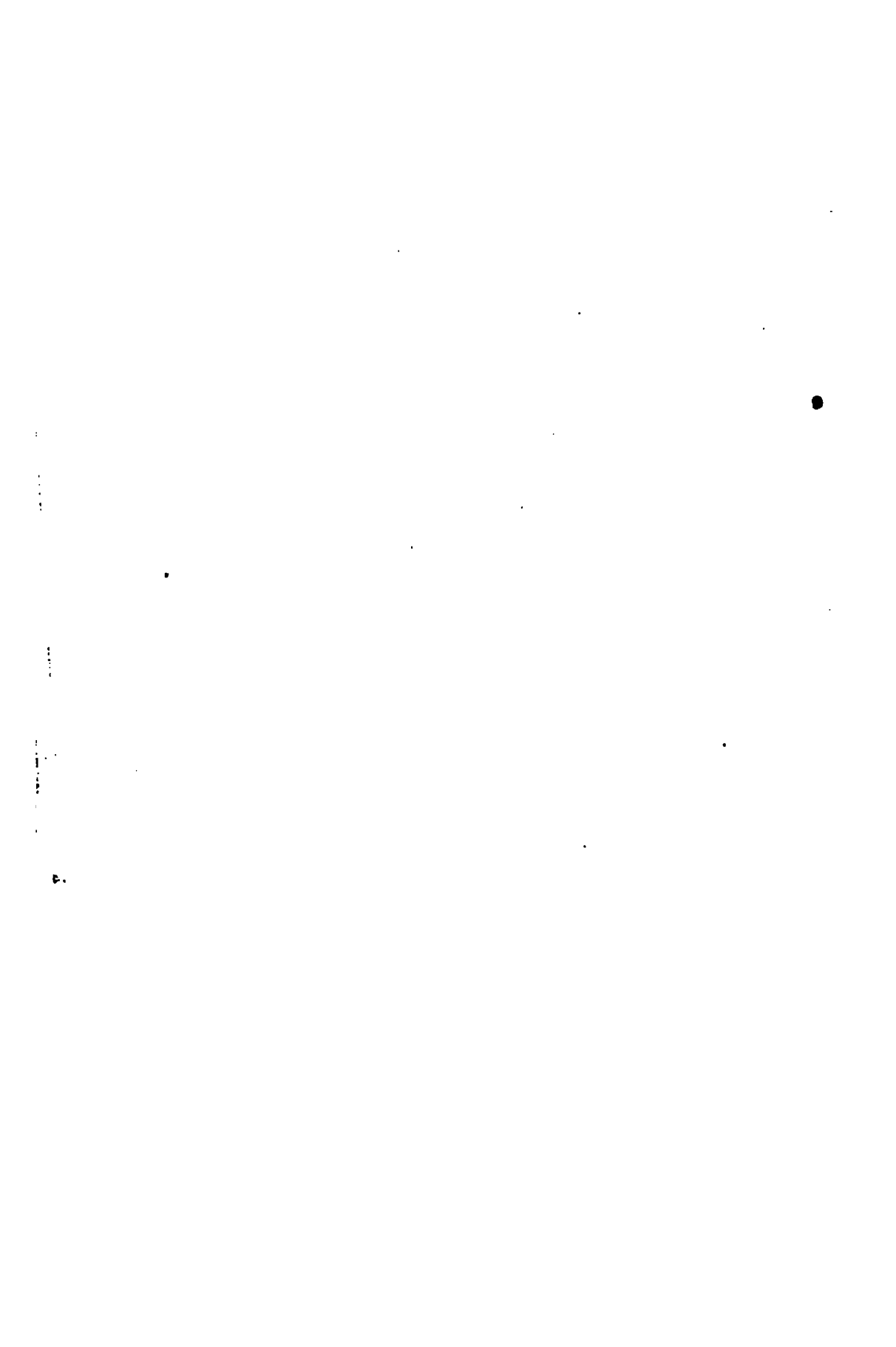
tablet having been properly oriented, the experimenter may at any moment tear off a record sheet and continue his tracing almost uninterruptedly. This method may be made to work satisfactorily even when a printed diagram of the maze appears on each record sheet, for the orientation of the block may be kept constant. When the image falls upon the record sheet from below it is necessary to use rather thin paper and to have the drawing board so arranged that the sheets fit neatly and may be quickly placed in position. Although we have tried only methods I and II in this laboratory, we are inclined to believe that it is more satisfactory on the whole to have the image come from below the record sheet. Especially in the first trials with a given animal, the time required is likely to be long, and the experimenter should be able to make his observations without undue discomfort or fatigue.

We recommend that as soon as the experimental device has been selected and properly adjusted, a zinc etching, which exactly reproduces the image of the maze as it falls upon the drawing board, be made, and that this be used in the preparation of blank record sheets. For, although a diagram of the maze is not absolutely essential, it has considerable value in connection with the early trials and sometimes prevents errors in the reading of records of later trials. It is extremely laborious and wasteful of time to draw the diagrams by hand, and if hundreds or thousands of record sheets are to be used, the cost of a zinc etching and of printing the sheets will be trivial in comparison with the value of the experimenter's time.

As appears from the above discussion, we are not in a position to recommend any one of the four methods over the others without careful consideration of the type of maze which is to be used, of the character of the lens, and the location of the apparatus. But we are fully convinced that all investigations with the maze should make use of some graphic method of recording reactions. There can be no doubt that the data previously obtained from maze experiments have only a fraction of the value which they should have, and that because of the inaccuracy and incompleteness of the records.









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